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Volume 2 May 5- 8, 1959
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FALLOUT FROM NUCLEAR WEAPONS TESTS

HEARINGS

BEFORE THE

SPECIAL SUBCOMMITTEE ON RADIATION

OF THE

JOINT COMMITTEE ON ATOMIC ENERGY

CONGRESS OF THE UNITED STATES

EIGHTY-SIXTH CONGRESS

FIRST SESSION

ON

FALLOUT FROM NUCLEAR WEAPONS TESTS

MAY 5, 6, 7, AND 8, 1959

VOLUME 2

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NOTE

This is volume 2 of the public hearings held May 5–8, 1959, on fallout from nuclear weapons tests. It covers summary of new data on uptake in milk, food, and human bone; fallout mechanism, uptake; developments in radiation biology; discussion of permissible exposure levels; status and implication of testing; discussion of surveillance; and summary of problems and needs, including level of support. Volume 1, starting with a general review of developments since 1957, covers also a summary of new data on atmospheric fallout; and global fallout. Volume 3 is an appendix to the hearings containing additional materials pertinent to discussions. These materials cover supplemental statements on general aspects of fallout problem; the "hot spot" problem and strontium 90 in foods; AEC quarterly reports; comments on General Advisory Committee report; maximum permissible levels; biological effects; carbon 14; report of the United Nations Scientific Committee on the effects of atomic radiation—1958; nuclear detonations and meteorological aspects; classification and declassification; fallout research and organization; and bibliography. It is planned at a later date to publish a detailed index of the hearings. The index would appear as volume 4.

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FALLOUT FROM NUCLEAR WEAPONS TESTS

WEDNESDAY, MAY 6, 1959—Continued

CONGRESS OF THE UNITED STATES,
SPECIAL SUBCOMMITTEE ON RADIATION,
JOINT COMMITTEE ON ATOMIC ENERGY,
Washington, D.C.

Representative HOLIFIELD. Our next subject is "Summary of New Data on Uptake in Milk, Food, and Human Bone." Proceed please, Dr. Straub.

STATEMENT OF CONRAD F. STRAUB,[1] PH. D., TAFT SANITARY ENGINEERING CENTER, PUBLIC HEALTH SERVICE, CINCINNATI, OHIO

Dr. STRAUB. Mr. Chairman and members of the committee, it is certainly a pleasure to be here to review with you the work we have done at our Cincinnati station of the Public Health Service.

In his statement, Dr. Francis J. Weber, Chief, Division of Radiological Health, Public Health Service, referred to the radiation surveillance networks operated at the Robert A. Taft Sanitary Engineering Center, Public Health Service, Cincinnati, Ohio. It has been suggested that his discussion be expanded in order to indicate our findings relative to activity levels from fallout in environmental media.

(Besides the data contained in this statement, additional PHS air, water, and milk data appear starting on p. 197 of vol. 1, see also vol. 3, app. B.)

As pointed out by Dr. Weber, the national air sampling and the basic data water quality networks have been established to measure a variety of organic and inorganic pollutants in air and water, respectively. Radioactivity is but one of the contaminants measured in these samples and the only contaminant of interest to the Joint Committee at this time. However, collateral information on other contaminants may be of considerable value when a more complete understanding exists of the synergistic and antagonistic effects of all sources of environmental contamination upon the disease pattern of the Nation.

The radiation surveillance activities of the Public Health Service have been developed to assess the levels of radioactivity in a wide variety of environmental media and to delineate and define the spe-

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cific radionuclides responsible for such contamination. Initially, our major effort was in the development of suitable techniques for measuring the low levels of radioactivity encountered in surface waters. (See attachments I, II, and III, pp. 1005, 1039, and 1045.) Later, studies were initiated to measure the gross activity in rainfall, vegetation, biological samples, soils, and so forth.

As environmental radioactivity levels increased, it became necessary to define the specific radionuclides responsible for the increased activity. Such analytical information is necessary to an estimation of the radiation dose resulting from the ingestion and inhalation of these materials.

In table 1 (see fig. 1, p. 965), I show the actual stations that are being operated by the various programs at Cincinnati in terms of identifying the specific activity associated with air, water and/or milk sampling programs. The green dots represent the air activity stations. You can see that we have quite a coverage. There are at the present time 114 urban stations and 51 nonurban stations. These collect a sample once every 2 weeks and this is submitted to us for measurement of gross activity. The triangular stations that you see there represent 51 stations on our surface streams which serve as sources of water supply for the larger cities of the Nation.

The 10 red irregularly shaped areas define the 10 milksheds that are currently being evaluated by the Public Health Service.

Representative HOLIFIELD. You have a total of 10 milksheds?

Dr. STRAUB. Yes. The actual location of these are defined in the formal statement. If you would like me to mention these, I will be very happy to do so, sir.

The first slide that I have represents the levels of air activity that we have found during the period 1953 to 1957. We have found that there has been essentially a fiftyfold increase in these levels based upon the national total of all samples identified. For comparison we have indicated the data for Cincinnati which shows during the same period of time a twentyfold increase.

(See table 1 in prepared statement, p. 967.)

The national radioactivity values range from a low of less than 0.1 to a high of 5,435 micromicrocuries per cubic meter (Salt Lake City, 1957), whereas the Cincinnati values ranged from less than 0.1 to 21.9 micromicrocuries per cubic meter. These data were summarized from the report "Air Pollution Measurements of the National Air Sampling Network, 1953–57" released by the Public Health Service in 1958.

The surface water analyses have indicated a gross total beta gamma activity for all surface waters that we sampled with the exception of Columbia River of 56 micromicrocuries per liter, of which about 50 percent of the total activity is associated with the suspended material. The reason we carry out this separation is that water-treatment processes will be effective in removing essentially all the activity associated with the suspended matter.

So you can say, where you have an approved water-treatment process, including chemical treatment, sedimentation, and filtration, about half of the total activity will be removed by such treatment.

Representative HOLIFIELD. Will you also comment on the removal of this material from milk?

Dr. STRAUB. In what terms?

Representative HOLIFIELD. You just say as I understand you that you can take most of this material out of water?

Dr. STRAUB. About half.

Representative HOLIFIELD. About half of the radioactive material out of water?

Dr. STRAUB. Yes, sir.

Representative HOLIFIELD. What is the comparison to milk?

Dr. STRAUB. We have done no work on this ourselves. Other people have done this and they can remove the strontium 90 and reconstitute the milk. I am sure the committee is familiar with this. There are new techniques suggested now by others to go ahead and remove strontium 90 from milk.

Senator GORE. How is it separated from calcium?

Dr. STRAUB. It can be by various techniques. One is a modified ion exchange technique and the other is through a high nitric acid technique. We can get separation of the strontium from the calcium fractions. In milk, the ion exchange technique could be used to remove both calcium and strontium. The basic problem with which we are faced is what effect does it have on the nutritional and other properties of the milk. Also, costwise, such processing would put it way out of the reach of people.

Senator GORE. In other words, when you take the strontium 90 out of it you may leave only water?

Dr. STRAUB. Essentially so.

Senator GORE. Instead of a nutritional food?

Dr. STRAUB. You perhaps have to put back the calcium in order to replace it if it is still felt that calcium is necessary. The decalcified milk, however, would still contain protein, carbohydrate, and fat values.

Senator GORE. Then you might as well take water in the first place and add calcium to it?

Dr. STRAUB. Water also contains strontium 90 as we will point out in a few moments.

Senator GORE. It is a rather expensive process to take the food value out of milk, reduce it to water, and add the food value back to the water?

Dr. STRAUB. This is so. It is a very expensive process. We are wondering what will happen from this if they do have to resort to some such measures for handling milk.

Senator GORE. You think that the reasonable rule for economic competition is for someone to take water in the first place?

Dr. STRAUB. This may be possible, yes.

The strontium-90 levels in these same surface water supplies, and to date we have only had opportunity to analyze 20 composite samples representing 3-month accumulations from these areas, have shown an average of 1 micromicrocurie per liter of strontium 90 with a range of 0.3 to 4.7. These values are included in the text and are compared with values that have been reported by personnel of the AEC and values reported by the British in the past.

From this it will be seen that there has been an upward trend in strontium 90 in these waters as one would expect. The strontium-90 levels in water are of interest from two points of view. The first is concerned with the use of such water as a potable supply for humans

and animals, particular where ponds, lakes, or small reservoirs may be used for such purposes as contrasted with river supplies which, because of their flow, show lower levels of this particular radionuclide.

The second has to do with the use of these waters in the preparation of food materials, such as soups, beverages, reconstitution of milk, preparation of baby formulas, or wherever water is used as an ingredient in food preparation. I would like to refer to two of these slides that we have here. These show essentially the levels of activity that have been indicated for various watersheds as shown.

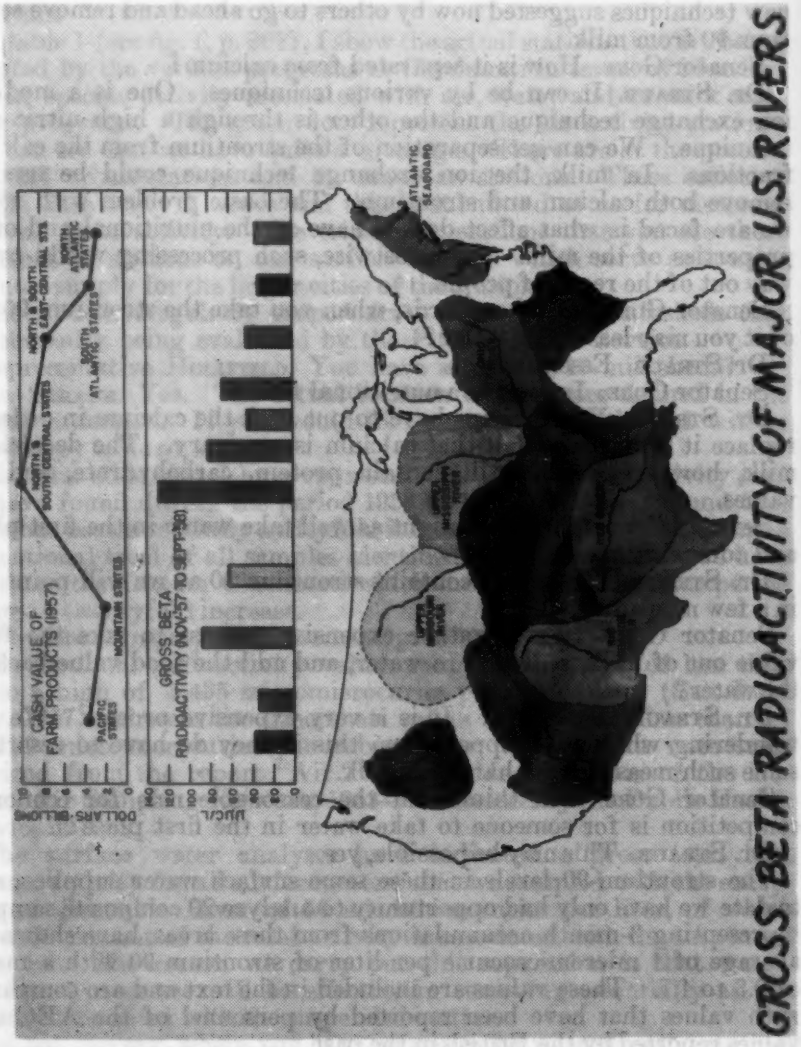
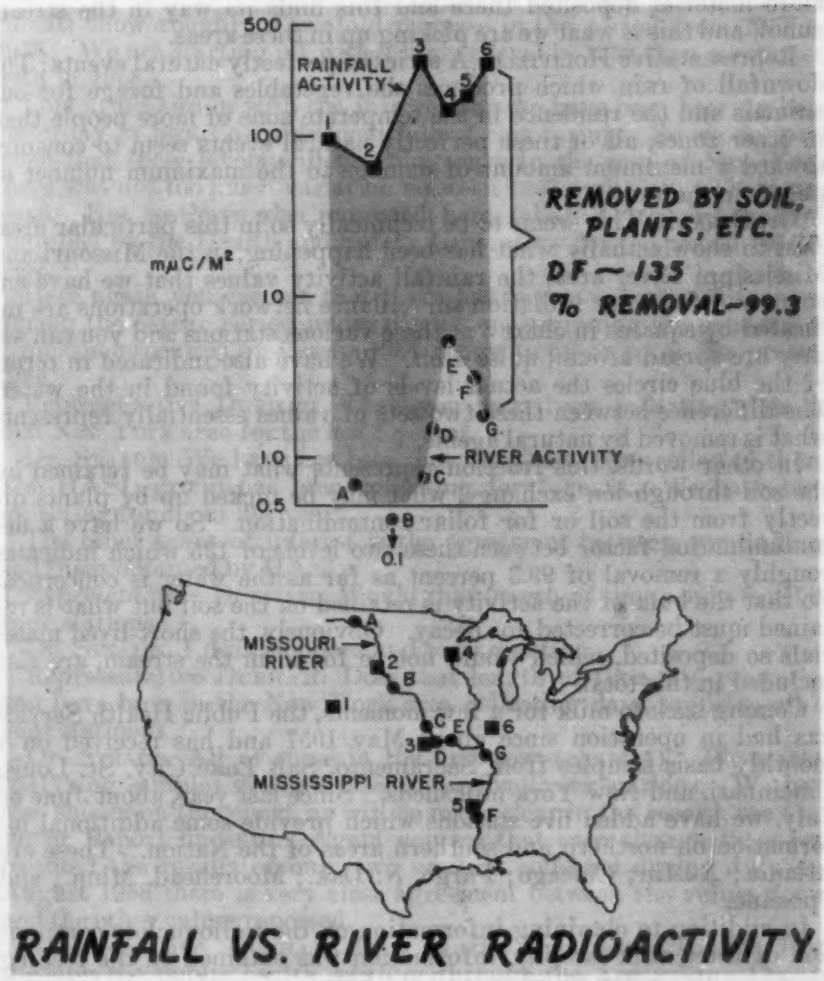


CHART 3



RAINFALL VS. RIVER RADIOACTIVITY

In this middle diagram (chart 2) we have indicated the actual levels of gross activity in micromicrocuries per liter. We have not shown the Columbia River in this analysis because activity is discharged from the Hanford installation and much higher levels of specific materials are discharged.

It will be seen here that although the national average has been 56 the lower Missouri River Basin has about the highest level, about 130 micromicrocuries per liter. It is also of interest to see that this area where we have the highest levels is also the area in which most of our food materials are produced in terms of our gross farm product for 1957.

Representative HOLIFIELD. Does that refer to air or water?

Dr. STRAUB. This is in terms of water. These are the actual levels found in water.

Representative HOLIFIELD. And the levels are higher in the food producing areas?

Dr. STRAUB. This is what we seem to find. Essentially there is more material deposited there and this finds its way in the stream runoff and this is what we are picking up in these areas.

Representative HOLIFIELD. A series of perfectly natural events: The downfall of rain which produces the vegetables and forage for our animals and the residence in the temperate zone of more people than in other zones, all of these perfectly natural events seem to conspire toward a maximum amount of damage to the maximum number of people in the world.

Dr. STRAUB. This seems to be technically so in this particular area. Just to show actually what has been happening, in the Missouri and Mississippi River area, the rainfall activity values that we have encountered from our radiation surveillance network operations are indicated by squares in chart 3 at these various stations and you can see they are spread around quite a bit. We have also indicated in terms of the blue circles the actual levels of activity found in the water. The difference between these two sets of values essentially represents what is removed by natural agents.

In other words, this fraction represents what may be retained by the soil through ion exchange, what may be picked up by plants directly from the soil or for foliar contamination. So we have a decontamination factor between these two levels of 135 which indicates roughly a removal of 99.3 percent as far as the water is concerned. So that the bulk of the activity is retained on the soil but what is retained must be corrected for decay. Obviously, the short-lived materials so deposited, which would not be found in the stream, are also included in this total.

Coming back to milk for a few moments, the Public Health Service has had in operation since about May 1957 and has received on a monthly basis samples from Sacramento, Salt Lake City, St. Louis, Cincinnati, and New York milksheds. Since last year, about June or July, we have added five stations which provide some additional information on northern and southern areas of the Nation. These are Atlanta; Austin; Chicago; Fargo, N. Dak.; Moorehead, Minn.; and Spokane.

In addition to obtaining information on the radionuclide composition of these milk samples, information was obtained on the feeding practices, the water supplies, and the breeds of cattle typical in each of these areas. Unfortunately, the data that have been so obtained have not been critically examined to draw any basic conclusions or to relate the findings in the field with the activity levels to date.

Representative DURHAM. What do you account for the difference in New York and Austin and Atlanta. Is it concentration in the temperate zone of this fallout? Your chart appears to be so different.

Dr. STRAUB. As I have said, we have not been able to correlate some of our environmental findings yet with the actual conditions. For the cities that you suggested, for example, levels of Sr^{90} at Atlanta are considerably higher than at Austin. These have been running around 10 to 13 micromicrocuries per liter and 3 or 6 micromicrocuries per liter, respectively. One of the differences could be rainfall. We have not made a serious study of this relationship.

The other point of difference (chart 4 (see fig. 2, p. 979)) that is of some significance is that Sacramento and Salt Lake City show essen-

tially very little difference in terms of the annual average of strontium 90 in these two sources. St. Louis, on the other hand, and Cincinnati show roughly a twofold increase in the period from 1957 to 1958. We are starting off with a higher level in 1959 than we started with in 1958.

This again ties in with the information we have been hearing here yesterday in terms of additional fallout this spring. So we can presume that these levels will be going on. In the case of New York there was not too much variation between 1957 and 1958. There was some. But we have also indicated here (chart 4) the information compiled by the Health and Safety Laboratory for the period 1954 to 1957.

Here again you can see that there has been a substantial increase during the 5-year period under discussion, beginning at about one to 2 micromicrocuries or less in 1954 rising to 10 or so at the present time.

Representative DURHAM. Then you have been collecting data in that New York area for the last 5 years?

Dr. STRAUB. We have not been, but data have been collected there. The HASL group has been collecting for 5 years. We have been collecting for about 2½ years.

The other point of interest is the agreement between our findings and those obtained by HASL.

Representative DURHAM. Would that length of time apply to all of your stations?

Dr. STRAUB. I didn't understand your question.

Representative DURHAM. Does that length of time, 2½ years that you have been in the New York area collecting data, apply to all of your stations?

Dr. STRAUB. All of these in the first row (chart 4). We started analyzing samples in June 1958 at the five lower stations. We have also shown on this chart, as will be made clear in the record, the Consumer Report findings. Again you see that even though these represented the results of only two samples collected during July and August 1958 there is very close agreement between the values found and the other values reported.

Representative DURHAM. How is the data assimilated? Is it through the public health setup or through the Agriculture Department?

Dr. STRAUB. All of these samples are obtained and the actual sampling programs have been developed through the various groups of the Public Health Service and the local milk producers themselves. This has been strictly a local operation as have been all of our sampling and our surveillance operations. The samples are collected by State and local health personnel, and are submitted to us for analysis. The same thing is done here.

Representative DURHAM. It is available to the public?

Dr. STRAUB. The milk information has been released on a monthly basis.

Representative DURHAM. I think that is a good thing to do as often as you can because there is a large concern about this problem.

Dr. STRAUB. In these releases the averages have been indicated along with the individual values that have been found. We do feel that the annual average is much more significant than a single value be-

cause this can fluctuate due to local variations at the time of sampling.

The closeness of the results by the various groups summarized on this chart is rather remarkable because of the fact that not in all instances has the same portion of the milkshed been sampled. This shows that over a period of time even though the same specific area may not be sampled, basically the results are still comparable. To bring this out a little bit more specifically perhaps you can examine this slide [refers to table 10 in prepared statement, p. —] which shows data taken from four additional portions of the milkshed in St. Louis. It will be seen that the strontium 90 levels, even though in the fairly large geographical areas included in this presentation, have varied only from 9 to 14.2 for these four additional areas, whereas, the routine sample we collected at that time showed a value of 12.2, which is a reasonable average of the other four values. St. Louis milk comes from an area in Missouri as well as from an area in Illinois.

Now I would like to go to a discussion of some of our nonnetwork operations which have been going on for quite some time. Under this category, I would like to give the results we have obtained not only for rainfall but also cistern water supplies and surface water supplies in the Cincinnati area.

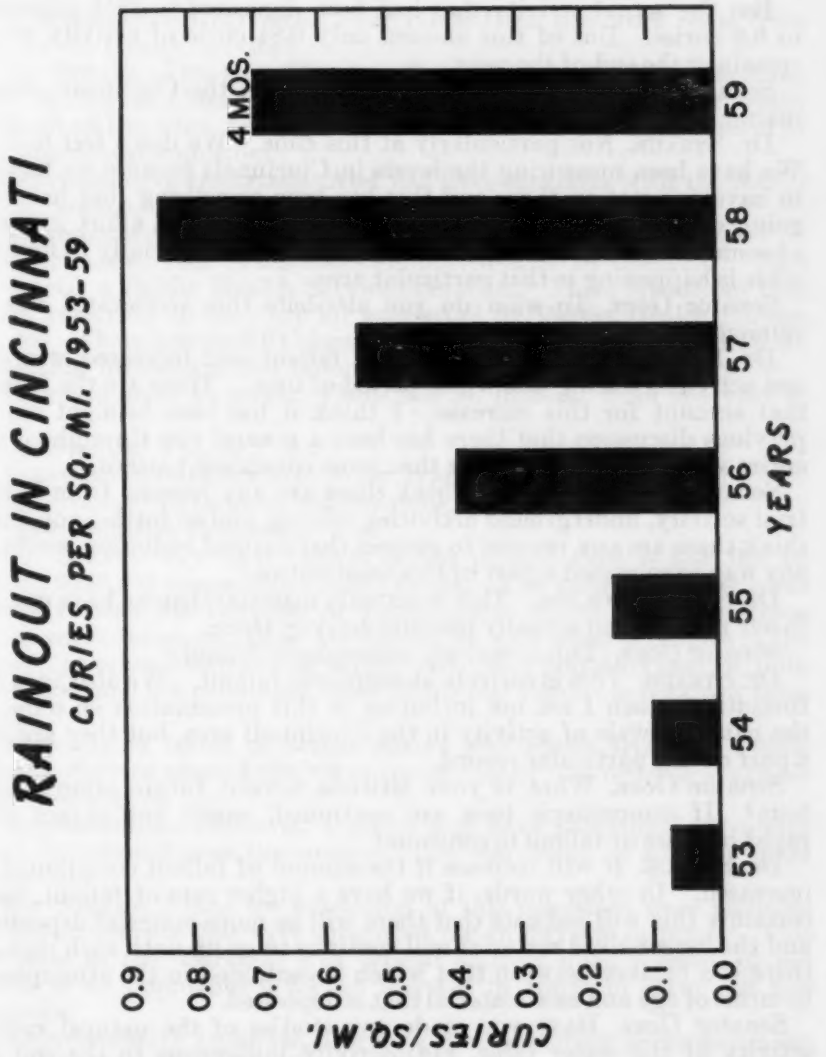
The levels of radioactivity in rainfall obviously reflect nuclear test operations and, in general, the larger percentage of the nuclear activity is found in the suspended fraction shortly after such tests. The values that we have found in rainfall, snow, cistern, and surface waters during 1958 are shown in this particular slide. [See figure on p. 984.]

The levels that we have in regard to rainfall are represented by the dotted portion at the top of the sheet. The black line going through is the actual average value. This rain has an activity level ranging from 10^4 to 10^3 micromicrocuries per liter. The cistern levels are represented by the crosshatched area and are a factor of 10 below that. The surface waters are lower as indicated and are more than one order of magnitude below the levels we encountered in rain. This again indicates there is removal by runoff from these natural agents.

It will be seen that the surface air particulate values run about a millionth of the rainfall activities. For comparison we have shown the strontium 90 levels in milk. That is this little line you see here. In Cincinnati this has ranged from about 6 to 8 or 10 micromicrocuries per liter.

The rainout data in terms of curies per square mile of 1-year-old bomb debris have been estimated and calculated for the period 1953 to 1959 and are summarized in chart 5. [Also refers to table 12, p. 986.] It will be seen that during this period the levels have increased from less than 0.1 micromicrocuries per square mile—this it must be remembered is gross activity—to a value of about 0.84. But during the first 4 months of 1959, again estimating what has been contributed in terms of 1-year-old activity, we have a value of 0.70, which is almost equal to what we estimated for all of 1958. This again bears out some of the comments that were made earlier about a much greater contribution of fallout during this coming spring and this season than we have had in past years.

CHART 5



Senator GORE. I don't quite understand your chart. Yes, I see. You begin with 1953.

Dr. STRAUB. This is 1953, sir.

Senator GORE. Then the rainout in Cincinnati is roughly eight times as much in 1958 as in 1953?

Dr. STRAUB. This is in terms of the actual contributed 1-year-old material.

Senator GORE. I don't understand the meaning of your term "contributed 1-year-old material."

Dr. STRAUB. I have a value indicated in the report that for 1958, of the activity we measured, we estimated that at the end of 1 year a certain fraction would remain. This we have plotted here. So we have reported essentially that it would be 0.70 in 1959.

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But the actual activity that had been deposited in 1958 amounted to 6.3 curies. But of this amount only 0.84 curie of activity would remain at the end of the year.

Senator GORE. Is the amount of rainout in the Cincinnati area a matter of concern for health authorities there?

Dr. STRAUB. Not particularly at this time. We don't feel it is so. We have been measuring the levels in Cincinnati because we happen to have our station there and this has been something that has been going on and does not require very much additional effort in terms of some of the other things we are doing. It is essentially to find out what is happening in that particular area.

Senator GORE. To what do you attribute this accelerated rise in rainout in the Cincinnati area?

Dr. STRAUB. Essentially increased fallout and increased weapons test activities during this whole period of time. These are the factors that account for this increase. I think it has been brought out in previous discussion that there has been a general rise throughout the entire world, and this follows that same consistent pattern.

Senator GORE. You don't think there are any reasons from industrial activity, underground activities, mining, and so forth—you don't think there are any reasons to suspect that natural radiation would in any way have caused a part of this acceleration?

Dr. STRAUB. No, sir. This is actually material that we have caught in our rainfall and actually measure activity there.

Senator GORE. This is entirely atmospheric fallout?

Dr. STRAUB. This is entirely atmospheric fallout. We also have information which I am not including in this presentation on some of the natural levels of activity in the Cincinnati area, but they are not a part of this particular record.

Senator GORE. What is your attitude toward future atmospheric tests? If atmospheric tests are continued, would you expect this rapid increase of fallout to continue?

Dr. STRAUB. It will increase if the amount of fallout contributed is increased. In other words, if we have a higher rate of fallout, then certainly this will indicate that there will be more material deposited and the longer-lived material will continue to go up until such time as there is a balance between that which is contained in the atmosphere in terms of age and new material that is deposited.

Senator GORE. Have you made any studies of the natural radioactivity of the water table, radioactivity indigenous to the soil in that area?

Dr. STRAUB. We have not done this in terms of any well samples in the immediate Cincinnati area. We have done it in terms of natural air activity and this we find to be considerably higher. We also do know from geological information that is available to us that we encounter higher levels of activity in these geological formations. In fact they are part of the Chattanooga shales that come out of Chattanooga and outcrop in the Cincinnati area. That is all part of the same area. We will be doing more of this as soon as we find personnel, staff, and space to carry out this particular operation.

We feel this is necessary in order to get a complete picture of total exposure which I will include in one of my later sections of the paper.

Representative HOLIFIELD. Your funds from this work come from the Public Health Service?

Dr. STRAUB. Yes, sir. This is a Public Health Service operation. Our facility at Cincinnati is the sanitary engineering center which does all of the work for the Bureau of State Services in the realm of environmental sanitation.

Representative HOLIFIELD. And you have contracts with the AEC?

Dr. STRAUB. Yes, sir; we do.

Representative HOLIFIELD. You take care of the 10 milksheds?

Dr. STRAUB. Yes. This is not supported at all by the AEC. This is strictly a Public Health Service operation. These operations that I am referring to here are all supported by Public Health Service funds. There are no AEC funds involved in any of the work that we are doing in terms of the fallout program I am discussing now.

Representative DURHAM. There was an increase from last year in this?

Dr. STRAUB. Yes, sir.

Representative HOLIFIELD. What is your budget in this area?

Dr. STRAUB. In terms of fallout alone, sir?

Representative HOLIFIELD. Yes.

Dr. STRAUB. My total budget at the present time is $337,000 of which $140,000 comes from AEC funds. The program funds come directly from the Division of Radiological Health. Because these other surveillance activities are carried on with other programs at the center, in other words, water supply and water pollution control, milk and food, and air programs, some additional funds are obtained from them.

Basically in terms of actual money that comes from the Public Health Service about $160,000 or something of that order is available for these studies.

Representative DURHAM. I notice you have the milksheds pretty well distributed over the country. Do you desire to add any more this year?

Dr. STRAUB. Plans for expansion of this operation were included in the statements that were submitted for an increase in funds. This increase in the operation will depend entirely upon whether or not there is an opportunity for an increase in funds to carry out this activity.

Representative DURHAM. Is your problem getting personnel in order to carry out your functions?

Dr. STRAUB. Our big problem is not so much personnel as it is space.

Representative DURHAM. In space?

Dr. STRAUB. Yes, sir.

Representative HOLIFIELD. I think you are to be commended on this milk sampling program, and I trust that you will be able to keep it up because I think it is one of the important concerns of the mothers of this country, that is, the concentration of any deleterious materials in milk which the babies drink.

Dr. STRAUB. May I refer to another aspect which is not included in our formal statement. This is our relations with the States in terms of this activity. The States would like to have us expand our network operations. At the present time, since we do not have the

space to expand these operations, we have been trying to approach this from other points of view.

One is through simplified analytical techniques. We have two proposals that appear to be very, very satisfactory in this regard which will permit a wider surveillance and more rapid determination of strontium 90 by perhaps less sophisticated or experienced personnel. This is one approach. These methods will be made available to the States just as soon as we are satisfied they are entirely satisfactory. This should be done within the next month or two.

Representative HOLIFIELD. I think this is very good approach. We have laboratory facilities in every State of the Union?

Dr. STRAUB. Yes, sir.

Representative HOLIFIELD. If you can simplify your techniques of sampling to the point where it can be done satisfactorily on a local level, I would think that many States would be interested in assuming this financial and technical burden themselves?

Dr. STRAUB. This has been demonstrated to us. The other approach is that the States themselves send personnel to Cincinnati for training to become familiar with the techniques that are being used. In fact this past week, we had a representative from Norfolk, Va., who has spent 2 weeks with us. We had representatives from Indiana who were in to see whether we could make arrangements for a program to carry out milk analyses with them. They were so pleased with the new methods that they said they could undertake the whole operation themselves, and we would not have to do anything.

This is the type of approach that is going on and we think it has considerable promise.

Representative HOLIFIELD. You might be able to fan out by that method of training people in different areas and organization much quicker than trying to do it yourself, and the amount of money you spend in training might be multiplied a great deal by having people trained in the field rather than to depend upon your own organization?

Dr. STRAUB. Yes, sir. May I make one other comment. You brought up this matter of training. The budget that I referred to did not include the budget for training. There is a companion training program going on at the center.

Representative HOLIFIELD. How much does that amount to in terms of dollars?

Dr. STRAUB. If I remember correctly, they have essentially under $200,000. This is something that will be provided in testimony that you requested from Dr. Weber yesterday. The amount involved is something of the order indicated.

Representatives HOLIFIELD. That would give you enough money to do quite a bit of training?

Dr. STRAUB. All of the training programs that we have in this area are oversubscribed.

Representative HOLIFIELD. We are running a little short of time. Have you completed your prepared statement?

Dr. STRAUB. No.

Representative HOLIFIELD. Hit the points on that and we will accept all these charts and other data in the record.

Dr. STRAUB. Let me refer to what we have done on food materials in the Cincinnati area and some of the material that we have presented in the record. The results we have obtained may be compared with the results that have been reported by the British, and we find that there is considerable agreement in various areas.

The one thing that I would like to bring out, however, in terms of our own data and the British data (refer to table 18, p. 996) is that although about 60 percent of the calcium taken in per day comes from milk products (this value is essentially identical with that of the British), only 36 percent of our strontium 90 comes in through this route on the basis of what we calculated at Cincinnati compared with 55 percent in Great Britain.

From our analysis of the food materials that reach the Cincinnati market, the strontium 90 through the vegetable food route approximately equals that received through the milk route. It is 39 percent versus 36 percent, respectively.

We have also initiated, as was indicated by both Dr. Dunham yesterday and by Dr. Weber, a joint program with the Atomic Energy Commission to determine the actual strontium 90 levels in personnel in the Nevada Proving Grounds area or in that vicinity. We have received within the last month 10 samples which have been analyzed and preliminary data indicate that we have strontium 90 levels ranging from six-tenths to 12 micromicrocuries of strontium 90 per gram of calcium, which appear to be higher than those reported in the past by other individuals. These are preliminary values, and there may be some slight changes. In addition, we will check naturally occurring radionuclides in these materials.

The reason for carrying out all these analyses is basically to find out what is the total intake by man of strontium 90 or other radionuclides to make some attempt to determine potention hazard to man. What we have done essentially, if I may have the last slide (refer to table 21, p. 1001), has been to summarize how much activity from strontium 90 specifically has come from various sources in the environment of the Cincinnati area.

It will be seen that air contributes a very small quantity of strontium 90 as far as intake is concerned. We have also put down cistern waters as well as surface waters and it will be seen that they can contribute an appreciable quantity of strontium 90.

Milk on the other hand, based on a daily intake of 1,400 milliliters provides 11.9 micromicrocuries per day, and food, 6.95 micromicrocuries for a total maximum value of 29 to 34 micromicrocuries per day in this vicinity—Cincinnati. This to us at the present hardly represents a negligible contribution. We do not feel there is any appreciable hazard from this but it is certainly desirable to carry out additional studies and determine what the potential intake of this material is in various sections of the country for three reasons.

One is that it will permit us to isolate those areas where people are receiving higher levels of exposure. Two, it will provide a basis for carrying out epidemiological studies in these areas to see what can be done about minimizing the exposure. The third would be to establish control measures before these levels became critical.

I would like to read the summary and conclusions we have here.

Results of findings from network operations as well as special studies by the Public Health Service at the Robert A. Taft Sanitary Engineering Center were reviewed. The use of environmental radioactivity values in estimating the total intake of strontium 90 is indicated by example. All of the data obtained, however, show the need for more knowledge of the factors affecting retention of strontium 90, particularly the effect of stable strontium and calcium. Since strontium 90 can be found anywhere in the environment, human studies can be carried out in which analyses are made of all food intake as well as all body eliminations. Thus, the objections of added radionuclides to carry out such studies are voided, and measurements are made with the radionuclide in the form in which it is taken into the body rather than in some other form.

Extensions of the studies described herein are needed to define areas of higher activity level and permit initiation of the necessary specific control measures before serious dosage or exposure results.

That completes my statement.

Representative HOLIFIELD. One of the interesting things I notice in your table 20 is that oysters from the Chesapeake Bay range from 124 to 178 micromicrocuries?

Dr. STRAUB. This was for zinc 65. This is the isotope that we have referred to in that table. But milk is very low.

Representative HOLIFIELD. Milk is four?

Dr. STRAUB. Yes, very low. The point of that actually was to indicate that the distribution of this particular radionuclide is far more widespread than believed in the past where it was reported essentially that it would be found in the irrigation waters more particularly around the Hanford area than anywhere else. These levels do not represent a public health hazard for this particular isotope. They are far, far below the currently accepted maximum permissible concentration value. It would be of the order of something like 6 million micromicrocuries per liter.

Representative HOLIFIELD. And we are talking about micromicrocuries in the range of 3 to 178?

Dr. STRAUB. Yes, sir.

Representative HOLIFIELD. Thank you very much.

Dr. STRAUB. Thank you.

(Dr. Straub's formal statement follows:)

STATEMENT ON NEW DATA ON UPTAKE IN MILK, FOOD, AND HUMAN BONE FOR JOINT COMMITTEE HEARINGS ON FALLOUT FROM NUCLEAR WEAPONS, MAY 5-8, 1959.

Conrad P. Straub, Ph. D.

In his statements, Dr. Francis J. Weber, Chief, Division of Radiological Health, Public Health Service, referred to the radiation surveillance networks operated at the Robert A. Taft Sanitary Engineering Center, Public Health Service, Cincinnati, Ohio. It has been suggested that his discussion be expanded in order to indicate our findings relative to activity levels from fallout in environmental media.

As pointed out by Dr. Weber, the National Air Sampling and the Basic Data Water Quality Networks have been established to measure a variety of organic and inorganic pollutants in air and water, respectively. Radioactivity is but one of the contaminants measured in these samples and the only contaminant of interest to the Joint Committee at this time. However, collateral information on other contaminants may be of considerable value when a more complete understanding exists of the synergistic and antagonistic effects of all sources of environmental contamination upon the disease pattern of the nation.

The radiation surveillance activities of the Public Health Service have been developed to assess the levels of radioactivity in a wide variety of environmental media and to delineate and define the specific radionuclides responsible for such contamination. Initially, our major

effort was in the development of suitable techniques for measuring the low levels of radioactivity encountered in surface waters. (See attachments 1, 2, and 3). Later, studies were initiated to measure the gross activity in rainfall, vegetation, biological samples, soils, etc. As environmental radioactivity levels increased, it became necessary to define the specific radionuclides responsible for the incfeased activity. Such analytical information is necessary to an estimation of the radiation dose resulting from the ingestion and inhalation of these materials.

NETWORK PROGRAMS

As has been indicated, the bulk of the routine activities of the Public Health Service insofar as radiation surveillance is concerned is through network programs in the air, water, and milk fields. The stations included in each of these network programs are shown in Figure 1.

Air

Samples of surface air particulates from 114 urban and 51 non-urban stations are collected on glass fiber filters by high volume samplers for a period of 24 hours on a bi-weekly schedule. Following collection, the samples are shipped to the Center and counted after two days — to allow decay of the natural alpha and associated beta-gamma activities — and again after four to seven days to estimate the approximate time of formation of the mixture from the rate of

Fig. 1

ENVIRONMENTAL SAMPLING STATIONS FOR RADIATION SURVEILLANCE

radioactive decay. From this information, a rough estimate of the radionuclide composition can be made.

National average surface air particulate radioactivity levels for the period 1953-1957 are summarized in Table 1, along with Cincinnati levels. It will be seen that there has been a continuous increase in the average annual surface air particulate radioactivity levels with a 50-fold increase in the National average for the period and a 20-fold increase in the Cincinnati levels. The National radioactivity values range from a low of less than 0.1 to a high of 5,435 micromicrocuries per cubic meter ($\mu\mu c/M^3$) (Salt Lake City - 1957), whereas the Cincinnati values ranged from less than 0.1 to 21.9 $\mu\mu c/M^3$. These data were summarized from the report "Air Pollution Measurements of the National Air Sampling Network, 1953-1957" released by the Public Health Service in 1958.

Water

Samples of surface waters are collected by local personnel from 51 sampling stations located on 16 major river basins and the Great Lakes and submitted to Cincinnati on a weekly basis. Upon arrival, the samples are filtered through membrane filters to separate radioactivity associated with suspended matter from that in solution. Since water treatment plants employing chemical treatment and filtration should remove essentially all of the suspended solids, this differentiation is important.

After separation, all samples are counted for gross beta-gamma

Table 1

Average Surface Air Particulate Radioactivity Levels

$\mu\mu c/M^3$

Location	Year				
	1953	1954	1955	1956	1957
Nation	0.2	1.0	2.4	2.1	10.5
Cincinnati	0.2	0.6	1.1	2.0	4.3

radioactivity and a fraction of the samples for alpha radioactivity. Those samples showing higher levels (generally above 500 micromicrocuries per liter)(μμc/l) are checked for specific radionuclides. Within the last six months, aliquots of the weekly samples have been composited for quarterly determination of strontium-90.

An examination of the results obtained on samples collected during the period November 1957 to October 1958 shows that the National surface water average was 56 μμc/l of gross total beta-gamma activity, of which the dissolved fraction amounted to 27 μμc/l or 48 per cent of the total activity (levels found in the Columbia River are not included), and averaged 3.7 μμc/l of gross alpha activity. The few strontium-90 determinations made (20 analyses to date) show a level of 1.0 μμc/l with a range of 0.3 to 4.7 μμc/l. Strontium-89 levels on these same samples averaged 1.6 with a range of 0 to 4.0 μμc/l. These values are summarized in Table 2 with values reported by the British (1) and the AEC (2). For comparison, some values for deep well supplies also are indicated. The upward trend of strontium-90 during the last few years is evident from these data.

Strontium-90 levels in water are of interest from two points of view — both involving ingestion. The first is concerned with the use of such water as a potable supply for humans and animals, particularly where ponds, lakes, or small reservoirs may be used for such purposes as contrasted with river supplies, which because of their flow show lower levels of

Table 2

Radiostrontium Content - Water Supplies

Source	Date	Sr^{90} Average	Sr^{90} Range	Sr^{89} Average	Sr^{89} Range
PHS-US-Surface waters	1958-1959	1.0	0.3-4.7	1.6	0-4.0
Great Lakes			0.7		0
HASL-42-Tap water, N.Y.	1954	0.06	0.04-0.09		
	1955	0.11	0.05-0.23		
	1956	0.15	0.01-0.26		
	1957	0.15	0.05-0.27		
Tap water, Chicago	1953-1956	0.11	0.05-0.16		
	1953	0.11	0.09-0.13		
River samples	1957	0.63	0.25-1.1		
Reservoirs	1954	0.16	0.01-0.11		
British Data-River supplies	1957-1958	0.24	0.11-0.44	2.30	0.36-8.1
Lakes	1957-1958	0.45	0.36-0.85	0.93	~0.19-2.2
Deep wells	1957-1958	<0.01			

this particular radionuclide. The second, has to do with the use of these waters in the preparation of food materials such as soups, beverages, reconstitution of milk, preparation of baby milk formulas, or wherever water is used as an ingredient in food preparation.

Milk

Currently, samples are received on a monthly basis from portions of milksheds serving the following cities: Sacramento, Salt Lake City, St. Louis, Cincinnati, New York, Atlanta, Austin, Chicago, Fargo-Moorhead, and Spokane. In each case, the selection of sampling stations within the milk shed was based upon the following criteria (3):

> "1. The milk represented in each sample was from a group of farms milking a total of at least 1,000 cows.
>
> "2. The number of individual farms was small enough so that collection of collateral field data on each farm was feasible.
>
> "3. The milk samples were from a supply that was a part of a metropolitan milkshed.
>
> "4. The conditions under which the milk was received were such that each sample was representative of the same farms in the production area."

The collateral farm survey data included information on feeding practices, water supplies, and breeds of cattle typical of each area. These data have not been examined as yet in relation to the levels of radioactivity in milk. Forms used for the shipment of samples and collection of collateral data are attached (Attachments 4 and 5).

Analytical results which have formed the basis of the monthly milk releases to the public and radionuclide and stable calcium levels in milk are summarized in Tables 3-8. Since the strontium, calcium, and iodine levels are of more interest, these have been redefined in terms of yearly averages in Table 9.

The data in Table 9 show that the average levels of strontium-90 at Sacramento and Salt Lake City have been essentially constant over the period indicated, that the levels for New York have shown a slight increase, and that the St. Louis and Cincinnati levels have increased by a factor of approximately two during this period. The five newer stations have not shown much fluctuation in the average levels of strontium-90, although there were marked differences between milksheds — Austin 3.5 to 6.0 μμc/l and Fargo-Moorhead 12.0 to 13.6 μμc/l. Highest strontium-89 levels were found in St. Louis and the lowest in Sacramento. Salt Lake City and St. Louis showed highest iodine levels and Sacramento and Atlanta the lowest.

In Figure 2, the strontium-90 values have been plotted on an annual basis to illustrate the above comments. The strontium-90 level reported by the Health and Safety Laboratory (HASL) for New York are also shown with the levels reported by Consumer Report for the two samples taken in each of 50 major U. S. cities in July and August, 1958. A steady increase is observed in New York strontium-90 levels since 1954

Table 3

Concentration of Strontium-89 in Milk
(expressed as μμc/l)

	MILKSHEDS									
1957-59	Sacramento	Salt Lake City	St. Louis	Cincinnati	New York City	Atlanta	Austin	Chicago	Fargo	Spokane
May 57	-	-	-	-	-					
June	-	-	-	-	-					
July	-	-	-	-	-					
Aug	26.2	71.7	79.3	87.3	74.0					
Sept	14.1	55.6	243.2	66.4	84.1					
Oct	21.7	90.5	141.9	131.7	122.0					
Nov	9.2	23.5	66.4	48.9	37.4					
Dec	21.4	11.1	29.5	12.3	14.1					
Jan 58	0	8.6	25.5	0.3	5.5					
Feb	2.9	8.1	14.8	8.5	3.4					
Mar	17.6	3.3	19.1	5.6	2.6					
Apr	77.4	2.2	35.5	13.1	2.0					
May	38.1	25.5	107.0	70.4	23.4					
June	39.6	68.0	122.1	106.2	63.1	37.4	25.7		79.6	
July	19.6	26.1	306.2	193.9	83.5	39.3	15.0	58.5	142.3	
Aug	23.8	44.0	263.2	96.9	40.9	113.8	40.1	63.9	53.3	63.9
Sept	17	44	283	98	73	72	32	94	140	43
Oct	11	26	138	56	85	76	35	53	151	53
Nov	18	13	168	108	55	30	49	64	53	35
Dec	10	5	184	37	11	27	32	14	31	21
Jan 59	15	6	77	33	10	62	27	6	27	20
Feb	12	5	84	25	6	62	37	1	17	16

Table 4

Concentration of Cesium-137 in Milk
(expressed as μμc/l)

Month 1957-59	MILKSHEDS									
	Sacramento	Salt Lake City	St. Louis	Cincinnati	New York City	Atlanta	Austin	Chicago	Fargo	Spokane
May 57	31.7	30.6	18.0	26.0	37.8					
June	40.6	52.6	69.1	35.8	30.4					
July	48.5	51.0	61.3	36.2	22.8					
Aug	78.7	39.0	36.4	40.3	48.8					
Sept	44.2	154.4	63.7	53.3	69.1					
Oct	40.5	47.2	65.7	41.6	48.4					
Nov	48.7	44.9	35.1	36.2	45.0					
Dec	33.3	35.4	48.9	41.3	40.2					
Jan 58	43.6	38.5	41.1	48.3	29.5					
Feb	44.2	65.3	44.7	55.5	46.3					
Mar	47.9	79.5	54.7	61.6	50.6					
Apr	102.2	65.0	61.4	50.2	44.3					
May	54.3	41.5	94.2	50.1	39.7					
June	79.1	59.2	95.8	77.5	70.7	118.1	74.3		109.5	
July	52.8	39.9	160.2	95.8	82.8	111.3	47.5	111.8	147.3	
Aug	65.0	43.0	109.1	84.2	67.2	103.4	50.9	88.5	96.1	92.6
Sept	72	48	104	59	76	79	43	71	113	76
Oct	39	31	71	53	53	77	33	55	67	72
Nov	46	37	74	64	67	56	42	64	56	72
Dec	48	39	75	63	71	72	44	71	77	84
Jan 59	35	34	64	55	65	84	39	64	67	73
Feb	46	37	73	60	59	81	49	71	61	84

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Table 5

Concentration of Barium-140 in Milk
(expressed as μμc/l)

Month 1957-59	MILKSHEDS									
	Sacramento	Salt Lake City	St. Louis	Cincinnati	New York City	Atlanta	Austin	Chicago	Fargo	Spokane
May 57	-	-	-	-	-					
June	-	71.5	94.7	-	-					
July	10.8	197.9	116.8	-	-					
Aug	9.6	46.3	51.3	98.8	76.4					
Sept	25.0	83.0	532.0	125.8	125.9					
Oct	43.3	130.8	120.9	68.5	158.8					
Nov	46.9	5.1	8.3	13.3	3.5					
Dec	25.7	3.5	16.4	3.5	0					
Jan 58	0	1.3	6.0	0	0					
Feb	8.2	0.5	4.1	1.7	8.7					
Mar	6.4	0.3	4.6	1.7	0.9					
Apr	20.8	0	50.2	0	0					
May	12.0	7.6	14.4	15.6	4.8					
June	5.9	20.9	44.3	35.7	20.4	6.4	1.2		17.6	
July	7.0	15.8	229.0	112.1	63.9	11.6	4.6	35.8	46.9	
Aug	0.0	20.3	112.7	83.8	27.5	49.5	17.6	17.9	14.1	10.5
Sept	0.0	14	46	13	15	7	6	10	53	5
Oct	1	5	14	8	14	4	0	1	16	8
Nov	14	4	36	10	30	8	30	3	28	52
Dec	6	0	8	4	0	8	12	3	0	7
Jan 59	0	5	14	4	3	8	2	0	1	3
Feb	0	0	4	0	0	0	3	0	0	0

Table 6

Concentration of Calcium in Milk
(expressed as gm/l)

Month 1957-59	MILKSHEDS									
	Sacramento	Salt Lake City	St. Louis	Cincinnati	New York City	Atlanta	Austin	Chicago	Fargo	Spokane
May 57	1.127	1.114	1.200	1.146	1.008					
June	1.092	1.120	1.204	1.151	1.047					
July	1.102	1.115	1.199	1.138	1.034					
Aug	1.114	1.125	1.200	1.142	1.038					
Sept	1.090	1.126	1.204	1.157	1.033					
Oct	1.141	1.167	1.273	1.222	1.116					
Nov	1.146	1.172	1.298	1.192	1.101					
Dec	1.157	1.165	1.313	1.162	1.116					
Jan 58	1.167	1.164	1.317	1.180	1.123					
Feb	1.123	1.092	1.310	1.154	1.092					
Mar	1.149	1.155	1.273	1.080	1.047					
Apr	1.146	1.131	1.244	1.144	1.078					
May	1.141	1.142	1.259	1.161	1.036					
June	1.122	1.129	1.217	1.227	1.093	1.164	1.115		1.112	
July	1.036	1.068	1.175	1.082	1.062	1.158	1.144	1.069	1.108	
Aug	1.150	1.108	1.280	1.102	1.048	1.188	1.160	1.098	1.116	1.168
Sept	1.161	1.207	1.303	1.132	1.241	1.154	1.121	1.161	1.125	1.163
Oct	1.109	1.187	1.271	1.175	1.141	1.220	1.148	1.123	1.215	1.202
Nov	1.111	1.162	1.280	1.157	1.069	1.193	1.172	1.141	1.166	1.223
Dec	1.166	1.161	1.274	1.148	1.114	1.214	1.176	1.103	1.165	1.241
Jan 59	1.150	1.137	1.329	1.141	1.060	1.242	1.216	1.131	1.159	1.268
Feb	1.151	1.176	1.282	1.155	1.115	1.218	1.153	1.119	1.135	1.183

Table 7

Concentration of Strontium-90 In Milk
(expressed as μμc/l)

Month 1957-59	MILKSHEDS									
	Sacramento	Salt Lake City	St. Louis	Cincinnati	New York City	Atlanta	Austin	Chicago	Fargo	Spokane
May 5"	4.5	4.6	10.1	5.5	4.6					
June	7.0	6.4	12.9	6.7	5.1					
July	3.6	4.9	11.0	5.1	8.7					
Aug	1.9	4.1	7.7	4.4	6.9					
Sept	1.5	4.3	9.1	5.0	6.1					
Oct	1.7	5.5	9.7	7.7	5.0					
Nov	9.6	4.8	7.8	7.0	5.2					
Dec	5.0	3.0	7.1	5.9	5.0					
Jan 58	5.9	3.0	7.9	4.1	5.6					
Feb	3.8	2.2	7.0	3.9	5.2					
Mar	4.2	3.0	10.0	4.1	3.9					
Apr	10.5	4.7	12.3	6.4	4.0					
May	3.8	3.3	10.0	8.6	3.6					
June	7.1	7.1	14.8	11.6	9.9	13.7	2.2		15.6	
July	3.3	4.7	18.7	13.0	10.5	10.2	3.5	7.1	16.3	
Aug	6.8	4.2	14.1	7.5	3.3	11.9	3.4	9.3	10.2	6.9
Sept	3.8	5.0	15.4	9.4	5.6	13.8	3.9	7.9	14.1	6.2
Oct	1.4	3.1	12.2	8.6	9.5	10.4	5.1	6.0	15.0	8.6
Nov	6.1	5.2	20.1	15.2	8.8	8.8	3.7	8.4	11.5	11.9
Dec	4.2	6.1	15.6	9.6	7.9	10.2	2.6	7.0	12.3	8.6
Jan 59	4.3	4.5	18.6	11.7	6.9	13.4	5.7	6.7	12.3	9.1
Feb	3.9	3.4	19.6	12.2	6.5	7.3	6.3	6.3	11.8	6.3

Table 8

Concentration of Iodine-131 In Milk
(expressed as μμc/l)

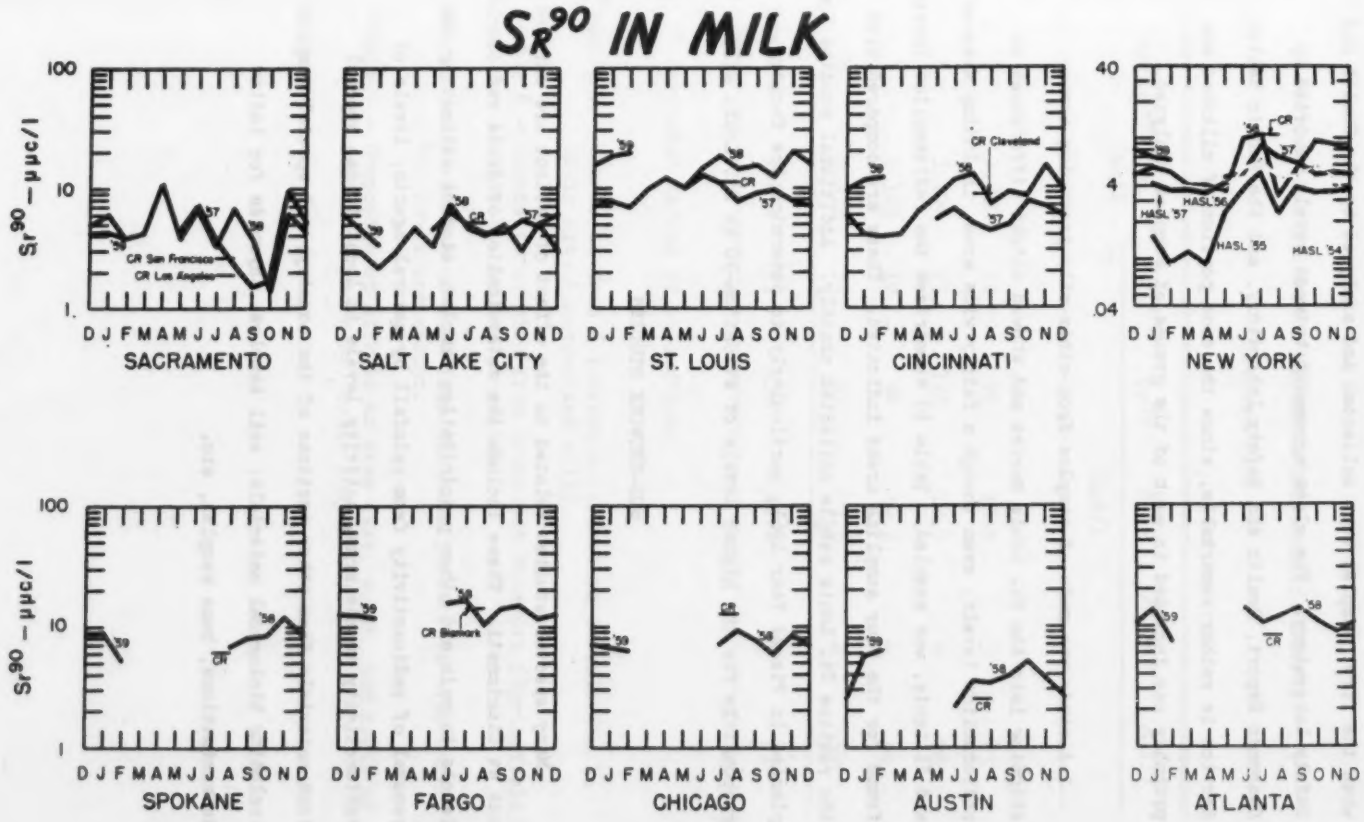
Month	MILKSHEDS									
1957-59	Sacramento	Salt Lake City	St. Louis	Cincinnati	New York City	Atlanta	Austin	Chicago	Fargo	Spokane
May 57	-	-	-	-	-					
June	250	440	410	370	40					
July	-	-	-	-	-					
Aug	10	200	210	230	50					
Sept	10	740	970	430	300					
Oct	20	990	890	300	250					
Nov	0	0	0	0	100					
Dec	0	50	0	0	0					
Jan 58	0	0	0	0	0					
Feb	0	40	0	0	0					
Mar	0	0	0	0	0					
Apr	10	30	100	30	50					
May	0	0	0	0	32					
June	240	0	0	0	0	0	0		30	
July	45	156	624	107	67	22	8	124	92	
Aug	22	77	61	42	69	65	50	43	48	68
Sept	9	14	19	5	10	20	16	13	27	11
Oct	12	11	35	31	61	6	11	20	20	8
Nov	17	33	123	177	35	32	181	56	86	67
Dec	14	8	23	8	9	21	39	8	0	30
Jan 59	0	2	7	2	0	7	6	4	2	7
Feb	3	1	0	4	5	5	10	8	8	4

Table 9. Radionuclide (μμc/l) and Calcium (gm/l) Levels in Milk - Annual Data

Station	Year	No. of Samples	Strontium-90	Strontium-89	Iodine-131	Stable Calcium
Sacramento	1957	5-8	4.4	18	50	1.121
	1958	12	5.1	23	30	1.132
	1959	2	4.1	14	2	1.151
Salt Lake City	1957	5-8	4.7	50	400	1.138
	1958	12	4.3	23	30	1.142
	1959	2	4.0	6	2	1.156
St. Louis	1957	5-8	9.4	112	410	1.236
	1958	12	13.2	139	80	1.267
	1959	2	19.1	80	4	1.306
Cincinnati	1957	5-8	5.9	69	220	1.164
	1958	12	8.5	66	30	1.145
	1959	2	12.0	29	3	1.148
New York	1957	5-8	5.8	66	120	1.062
	1958	12	6.5	37	30	1.097
	1959	2	6.7	8	2	1.088
Atlanta	1958	7	11.3	56	20	1.184
	1959	2	10.4	62	6	1.230
Austin	1958	7	3.5	33	40	1.148
	1959	2	6.0	35	8	1.184
Chicago	1958	6	7.6	58	40	1.116
	1959	2	6.5	4	6	1.125
Fargo-Moorhead	1958	7	13.6	94	40	1.144
	1959	2	12.0	22	5	1.147
Spokane	1958	5	8.4	43	40	1.199
	1959	2	7.7	18	6	1.226

Fig. 2

Fig. 2


Sr90 IN MILK
Sr90 — μμc/l
100
10
1
40
4
.04
D J F M A M J J A S O N D
SACRAMENTO
SALT LAKE CITY
ST. LOUIS
CINCINNATI
NEW YORK
SPOKANE
FARGO
CHICAGO
AUSTIN
ATLANTA
CR San Francisco
CR Los Angeles
CR Cleveland
CR Bismark
HASL '57
HASL '56
HASL '55
HASL '54
CR
'59
'58
'57
'56

when the first samples were collected and analyzed by the Health and Safety Laboratory. The close agreement between levels reported by Consumer Report, Health and Safety Laboratory, and the Public Health Service is rather remarkable, since the same portion of milkshed was probably not included in each of the groups of samples analyzed.

A study was made of samples from other milk producing areas shipping into the St. Louis market and showed slight differences in radionuclide levels, even though a fairly wide area, including Missouri and Illinois, was sampled. Table 10 summarizes the radionuclide levels found for the four sampling areas indicated. These are compared with the routine St. Louis sample collected monthly. Additional studies are planned in Fiscal Year 1961, particularly to determine the factors responsible for the higher levels of strontium-90 in St. Louis milk.

NON-NETWORK STUDIES

Many special studies related to the effect of fallout are carried out at Cincinnati. These include the determination of gross radionuclide levels in rain and other precipitation samples, studies estimating the removal of radioactivity from rainfall by natural agents, levels of radioactivity in cisterns, activity levels in local foods, special food materials from other sections of the country, and special samples including biological materials, soil samples, thyroids for iodine determinations, bone samples, etc.

Table 10. St. Louis Milkshed Radionuclide Levels

October 1958

Sample**	Ca mg/l	I^{131}*	Sr^{89}	Sr^{90}	Ba^{140}*	Cs^{137}
				μμc/l		
1	1.217	11	99	12.4	3	45
2	1.274	15	125	14.7	6	78
3	1.143	17	54	14.2	4	54
4	1.218	15	91	9.0	2	59
Routine	1.271	35	138	12.2	14	71

* Corrected to time of collection.

**Sample 1 - Composite from 6 largest distributors in relation to their size (pasteurized milk).

Sample 2 - Composite of milk from southwest Missouri (Springfield) under St. Louis inspection.

Sample 3 - Composite of milk from Illinois production area under St. Louis inspection.

Sample 4 - Composite of milk in an area west, south, and north of St. Louis under St. Louis inspection.

Rainfall

Under this category consideration will be given to the results obtained in analyzing rainfall samples, cistern water supplies, and surface waters (Cincinnati, and the Ohio area, specifically).

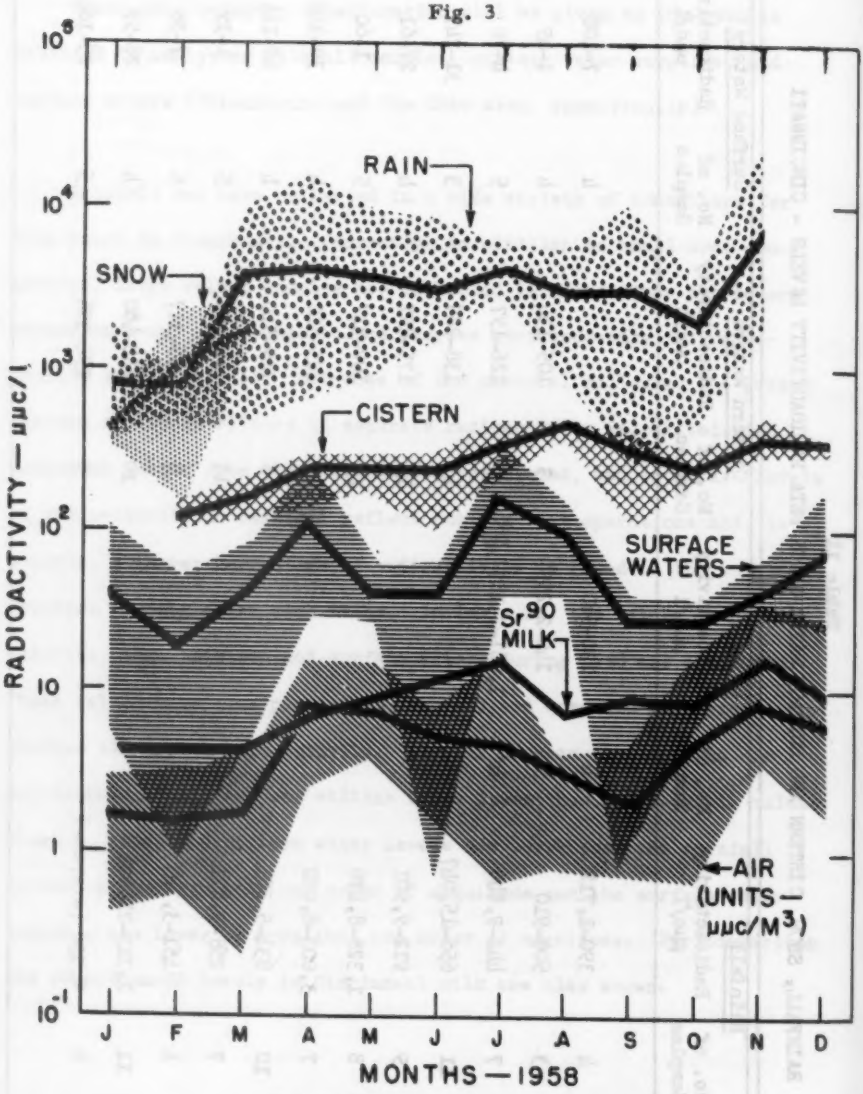
Rainfall has been collected in a wide variety of containers for many years in Cincinnati. Currently, in addition to small area containers, large volume samples are collected in a container two square meters in area. The large volume samples were necessary for radionuclide determinations. Aliquots of the material collected are passed through millipore filters to separate radioactivity associated with suspended matter from that in solution, processed, and counted. Levels of radioactivity in rainfall reflect nuclear test operations and, in general, a larger percentage of radioactivity is found in the suspended fraction shortly after such tests. In Table 11, the values found in rainfall, snow, cistern,and surface waters during 1958 are indicated. These values have been plotted in Figure 3 and compared with gross surface air particulate activity. It will be seen that surface air particulate levels are one million times lower than the rainfall values shown, that the cistern water levels are lower than the rainfall levels by approximately one order of magnitude, and the surface water supplies are lower by more than one order of magnitude. For comparison the strontium-90 levels in Cincinnati milk are also shown.

Table 11

RAINFALL, SNOW, CISTERN AND SURFACE WATER GROSS BETA RADIOACTIVITY LEVELS - CINCINNATI

	Rainfall		Snow		Cistern Waters		Surface Waters	
Month	No. of Samples	Radioactivity μμc/l	No. of Samples	Radioactivity μμc/l	No. of Samples	Radioactivity μμc/l	No. of Samples	Radioactivity μμc/l
1958								
Jan.	4	390-1,712	4	230-560			4	7-108
Feb.	3	500-910	5	140-2,255	3	105-134	4	1-49
Mar.	7	442-9,840	4	1,081-2,187	8	126-197	5	4-70
Apr.	11	656-15,047			13	138-285	3	31-247
May	9	912-9,901			13	175-297	4	27-61
June	8	1,328-8,476			13	102-317	5	1-60
July	7	3,003-6,022			15	225-512	4	39-340
Aug.	10	834-5,381			6	389-510	4	42-141
Sept.	7	258-9,811			10	116-405	5	1-39
Oct.	4	451-3,325			8	205-317	5	7-36
Nov.	11	1,313-21,924			10	288-480	4	28-59
Dec.	4	2,550 (average)			9	294-396	5	25-164

Fig. 3



RADIOACTIVITY LEVELS
ENVIRONMENTAL SAMPLES
CINCINNATI, OHIO
1958

Rainout data in terms of curies of one-year old bomb debris per square mile for the period 1953-1959 are summarized in Table 12 and show that the levels have increased from a value of less than 0.1 in 1953 to about 0.70 curies per square mile during the first four months of 1959. The one-year old material has a half life of about 365 days, and since the activity is deposited as fresh detonation products having a half life of a few days to old products having a half life of 5 months or more, the activity when deposited was much greater than that indicated for the one-year old products. For example, the 1958 estimated fallout was 0.84 curies of one-year old products per square mile as compared to 6.3 curies of total activity per square mile at the time of deposition.

The fallout data for 1958 is summarized in Attachment 6.

Gamma spectral measurements and isotopic analyses, particularly for the determination of total radiostrontium and strontium-90 have been made. To date, the radionuclides identified from the gamma spectral measurements include Ce^{141} and/or Ce^{144}, I^{131}, Ru^{103} and/or Ru^{106}, Cs^{134}, Cs^{137}, Zr^{95}-Nb^{95}, Zn^{65}, Co^{60}, K^{40}, and probably W^{185}. The strontium levels found in rainfall samples are summarized in Table 13 and range from 0.6 to 12.3 μμc/l for total strontium and from 0.3 to 7.5 μμc/l for Sr^{90}. Values reported by the Lamont Geological Survey Observatory are included in the summary.

Table 12. Annual Fallout of Long-lived (One Year Old) Radioactivity - Cincinnati*

Year	Precipitation		Radioactivity		
	Inches	mm	μμc/l	mμc/m²	Curies/sq. mi.
1953	22	558	56	31.3	0.081
1954	36	914	41	37.5	0.097
1955	45	1142	55	63.2	0.16
1956	42	1065	141	150	0.39
1957	49	1242	168	212	0.55
1958	38	955	340	325	0.84
1959**	10	261	1040	272	0.70

* Estimated long-lived activity from decay curves on individual rains.

** January 1 to April 29, 1959.

Table 13. Radioactive Strontium - Rainfall - μμc/l

Date	Total Strontium	Strontium-90	Remarks
June-Sept. 1957	0.6 - 12.3	0.3 - 7.5	Cincinnati rains
June 27-28, 1957	2.1 ± 0.1	1.3 ± 0.1	Cincinnati rain - dissolved fraction
June 27-28, 1957	0.7 ± 0.1	0.9 ± 0.1	Cincinnati rain - suspended fraction
December, 1954		0.11 - 12.4 Average 1.50	Lamont Observatory

The suspended and dissolved solids in recent rains (January-April, 1959) have been concentrated on ion exchange resins and have permitted, at least qualitatively, detection of a wide variety of radionuclides some not previously identified on the unconcentrated rain samples. The gamma spectra identified in the three fractions: particulate, cation exchange resin, and anion exchange resin are summarized in Table 14.

Since rainfall may serve as a source of drinking water, the radioactivity of a number of cistern water supplies were routinely tested. Table 11 included data obtained for a private cistern in Cincinnati and Table 14 shows levels found in Coast Guard cistern water supplies located along the Atlantic, Gulf, and Pacific coasts. The levels have increased in all instances, but are still considered low. Studies are underway to evaluate the efficiency of simple ion exchange systems for the removal of the bulk of the radioactivity associated with these supplies so that such systems may be installed when higher levels are encountered. Such simple control measures may be used to reduce radioactivity in such water supplies.

The activity levels in the Cincinnati cistern increased during the year from an average value of 119 μμc/l in January to a value of 346 μμc/l in December. The highest activity levels were found in August when an average level of 459 μμc/l was found for six determinations. Gamma

Table 14. Gamma Emitting Radionuclides - Rain Samples

(January-April 1959)

Fraction	Radionuclides
Ash Particulate	Ce^{144}, Ru^{106}, Zr^{95}-Nb^{95}, Co^{60}
Cation Exchange Resin	Ce^{144}, Ba^{140}-La^{140}, $Cs^{134,137}$, Ru^{106}, Zr^{95}-Nb^{95}, Zn^{65}, Sc^{46}, Co^{60}
Anion Exchange Resin	Ru^{106}, Zr^{95}-Nb^{95}, Mn^{54}(?), Zn^{65}

scans showed the presence of Ce^{141} and/or Ce^{144}, Ru^{103} and/or Ru^{106}, Zr^{95}-Nb^{95}, and K^{40}. Recently, composite samples yielded strontium-90 levels of 3 to 6 (average 5.8) and strontium-89 levels of 25 to 125 (average 83) μμc/l.

The activity levels in the Coast Guard cistern water supplies (Table 15) ranged from 14 to 409 (average 130) in 1957, from 3 to 705 (average 204) in 1958, and from 1 to 747 (average 374) in 1959. Gamma scans of the more radioactive samples showed the following gamma emitting radionuclides: Ce^{141} and/or Ce^{144}, Ru^{103} and/or Ru^{106}, Zr^{95}-Nb^{95}, Zn^{65}, Sc^{46}, and K^{40}. Strontium determinations on composite samples show a level of strontium-90 ranging from 3 to 7 (average 4.7) and strontium-89 from 35 to 130 (average 77) μμc/l.

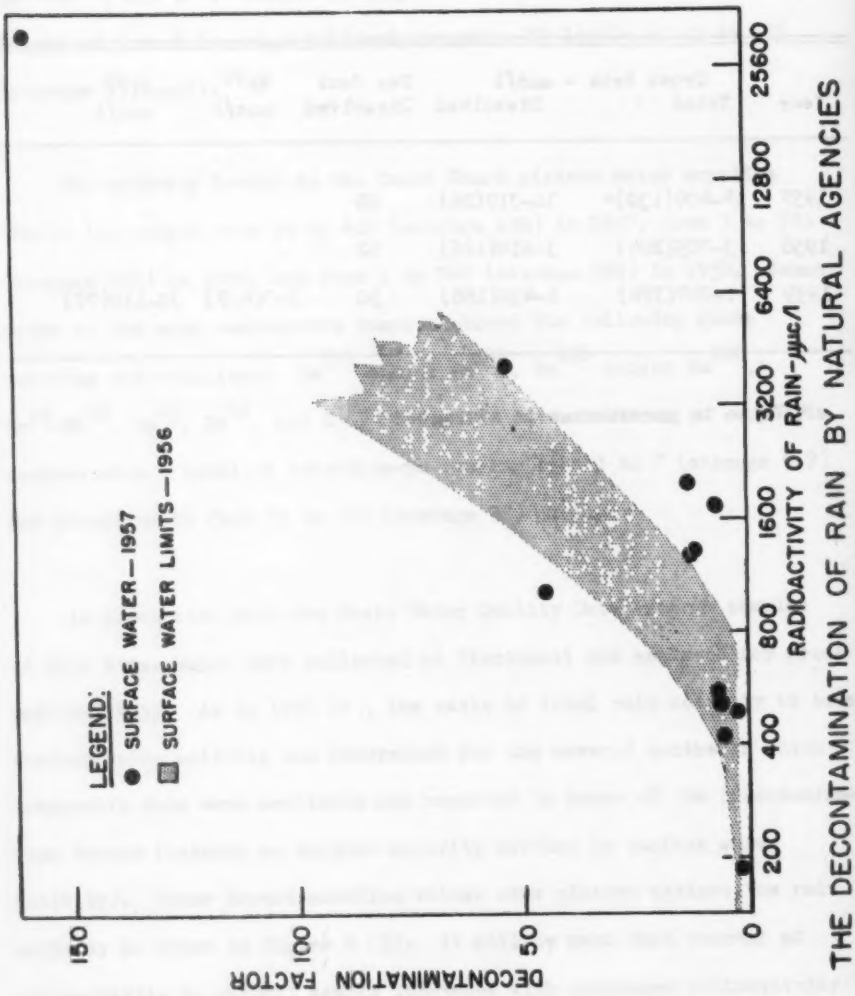
In connection with the Basic Water Quality Data Program samples of Ohio River water were collected at Cincinnati and analyzed for gross radioactivity. As in 1956 (4), the ratio of total rain activity to total surface water activity was determined for the several months in which comparable data were available and reported in terms of the decontamination factor (rainout or fallout activity divided by surface water activity). These decontamination values were plotted against the rain activity as shown in Figure 4 (5). It will be seen that removal of radioactivity by natural agents increases with increased radioactivity of the rain.

Table 15. Coast Guard Water Supplies

Year	Gross Beta - μμc/l Total	Dissolved	Per Cent Dissolved	Sr^{90} μμc/l	Sr^{89} μμc/l
1957	14-409(130)*	10-319(86)	66		
1958	3-705(204)	3-610(126)	62		
1959	1-747(374)	1-430(188)	50	3-7(4.7)	35-130(77)

* Value in parentheses is average.

Fig. 4



THE DECONTAMINATION OF RAIN BY NATURAL AGENCIES

THE DECONTAMINATION OF RAIN BY NATURAL AGENCIES

Food Activity Levels

To obtain an estimate of the average radionuclide values in foods, samples purchased on the Cincinnati market were divided into different categories, mixed in equal amounts, ashed, and analyzed by gamma-ray spectrometry, by radiochemical methods, and by flame spectrophotometry. The classes of materials analyzed included eggs, fresh fruits, fruit juices, pastries, cereals, dairy products, sea foods, meat, fish, leafy vegetables, fresh root vegetables, fresh legumes and corn, instant coffee and tea, nuts - fried and salted, and rice. The vegetables and fruits were washed before processing.

Analytical data including calcium, potassium, and specific radionuclide levels in the various foods are shown in Table 16, and the daily intake (6, 11) values of these food materials are compared in Table 17 with intake values reported by Bryant, Chamberlain, Spicer, and Webb (7). The estimated Cincinnati intake levels of 704 mg for calcium and 8.65 μμc/day for strontium-90 may be compared with British levels of 1199 mg and 6.65 μμc/day, respectively. These levels will obviously vary with the levels of activity in specific food materials ingested in other areas.

The contribution of calcium and Sr^{90} from various food types is summarized in Table 18. From the data it will be seen that although approximately 60 per cent of the calcium comes from milk and milk products in both instances, in Cincinnati only 36 per cent of the Sr^{90}

Table 16. Calcium, Potassium, and Specific Radionuclide Content of Foods*

(Cincinnati - 1958 levels - activity levels at time of analyses)

Sample	Ca (gm/kg)	K (gm/kg)	Sr^{89}	Sr^{90}	Ba^{140}	Cs^{137}**	Ra^{226}
			(μμc/kg)				
Eggs	0.695	1.369	—	3.0	0	2	0.52
Fruits	0.178	2.484	—	3.7	2	63	—
Fruit Juices	0.128	1.791	—	2.1	0	31	0.08
Pastries	0.428	1.307	10	10.6	0	75	0.28
Cereals	0.480	1.368		12.4	0	110	0.12 to 0.60
Dairy Prod.	4.836	—	61	39.0	0	21	0
Sea Foods	0.335	—	0	0.2	0	25	—
Meats	0.172	3.483	0	1.0	0	160	0.10
Leafy Vegetables	0.508	3.404	21	3.9	12	49	0
Root Vegetables	0.273	3.632	47	12.5	3	53	0
Legumes and Corn	0.234	3.132	31	7.5	0	57	0
Instant Coffee	1.900	44.280	31	19	0	1900	—
Instant Tea	0.150	45.320	150	60	—	890	—
Mixed Nuts	0.727	6.975	—	8.2	0	350	2.23
	0.643	6.650	6	8.1	0	210	0.19
Rice	0.196	0.811	—	1.2	0	95	0
Milk (1958 Av)	1.145		55	8.5	24	75	0

* Data made available through courtesy of Dr. G. K. Murthy.

** In ash only.

Table 17. Average Daily Intake by Adults of Strontium-90 in Various Foods

Food	Daily Intake			
	PHS (Murthy) (6, 11)		Bryant et al. (7)	
	Calcium (mg)	Strontium-90 (μμc)	Calcium (mg)	Strontium-90 (μμc)
Eggs	24	0.11	24	0.05
Fruits and Fruit Juices	16	0.31	58	0.87
Pastries	43	1.00	332	0.66
Cereals	10	0.27	11	0.11
Milk* & Dairy Products	410	3.10	667	3.64
Sea Foods or Fish	11	0.01	19	0.02
Meats	51	0.30	27	0.40
Leafy Vegetables	54	0.42	16	0.20
Root Vegetables	50	2.30	25	0.51
Legumes and Corn	20	0.64	5	0.04
Other Foods	15	0.19	15	0.15
Total	704	8.65	1199	6.65

* Average Sr^{90} content of Cincinnati milk for 1958 is 8.5 μμc/l, assuming average intake of 200 gm/day (approximately 200 ml/day) for milk. Higher intake levels are usually indicated for children.

Table 18

Contribution of Calcium and Sr^{90} from Various Food Types

Food Type	PHS Data		British Data	
	Ca mg/day	Sr^{90} μμc/day	Ca mg/day	Sr^{90} μμc/day
Milk Products	410 (58.2%)	3.10 (35.9%)	667 (61.1%)	3.64 (54.7%)
Vegetables	124 (17.6%)	3.36 (38.9%)	46 (4.2%)	0.75 (11.3%)
Cereals and Pastries	53 (7.5%)	1.27 (14.7%)	343 (31.4%)	0.77 (11.6%)
Meats, Eggs, Sea Foods	86 (12.2%)	0.42 (4.9%)	70 (6.4%)	0.47 (7.1%)
Fruits & Fruit Juices	16 (2.3%)	0.31 (3.6%)	58 (5.3%)	0.87 (13.1%)
Others	15 (2.1%)	0.19 (2.2%)	15 (1.4%)	0.15 (2.3%)
Total	704	8.65	1199	6.65

comes from this source; and in Great Britain about 55 per cent. A large part of the Cincinnati Sr^{90} appears to come from vegetables, whereas a considerably smaller amount is taken in from this source in the United Kingdom. Other comparisons can be made from the data shown.

Additional selected data are available on strontium-90 levels in food materials from the California area (8) and on the levels of Zn^{65} found in food materials purchased on the Cincinnati market (9). These data are summarized in Tables 19 and 20, respectively. The data in Table 19 show that table processing, that is, washing of lettuce and paring of carrots, before use has a marked effect on reducing levels of strontium-90 in these food materials. Since 60 per cent of the strontium-90 was removed in washing the lettuce and 87 per cent in paring the carrots, these data imply that the bulk of the activity was surface deposited or surface associated rather than metabolized. Data of this nature are of value, since they indicate the method and, in the case of food processing, will determine to some degree the level of radioactivity in processed food. Since the California situation involved high local fallout, the statements above of non-metabolized activity may not be generally applicable.

Although the concentrations of Zn^{65} observed in the foods listed in Table 20 cannot be considered to constitute a health hazard, they are of interest in pointing to the wide distribution of this radionuclide.

Table 19. Effect of Food Preparation on Radiostrontium Levels

Sample	Treatment	Sr^{90} - μμc/gm - ash		Reduction (%)
		Before Treatment	After Treatment	
Lettuce	Washed	1.33 ± 0.07	0.53 ± 0.10	60
Carrots	Pared	1.47 ± 0.10	0.19 ± 0.09	87

Table 20

Concentration of Zn^{65} in Foods

Sample	Date of Sampling	Zn^{65} - μμc/kg
Sea Food		
Oysters (Chesapeake Bay)	March, 1958; January, 1959	124; 178
Clams (East Coast)	May, 1958	40
Meats[a]	September, 1958	17
Fresh Vegetables[b]	August, 1958	4-12
Eggs	August, 1958	6
Milk	January, 1959	4
Fresh Fruits[c]	August, 1958	3

[a] Equal parts of chicken, lamb, beef, and pork.

[b] Equal parts of lettuce, cabbage, spinach, broccoli, celery, and cauliflower; potatoes, sweet potatoes, carrots, beets, radishes, and turnips; and shelled peas, string beans, shelled lima beans, and corn.

[c] Equal parts of apples, grapes, grapefruit, oranges, peaches, plums, strawberries, and cantaloupe.

Bone Analyses

A joint Atomic Energy Commission-Public Health Service study was initiated recently to measure activity levels in bone samples from the vicinity of the weapons test site. To date, 10 samples have been received from Utah and California. Preliminary findings show strontium-90 levels ranging from 0.2 to 2.7 μμc Sr^{90}/gm bone ash or 0.6 to 12.0 μμc Sr^{90}/gm calcium.

Special Analyses

During the course of some special studies on the Nevada soils following a nuclear detonation (balloon shot - 1957 series), two previously unreported induced radionuclides, scandium-46 and cesium-134, were detected in fallout material (10).

Total Intake

Analytical data on specific environmental radionuclide levels is necessary to estimate the total daily intake of such radionuclides. For example, the daily intake of strontium-90 from a variety of sources has been computed in Table 21. It will be noted that, depending on the source of water supply, the daily strontium-90 intake may vary from approximately 9 to 24 μμc. These data also show that strontium-90 in air represents an insignificant contribution to the strontium intake. It is also obvious that if a person uses a deep well supply, he may not get any strontium-90 through the water route. To estimate actual

Table 21

Strontium-90 Intake - Environmental Sources - Cincinnati*

Source	Radionuclide Activity Level		Daily Intake ml or grams	Intake of Sr^{90} μμc/day
Air	10^{-4} - 1.2×10^{-2}	μμc/M^3	2×10^7 ml	0.002 - 0.24
Water				
Cistern	3 - 7	μμc/l	2,200 ml	6.6 -15.4
Surface	0.3 - 4.7	μμc/l	2,200 ml	0.66 -10.3
Milk	8.5	μμc/l	200 ml	1.7
Food	0.2 -60	μμc/kg	1,025 gm	6.95
Totals	Minimum Values [9.3 - 15.3 μμc/day]			
	Maximum Values [19.2 - 24.3 μμc/day]			

* See notes under table 17.

exposure to man, it is necessary to determine the intake of all other radionuclides and to calculate the exposure due to each. Summation of these exposures will indicate total exposure from ingested sources. To this must be added external exposure from cosmic rays, natural radioactivity, occupational exposure, etc., as well as exposure from radiation producing devices or radioisotopes used in diagnosis and therapy. It is only through such calculations that we can isolate areas where people are receiving higher levels of exposure, can carry out epidemiological investigations, and can reduce environmental levels before they become critical.

SUMMARY AND CONCLUSIONS

Results of findings from network operations as well as special studies by the Public Health Service at the Robert A. Taft Sanitary Engineering Center were reviewed. The use of environmental radioactivity values in estimating the total intake of strontium-90 is indicated by example. All of the data obtained, however, show the need for more knowledge of the factors affecting retention of strontium-90, particularly the effect of stable strontium and calcium. Since strontium-90 can be found anywhere in the environment, human studies can be carried out in which analyses are made of all food intake as well as all body eliminations. Thus, the objections of added radionuclides to carry out such studies are voided, and measurements are made with the radionuclide in the form in which it is taken into the body rather than in some other form.

Extensions of the studies described herein are needed to define areas of higher activity level and permit initiation of the necessary specific control measures before serious dosage or exposure results.

ACKNOWLEDGEMENTS

The assistance of my entire staff is acknowledged in the collection, summation, and evaluation of these data collected since the last Fallout Hearings. Appreciation is expressed to Drs. G. K. Murthy and J. E. Campbell for the use of the data included in Tables 3-8, 10, and 16, and to Drs. Murthy, Campbell, and A. S. Goldin for use of the data in Table 20.

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ATTACHMENT I

ROBERT A. TAFT SANITARY ENGINEERING CENTER

OFFICE MEMORANDUM - U N I T E D S T A T E S G O V E R N M E N T

TO: Those Concerned DATE: January 20, 1959

FROM: Chief, Radiological Health Research Activities
Division of Radiological Health, SEC

SUBJECT: Determination of Gross Radioactivity in Water, Silt, and Biological Materials.

On January 30, 1957, the initial memorandum on this subject (entitled Measurement of Radioactivity in Water, Bottom Silts and Biological Materials) was released. There was considerable interest in the information contained therein and many requests were received for this document.

Additional experience at the Center since the preparation of the original memorandum has suggested several minor modifications in methodology and some modification in the manner of presenting the material. These changes have been incorporated in the current revision.

When programs for the measurement of gross activity in water, biological materials, and bottom muds are developed at local and State levels, the Sanitary Engineering Center will furnish control or check samples for processing by the cooperating laboratories. These reference samples will provide a basis for evaluation of the procedures used and for the uniform reporting and interpretation of counting data.

The Sanitary Engineering Center will provide further details on the techniques and procedures used and will assist in the development of programs relating to the measurement of radioactivity in the environment if such information is desired.

Any comments relative to the application of the methodology incorporated in this material will be greatly appreciated.

Conrad P. Straub

Conrad P. Straub

Attachment

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DETERMINATION OF GROSS RADIOACTIVITY IN WATER, SILT, AND BIOLOGICAL MATERIALS*

1. INTRODUCTION

The naturally occurring elements of the uranium, thorium, and actinium series, and some other elements such as potassium, are radioactive. They emit alpha or beta particles, frequently accompanied by gamma radiation. The fission of nuclear fuel gives rise to a number of elements, most of which emit beta and gamma radiation.

As yet no standard procedures have been developed for the radiological examination of water samples, silts, and biological specimens. The methods outlined below are based on general methods modified, tested, and used by personnel of the Radiological Health Research Activities group, DRH, Robert A. Taft Sanitary Engineering Center, Cincinnati, Ohio.

2. SCOPE AND LIMITATIONS

Measurements of radioactivities of the order of 10 micromicrocuries per liter (10^{-8} μc/ml) or lower are needed to determine the natural background radioactivity in waters and to detect increases

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* - Prepared by RHRA, DRH, Robert A. Taft Sanitary Engineering Center, Public Health Service, U. S. Department of Health, Education, and Welfare, Cincinnati 26, Ohio.

in activity before they reach levels which would interfere with the use of the water. The Committee on Permissible Internal Dose of the National Committee on Radiation Protection has established a provisional maximum permissible concentration of 10^{-7} μc/ml for unknown alpha or beta emitters.* A suitable assay method should be able to detect activities as small as one-tenth of this.

Since no instrument currently available is capable of measuring activities of this level in water directly, some method of concentration is required. The method ordinarily used is evaporation even though radioactive gases and volatile substances may be lost. Isotopes with short half-lives will die out during sample preparation. Within these limitations the method will give an adequate measurement of total activity providing the solids content of the water is not too great. For determination of the activity due to specific elements more elaborate techniques are required.

This method is not intended to be highly accurate. Extreme accuracy in such a method would be deceptive, since the pollutional significance may vary by a factor of 1000 or more if different isotopes are present from time to time.

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* - National Bureau of Standards Handbook 52, U. S. Dept. of Commerce, March, 1953, page 11.

One of the major sources of inaccuracy is the uncertainty in the calibration factor, in particular the self-absorption. This uncertainty for 5 mg/cm^2 thickness of sample solids may lead to an error of probably less than 25 per cent in the beta assay and more than 25 per cent in the alpha assay. If there is sufficient activity to warrant a more accurate determination, a smaller sample can be assayed so that the final thickness of sample solids is 1 mg/cm^2 or less. In this smaller sample, the self-absorption error is reduced, but an appreciable residual error may remain, due largely to uncertainty in backscatter and to non-uniform deposition.

Another source of error is the effect of alpha activity on beta counting. The alpha plateau extends into the beta plateau with a slight slope. As a consequence somewhat more than one count is recorded at the beta voltage for each alpha count. If the alpha count is much lower than the beta count, this error is small. If the alpha count is comparable to the beta count, then the alpha is ordinarily of more pollutional significance, and the resulting error in the beta assay is less important.

In order to obtain a rounded picture of the radioactivity level of a stream, determinations of the radioactivity of the silts and of biological samples are also desirable. The limitations of the method as applied to these materials are comparable to those applicable to water.

2.1 Principles of Sample Preparation: In any radioactivity determination, the nature of the sample presented to the counter must be in conformity with the requirements of the counter. These requirements are that the sample be small enough in area for the detector to observe it efficiently and that it be thin and uniformly spread, for minimum self-absorption. Minimizing self-absorption contributes to counter efficiency and also to the reliability of the correction factor. In addition to these general requirements, an internal counter has specific requirements--the sample should be a reasonable good electrical conductor and must make good electrical contact with the chamber, it must not give off vapors which would change the characteristics of the counting gas (in particular, it must be thoroughly dry), and it must not be dusty or flaky so as to contaminate the chamber.

In practice, a compromise must be made between some of these requirements. For example, a thin sample of small area would require a small volume of sample except for samples of low solids content. By choosing a counter which "sees" a two-inch pan (about 20 sq cm), a solids content of about 100 mg can be tolerated without serious difficulty. Thus, 250 ml of a water with solids amounting to 400 ppm, and correspondingly larger amounts of water of lower solids content, can be used. For maximum sensitivity, it is better

not to go below 250 ml, even if the solids content is considerably above 400 ppm. However, if the activity present is sufficiently high that a smaller sample will give enough counts (see section 4) in a reasonable time, the smaller sample should be used.

Care must be taken to avoid contamination of glassware or other equipment. Glassware is washed with acid and the acid discarded; very dirty equipment is cleaned with disposable pads. Glassware used for samples of high activity should not be used for samples of low activity.

3. COUNTING TECHNIQUES

The disintegration of radioactive atoms results in the emission of alpha or beta particles, which may be accompanied by gamma radiation. The function of the instrumentation is to detect the particles with as high an efficiency as possible.

Most counters operate on the principle of collecting the electrons which are produced by the alpha or beta particles passing through the counter gas. These electrons are collected on a thin wire maintained at a high positive voltage. An internal proportional counter is suggested in this method, since it has been found capable of counting either alpha or beta activity with a high efficiency; it has been tested extensively and found satisfactory.

The details of counting will depend on the specific instrument purchased and will vary in accordance with instructions contained in the operating manual. In general, a counting gas specified by the instrument manufacturer will be used and the instrument will detect either alpha or alpha-plus-beta activity depending on the operating voltage.

3.1 Plateau; Operating Voltage: Counting is done at a voltage where the count rate does not change much for a small voltage change. This plateau region is determined by the following technique.

Using an alpha source, having an area comparable to the largest sample dishes, make a series of determinations of the counting rate versus voltage above and below the approximate alpha operating voltage indicated by the manufacturer and plot the results. There should be a region extending over at least 150 volts where the counting rate is relatively independent of voltage. Choose the alpha operating voltage near the center of this region.

Using a large-area beta source, make a similar series of measurements and determine the beta plateau and beta operating voltage.

3.2 Instrument Operation; Background: In order to assure that counters are operating properly, they are checked two or three times

a day by counting an alpha or beta source of known counting rate.* The counting rate so determined should check the known rate within two standard deviations (2 per cent if the cotal count is 10,000). If the determination is not within this precision, the operating voltage should be checked and, if necessary, a new plateau determination made. If this does not correct the discrepancy, the instrument requires servicing.

Alpha and beta backgrounds are determined by measuring the instrument counting rate at the appropriate voltage with a clean empty counting pan in the chamber. Background should be measured two or three times a day and the average background counting rate calculated. Total background counting time should equal or exceed the longest sample counting time used but too extensive background counting time is of questionable value.

3.3 Sample Counting: Samples are counted by placing them in the counting chamber at the appropriate operating voltage, flushing the chamber to replace the air with counting gas, and recording the counts over a specified time.

The sample placed in the chamber must not unduly affect the electrical properties of the chamber or change the nature of the

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* - The "known counting rate" may be determined by counting the source for a period long enough to give desired precision. A total of at least 40,000 counts (1%) is suggested.

chamber gas. In particular, the sample must be thoroughly dry.

3.4 Calibration of Overall Counter Efficiency: Since no instrument can detect all the radioactive disintegrations occurring in a sample, it is necessary to correct the observed counting rate for this inefficiency.

A number of factors are involved in this loss of counting efficiency. These include geometry, back-scatter, absorption, and self-absorption. The effects of these factors are discussed in detail below.

These factors are constant for a given instrument, sample preparation method, and radioisotope. When the nature of the radioactive material is not known, the factors cannot be determined exactly. The corrections used in this method are based on the assumption that alpha activity is due to natural uranium, and the beta activity to mixed fission products (fallout).

The overall efficiency of counting may be determined, without any knowledge of the efficiency factors involved, by adding a known amount of radioactive material to a counting pan and measuring the counting rate. Since the self-absorption correction depends on the thickness (mg/cm^2) of sample solids, it is necessary to prepare a number of such counting pans containing varying weights of solids.

Calibrated solutions of various isotopes are available from the National Bureau of Standards. Of these, thallium-204 is used most frequently as a calibrating standard simulating fission products, and is therefore recommended for the beta determination. Uranium salts are recommended as calibrating standards for the alpha determination. (Note: 1 mg natural U emits 1520 alpha particles per minute.)

Prepare, in weighed counting pans, several non-radioactive samples of different weights up to 300 mg by processing variable volumes of tap water* or variable weights of appropriate uncontaminated sample solids. Add to each an identical aliquot of uranium or thallium-204 containing about 1000 counts per minute, for alpha or beta calibration, respectively. Add water, disperse the radioactive material uniformly, dry, weigh, and count. Divide the observed net counting rate by the known disintegration rate for each sample to obtain counting efficiency. Plot a graph of counting efficiency versus weight of sample solids as a calibration curve.

3.4.1 Alternate Method: Use of Known Efficiency Factors:

Geometry (G) -- Not all the radiation is emitted in the direction of the detector. The geometry factor measures the

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* - If the solids content of tap water is so low as to make this impractical, fine calcium carbonate may be added before processing.

fraction of radiation emitted in the proper direction. For a small sample in the center of a counting pan in an internal counter, this fraction is 0.50 (50 per cent) and it has been shown experimentally that this factor may be used for a sample spread uniformly over a two-inch diameter counting pan.

Back-scatter (B) -- When a sample is mounted on a metallic counting pan, a fraction of the beta particles emitted into the pan is reflected into the detector. This is called back-scatter. Alpha particles are also back-scattered, but to a much smaller extent.

Absorption -- In instruments in which the sample is outside the detector, some of the alpha and beta particles are stopped in the window of the detector or by the air gap between sample and window, and are not counted. This does not occur in internal counters.

Self-absorption -- Loss of radiation in the sample itself is called self-absorption. Some of the radiations from lower levels are absorbed before they can penetrate through the sample, and so do not reach the detector. Alpha and low-energy beta particles have very low penetrating power and so are especially susceptible to counting loss by self-absorption. The transmission factor (T) measures the fraction of the radiation which is not lost by self-absorption.

Counting efficiency (e) -- The counting efficiency of an internal can be obtained by multiplying together the factors G, B, and T. For alpha activity, e = 0.50 x 1.02 x T or 0.51T. For beta activity of mixed fission products mounted on a stainless steel dish, e = 0.50 x 1.36 x T = 0.68T. The values of T for samples of different thickness are listed in the following table. In general the greater the sample thickness the less reliable is the value of T.

Sample Thickness In mg/cm^2 Giving Stated T

T	1.0	0.9	0.8	0.7	0.6	0.5
Beta*	0	1.4	3.9	6.6	13.5	---
Alpha**	0	0.8	1.6	2.4	3.2	4.0***

* - For mixed fission products.

** - For natural uranium.

*** - For sample thickness above 4 mg/cm^2 divide 2.0 by the thickness, e.g., T = 0.25 for 8 mg/cm^2.

4. STATISTICAL VARIABILITY OF MEASUREMENTS

The variability of any measurement is denoted by the standard deviation, which can be obtained from replicate determinations by well-known methods. There is an inherent variability of radioactivity measurements because the disintegrations occur in a random manner described by the Poisson distribution. This

distribution is characterized by the property that the standard deviation (s) of an average number of events ($\bar{N}$) is equal to its square root or

$$s(\bar{N}) = \sqrt{\bar{N}} = \bar{N}^{1/2} \qquad (1)$$

If a single observation is made, the observed value (N) is taken as equivalent to the true value ($\bar{N}$) so that

$$s(N) = N^{1/2} \qquad (1a)$$

Generally, we are not interested in the standard deviation of the number of counts, but rather in the deviation of the rate (R) (number of counts per unit time):

$$R = \frac{N}{t} \qquad (2)$$

where t is the time of observation, which is assumed to be known with such high precision that its error may be neglected. The standard deviation in the counting rate can be calculated by the usual methods for propagation of error:

$$s(R) = N^{1/2}/t = (R/t)^{1/2} \qquad (3)$$

In practice, all counting instruments have a background counting rate, B, when no sample is present. When a sample is present, the counting rate increases to R_o. The counting rate (R) due to the sample then is

$$R = R_o - B \qquad (4)$$

By propagation of error, the standard deviation of R can be calculated as:

$$S(R) = (R_o/t_1 + B/t_2)^{1/2} \qquad (5)$$

where t_1 and t_2 are the times over which the gross sample and background counting rates were measured, respectively. Practical minimum counting times are 30 minutes or 2500 total counts above background, whichever is less. It is desirable to divide the counting time into two equal periods, so as to check the constancy of observed counting rate. For low-level counting, t_2 should be about the same as t_1. The error thus calculated includes only the error caused by inherent variability of the radioactive disintegration process and should be reported as the "counting error".

4.1 Limits of Error; Confidence Levels: If the standard deviation is multiplied by a constant, limits of error are obtained. For each constant, there is a confidence level, as listed below

Constant	Confidence Level
0.67	50%
1.00	68%
1.64	90%
1.96	95%

There is a 95 per cent probability that the true value lies within $\pm$ 1.96 standard deviations of the observed value. This 95 per cent confidence level is used throughout this presentation.

5. APPARATUS AND EQUIPMENT

5.1 Counting Instrumentation: Internal proportional counter chamber, preamplifier, scaler, register, and timer, with suitable counting gas supply. This may be purchased as separate units or as a complete assembly. The chamber shall accept sample dishes approximately 2 inches in diameter and 1/4 inch high.

5.2 Counting Pans: Stainless steel, approximately 2 inches in diameter and 1/4 inch high. Other suitable metal pans may be used with allowance made for the proper backscatter factor.

5.3 Radioactive Sources: A suitable alpha source consists of uranium oxide (U_3O_8) plated on a metal planchet of not less than 45 mm diameter.

A suitable beta source consists of a uranium source as that above which is covered with 0.001 inch aluminum foil.

5.4 Filters: Cellulose acetate membrane filters (Millipore HA or equivalent), 45-50 mm, in diameter and associated filtering equipment.

6. SAMPLE COLLECTION

6.1 Water: Representative samples of at least one liter in size shall be collected in polyethylene or resistant glass bottles. No acid or preservative shall be added to the samples.

6.2 Silts: Representative silt samples of not less than one-half pint shall be collected in large-mouth, water-resistant containers.

6.3 Biological Material: Fish and other large specimens (crayfish, snails, insect larvae, etc.) -- Wash free of any extraneous matter, such as sand, blot dry, and weigh in the field. Glass or plastic containers are suitable for collection. Refrigeration rather than preservatives is recommended. Formalin or alcohol may be used as preservatives on samples to be processed for total activity providing the preservative is processed as part of the sample.

Algae, plankton, very small specimens -- Collect samples of two grams (or more, if available) free from extraneous matter, blot dry, and weigh in the field. The remarks in the previous paragraph concerning preservation also apply to these samples.

7. SAMPLE PREPARATION

7.1 Water Samples: The size of the sample to be counted is limited by the solids content, as discussed in section 2.1. For good sensitivity, the minimum sample size should be 250 ml for most surface waters, and maximum volumes as indicated in the following table:

Solids Content	Maximum Sample Size
ppm	ml
>400	250
300-400	300
200-300	500
100-200	1000
50-100	2000
<50	2500

7.1.1 Non-Filterable (Suspended) Radioactivity: Filter a sample of suitable size through a membrane filter with vacuum. Retain the filtrate for use in section 7.1.2. Wash the filter and solids with distilled water and suck air through to dry. Transfer filter and solids to a weighed counting dish, saturate with ethyl alcohol, and burn off the alcohol. If any of the filter remains, add a second portion of ethyl alcohol and burn it off. Flame the dish for a few seconds, then cool, weigh, and store in

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a desiccator until ready to count. Postpone counting for at least four hours to permit decay of short-lived natural radioactivity.

7.1.2 Filterable (Dissolved) Radioactivity: Transfer the filtrate from 7.1.1 to a Pyrex beaker and evaporate to near dryness without boiling (a gentle stream of filtered air may be passed into the beaker to speed up the evaporation). Transfer the residue to a weighed counting pan with the aid of a wash bottle and a rubber or plastic policeman. Rinse the beaker with a few ml of distilled water (plus not more than 1 ml of 1 N HCl), police down the beaker walls and transfer the rinse liquids to the counting pan, dry (103°C), weigh, and store in a desiccator until ready to count.

7.2 Silt Samples: Remove any extraneous matter, such as gravel or bits of vegetation, from a representative portion of the sample, and, if necessary, air-dry and grind to a homogeneous powder. Make a thin slurry with distilled water and transfer enough sample to each of three counting pans to give dried weights of about 50, 100, and 200 mg, respectively. Dry in an oven at 103°C, weigh, and store in a desiccator until ready to count. The 50- and 100-mg samples are alpha-counted; the 100- and 200-mg samples are beta-counted.

If sample solids are powdery, treat them with one ml of a solution of lucite in acetone (0.5 g per 100 ml), and enough additional acetone to wet the sample completely, re-dry, and count.

7.3 Biological Materials: Process a sample of 2 to 10 grams, if available. Remove any visible extraneous matter, such as silt, from the specimen by washing with distilled water, blot dry, and re-weigh to determine live weight. Place the sample in a weighed porcelain or silica dish (or in a counting pan, if it is sufficiently small), and determine dried solids (103°C) and fixed solids (600°C). Distribute up to 200 mg of fixed solids in a counting pan. Dry (103°C), cool, weigh, and store in a desiccator for counting. Note: If the sample solids are powdery, treat them with lucite as described in section 7.2.

7.3.1 Wet-Ashing Method: Some biological materials, especially those containing fatty tissues are difficult to process according to section 7.3. An alternate method is to digest 2 to 10 g of sample in a porcelain or silica dish with 6 N nitric acid first at room temperature until liquified and subsequently at a boiling temperature. Evaporate to dryness and ignite at 600°C. Cool, weigh, transfer known aliquots of up to 200 mg to counting pans, and proceed as in section 7.3.

8. COUNTING PROCEDURE

8.1 Alpha Counting: Count the sample at the alpha operating voltage for a time sufficient to give the necessary precision, as discussed in section 4.

8.2 Beta Counting: Count the sample at the beta operating voltage for a time sufficient to give the necessary precision, as discussed in section 4.

9. CALCULATIONS AND REPORTING

General calculations applicable for the assaying of gross alpha or gross beta activity in any counter are as follows:

Let R equal R_o-B or the net counting rate of a sample (equation 4, section 4).

Let t equal the time in minutes for counting.

e " the efficiency of counter in cpm/dpm = GBT for internal counter (section 3.4.1)

E " the error of count at the 95% confidence level, i.e., 1.96 standard deviations from equation 5, section 4.

By definition, 2.2 dpm = 1 μμc.

Then the radioactivity (μμc/sample) equals R/2.2e and the counting error at the 95% confidence limit (μμc/sample) is

$$= 1.96\, s(R)/2.2e \qquad \text{(section 4.1)}$$

$$= 2.2\, e \frac{1.96\,(R_o+B}{t}$$

9.1 Alpha Activity of Water, Silt, or Biological Material: Report the alpha activity in water and the counting error in terms of μμc/l by dividing the μμc/sample by the sample volume in liters. Similarly, calculate and report the alpha activity in μμc/gram of moist biological material or per gram of moist and per gram of dry silt.

9.2 Gross Beta Activity When Alpha Activity Is Insignificant: For samples having an alpha activity less than one-half of the beta counting error (both in cpm), calculate and report the gross beta activity and counting error in μμc/liter of water or fluid, μμc/g of moist (live weight) biological material, or μμc/g of moist and per gram of dry silt according to sections 9.and 9.1 disregarding the slight amount of alpha activity.

9.3 Gross Beta Activity When Alpha Activity Is Significant: For samples having an alpha activity greater than one-half of the beta counting error (both in cpm), deduct the net alpha cpm from the net beta cpm to give the corrected beta cpm. Proceed as in 9.2 to calculate and report the beta radioactivity in μμc/liter of water, μμc/g of moist biological sample or per gram of moist and dry silt. When the alpha activity represents a small fraction of the beta activity, a rough approximation of the beta counting error consists of the gross beta counting error. Where greater precision

is desired, such as when the alpha activity is a substantial fraction of the gross beta activity, the beta counting error = $(E_a^2 + E_b^2)^{1/2}$ where E_a = alpha counting error,

and E_b = beta counting error.

9.4 Miscellaneous Information To Be Reported: In reporting radioactivity data, it is important to adequately identify the sample, sampling station, date of collection, volume of sample, type of test, type of activity, type of counting equipment, standards used for calibration, time of counting (particularly, if short-lived isotopes are involved), weight of sample solids, and the kind and amount of radioactivity. Insofar as possible, the data should be tabulated for ease of interpretation and repetitious items should be incorporated in the table heading or in footnotes. Unless particularly inconvenient, quantity units should not change within a given table. For low level assays, where the counting error represents a significant fraction of the measurement, it should be reported to assist in the interpretation of results.

9.5 Reporting Form: An example of the form of reporting is as follows:

Beta Radioactivity of Surface Water

for "A" River 1959

Sampling Sta.	Date Sampled	Date Counted*	Solids** Susp. ppm	Solids** Dissol. ppm	Beta Activity*** Susp. μμc/l	Beta Activity*** Dissol. μμc/l	Beta Activity*** Total μμc/l
1	6/10	6/15-16	300	200	30	60	90
1	7/20	7/22	800	210	400	80	480
2	7/20	7/22	20	220	25	60	85
3	7/20	7/22	20	230	25	60	85

* Date of counting suspended and/or dissolved activity
** Based on 500 ml samples and membrane filters
*** Counting time 30 mintues, thallium-204 standard and counting error at the 95% confidence limit of less than 10 μμc/l except in (a) the error of which is 25 μμc/l.

10. APPENDICES

10.1 Methods Using Other Instrumentation: The instruments described in this section are less sensitive than the internal proportional counter described earlier or our experience with them is too scant for adequate evaluation. In addition, the instruments may not be adequate to both the alpha and beta determination. However, in order to encourage cooperation by laboratories which may have such instrumentation, methods for use with them are discussed briefly.

In general, the method as described for internal proportional counters is applicable with changes as required by the instruments. For example, most end-window Geiger tubes are designed for use with samples one inch in diameter, and counting dishes, and alpha and beta sources of suitable size will have to be chosen. Use of smaller dishes will increase self-absorption losses, and samples may have to be reduced in size with consequent loss of sensitivity. A brief description of other instrumentation follows.

Scintillation Counter - The (alpha) scintillation type detector consists of a photomultiplier tube equipped with an alpha-sensitive phosphor and its circuitry encased in a light-tight housing. Since the output pulses are too small to trigger a conventional scaler, an amplifier is required. Some sort of sample mount permitting reproducible positioning of samples must be used.

Photomultiplier tubes may be permanently damaged by excessively high voltage. For this reason, it is necessary to determine the plateau region very carefully and to stop as soon as a suitable operating voltage 50-75 volts above the lower end of the plateau is determined. Care must be taken to avoid exposure of photomultiplier tubes to light with the high voltage on.

Scintillation counters have been widely used for alpha counting; from our very limited experience with them, they seem to have counting efficiencies somewhat lower than internal proportional counters, with comparable backgrounds. Since the sample is external to the detector, its electrical properties or any tendency to emit vapors do not interfere.

Scintillation counters have been used for beta counting, with anthracene crystals or organic plastic scintillators replacing the alpha-sensitive phosphor. The Sanitary Engineering Center has not had any experience with this technique, and therefore cannot specify or recommend any procedure.

Geiger-Müller Tube - The detector of a Geiger-Müller unit consists of a Geiger-Müller tube having a thin (< 2 mg/sq cm) window to permit entrance of particles, with a sample mount to permit reproducible positioning of samples. These are usually inside a lead shield one or two inches thick to reduce the background

radiation. No amplifier is required since the G-M tube produces a pulse large enough to operate a conventional scaler directly.

Geiger-Müller detectors have the advantage of simplicity in not requiring an amplifier unit and, since the sample is external, they have no counting difficulties arising from the electrical or chemical nature of the samples. On the other hand, they are almost insensitive to alpha particles and have only ten to twenty per cent of the beta efficiency of an internal counter, and somewhat less than this for beta particles of low energy. This disadvantage is only slightly offset by the lower background of these tubes. Geiger tubes, like photomultiplier tubes, may be ruined by application of excessively high voltage, and so the precautions necessary in determining plateaus of scintillation counters are also applicable to them.

Since they cannot be used in alpha determinations and are relatively insensitive in the beta determination, Geiger-Müller units cannot be recommended for determining low-level radioactivity in water or stream samples. As an additional unit, they can be very useful in determining energy of beta emitters in cases where higher levels of activity exist.

10.2 Suggested Reading Material:

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10.3 Equipment Specifications:

Scaler:

Input Sensitivity 0.25 volts

High voltage supply 0.250C volts, with regulation such that 1% change in line voltage results in about 0.01% change in output.

Scaling stages either (a) binary, 8-16-32 (higher scales convenient but not essential), (b) decimal, scale of 100 or higher.

Timer adjustable to 1 sec or 0.01 minute; longest range at least 1 hour (may be purchased separately).

Register, electrical or mechanical capable of being driven (may be purchased separately).

Suitable scalers are available from a large number of distributors. See, for example, Buyers Guide in November 1958 Nucleonics.

Internal Proportional Chamber-Preamplifier Unit:

Note: Since it is very desirable that the chamber and preamplifier be matched to each other, they are listed together, although separate units may be satisfactory.

Large enough to accept sample dishes two inches in diameter and 1/4 inch high.

Able to count alphas in presence of betas and also alpha-plus-beta.

Alpha background less than one cpm.

Beta background less than 75 cpm.

Output pulse at least 0.25 volts.

Chamber should be easy to decontaminate.

Distributors known to us. (1) (2) (3) (4)*

Counting Dishes:

Stainless steel, 18-8, able to withstand nitric acid and heating to 600°C.

Two-inch diameter and 1/4 inch high (approx. dimensions).

Distributors known to us. (2) (3) (5)*

Radioactive Sources:

Uranium oxide, plated, 45-50 mm active diameter.
Suggested activity (50% efficiency) 10,000 cpm.

Distributors known to us. (6) (7)*

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* - Numbers in parentheses refer to attached distributor list

Filters:

Cellulose acetate, membrane filter type

Pore size less than 0.6 micron

Diameter 45-50 mm

Distributors known to us (8) (9)*

Scintillation Detector Assembly:

Photomultiplier tube, Dumont #6292, RCA #5819, or equivalent, with tube base voltage divider

Pre-amplifier

Phosphors which are sensitive to the type of radiation (alpha or beta) to be measured

Note: It is preferable that the above units be furnished as a complete assembly in order that they be properly matched. A pre-amplifier built into a scaler is satisfactory provided it is adequately sensitive, otherwise a separate amplifier will be necessary. Background levels on such equipment should be less than 1 cpm for alpha activity and less than 75 cpm for beta activity. Varied phosphor materials are available and can usually be interchanged in the same assembly. Those most commonly employed are (1) for alpha measurement, zinc sulfide (silver activated) and (2) for beta measurement, anthracene or organic plastic scintillators.

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* - Numbers in parentheses refer to attached distributor list

Scintillation detectors including phosphors are available from a large number of distributors, see Buyers Guide, November 1958, Nucleonics

Geiger-Müller Tubes:

End-window type, window diameter preferably an inch or more
Self-quenching
Window thickness 2 mg/cm^2 or less
Plateau of at least 100 volts with a slope of not more than 3% per 100 volts

Tube life at least 10^8 total counts

Available from a large number of distributors, see Buyers Guide, November 1958, Nucleonics

Shields (For background reduction with Geiger-Müller Counters):

Wall thickness one inch of lead or equivalent
Inner sleeve of a low atomic number material (iron, brass, or aluminum) next to lead

Equipped with mount for counter tube

Hinged door providing easy access for changing samples

Available from a large number of distributors, see Buyers Guide, November 1958, Nucleonics

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* - Numbers in parentheses refer to attached distributor list

Equipment Distributors:

Note: This is not intended as a complete list of suppliers nor to indicate any preference for the companies listed over unlisted ones. It is merely a list of distributors with whom we are familiar.

Companies whose names are followed by (c) carry a fairly complete line of radioactivity instrumentation.

1. Baird Associates - Atomic Instrument Co. (c)
   Cambridge 38, Massachusetts

2. B. J. Electronics, Detectolab Products
   Santa Anna, California

3. Radiation Counter Laboratories, Inc. (c)
   Skokie, Illinois

4. Nuclear Measurements Corporation, Indianapolis, Indiana

5. N.R.D. Instrument Company, St. Louis, Missouri

6. U. S. Radium Corporation, Morristown, New Jersey

7. Canadian Radium and Uranium Corporation, New York, N. Y.

8. Millipore Filter Corporation, Watertown, Massachusetts

9. Schleicher and Schuell, Keene, N. H.

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10. Tracerlab, Inc. Boston 10, Massachusetts (c)

11. Nuclear Instrument & Chemical Corporation, Chicago, Illinois (c)

12. Radiation Instrument Development Laboratories, Chicago, Ill. (c)

13. Technical Associates, Burbank, California

14. The Atomic Center, New York, New York

15. Berkley Division of Beckman Instruments, Inc., Richmond, California.

Revised: January 15, 1959.

ATTACHMENT II

The Determination of Gross Beta Activity of Airborne Particulate Matter - Thin-Window Proportional Counting of Whole Filter Samples

Apparatus:

1. Air particulate samples are collected on 8 x 10 inch glass fiber filters (Mine Safety Appliance, Pittsburgh, Pa., Cat. No. CT-75428), by means of a high volume (40-60 cfm) blower. The dust particles are deposited on an area 7 x 9 inches.

2. A large area (7.5 inch diameter) proportional gas-flow counting chamber and preamplifier (Nuclear Measurements Corporation, Indianapolis, Ind., Model PCC-12) was converted to a thin-window (Mylar) counter, connected to a conventional scaler and timer (Nuclear Chicago Model No. 181A decade scaler and Model T1 timer) and used for counting. The filter sample, placed on a sliding tray (which replaced the piston), was centered beneath the counting chamber 5mm below the thin-window. With this arrangement the ends of the filter sample overlap and the slides underlap the window diameter which has an area of 44.19 sq inches.

A standard source (secondary standard) of uranium oxide* plated on stainless steel and covered with 8 mg/cm^2 of aluminum foil is used to determine the voltage plateaus and check instrument performance. Standard solutions of thallium-204** are used to determine the counting efficiency of an instrument.

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* U. S. Radium Corporation or equivalent

** National Bureau of Standards, Department of Commerce, Washington 25, D. C.

Instrument Calibration

Prior to calibration and counting, the counting chamber is routinely flushed with counting gas for about an hour. The secondary standard of uranium oxide*** is centered under the counting chamber window, a distance of 5 mm and counted for 1 minute at a center wire voltage of 1500 V. and up in increments of 50 V. until the length and slope of the plateau has been defined. A counting rate of less than 6 per cent variation for a 200 V. change in operating voltage is considered a satisfactory plateau. The voltage in the middle of the plateau is selected for subsequent operation. The plateau is determined periodically (every second or third week) to adjust the operating voltage if necessary.

The stability of the instrument is further checked at the operating voltage by counting standard and background, morning, noon, and at the close of the work day. The average of two successive background counting rates is deducted from observed sample counting rate to obtain net sample counting rate.

Calibration of Counter Efficiency

A source of thallium^{204} having an area of 18.1 cm^2 was counted in an internal proportional counter having 2π geometry, a backscatter factor of 1.31 or an overall efficiency of 0.655 cpm per dpm. The same source centered 5 mm below the thin-window (7.5" diam.) counted 86 per cent of the internal proportional counting rate. Thus the efficiency of the thin-window counter under these conditions is 0.86 x 0.655 or 0.563 cpm per dpm.

*** Counting rate of 10,000 ± 200 cpm.

A point source of thallium^{204} (2mm diam.) was placed on the tray and at various radial distances from the center of the window and out to beyond the periphery of the window in four directions. The plotted results of these four sets of data indicated excellent agreement for a given radial distance but a diminution of the counting rate as the source approached the window periphery.

The results of these tests, summarized in Table A-1, show the effective counting area to be 41.90 square inches as compared to the total sample area of 63 square inches. Thus, the equivalent of 66.5 per cent of the sample area is counted.

TABLE A-1: Relative Counting Rate and Effective Area of a Point Source Positioned Radially In A Thin-Window Counter

Radial Distance from Center		Annulus Area	Relative Counting Rate	Effective Area
Inches		Sq. In.	%	Sq. In.
0	-2	12.57	100	12.57
2	-2.5	7.07	98	6.93
2.5	-3.0	8.91	96	8.55
3.0	-3.25	4.91	91	4.47
3.25	-3.50	5.30	82	4.35
3.50	-3.625	2.80	72	2.01
3.625	-3.75	2.84*	57	1.62
3.75	-4.25	7.0*	20	1.40
				41.90

* Corrected for underlap.

An estimation of the filter absorption was obtained from concurrent collections of air particulates on glass fiber filters and membrane filters at equal filtering rates. The membrane filters were ignited to remove the possibility of filter absorption while the glass fiber filters were treated with conducting fluid and air dried prior to counting both types of samples in an internal proportional counter.

From 88.7 to 92 or an average of 90.3 per cent of the membrane filter activity was recovered on glass fiber filters. Thus assuming equal efficiencies for equal rates of collection, the filter absorption factor is 0.903.

The self-absorption factor in internal counters has been determined with 5-day, 1-year, and 2.5-year old bomb debris. The age of the bomb debris and possibly a variable chemical composition of local dusts changes the transmission of radiation through sample solids as shown in Table A-2.

Table A-2: Transmission Of Mixed Fission Products Through Sample Solids.

Sample Solids	Percentage Transmission (self-absorption) of MFP			
mg/63 sq. in.	5-day	1-year	2.5-year	Average
200	96.5	99.5	100	99
400	93	97	99	96
600	90	94	94	93
800	86.5	90.5	89.7	89
1000	83.5	87	86.3	86
1200	80.3	85	82.7	83
1600	74	80	78.0	77
1800	71.5	78.0	76.0	75.2
2000	69.3	76.2	74.8	73.4
2400	65.5	73	72.3	70.3

Recapitulating the factors for efficiency and conversion to micro-microcuries per cubic meter, we have:

$$\mu\mu c/M^3 = dpm/2.22V = cpm/0.563 \times 0.655 \times 0.903 \times 2.22\ VT$$

$$= 1.34\ cpm/VT$$

where V = Volume of sample in cubic meters and

T = transmission (self-absorption) as shown in Table A-2 divided by 100.

Sample Counting

The samples are counted for two 2-minute periods usually upon receipt and again after 4 to 7 days. If the rate of decay is relatively low, indicating fission products formed at least 10 days or more before the first count, the sample counting rates are plotted on semi-log paper and extrapolated back to the time of sample collection. On the other hand, if a rapid rate of decay is observed, the counting rates are plotted on log-log paper in such a fashion that the Hunter and Ballou decay factor of -1.2 or somewhat less was obtained. The counting rate is then extrapolated to time of collection and converted to gross beta activity in $\mu\mu c/M^3$ according to the formula given in the preceeding section.

Estimate Of The Reliability Of Data

The reliability of data depends on various factors, such as the accuracy of the volume of air filtered, the filter characteristics, the age of the nuclear debris when first counted, the number of days from collection to date of first count and the counting error which is a function of the observed sample counting rate and the instrument background ca. 500 cpm. For fresh nuclear debris of less than 5 days of age and relatively high counting rate, it is possible that the extrapolated value could be in variance by a factor of 2 because fractional parts of a day are not considered. For activity having an age of 10 or more days, the determination should be within 30 percent of the correct value. For low activity in the order of 1000 cpm/sample the raw counting error at the 95 percent confidence level may be estimated to be $\frac{1.96}{2}\ (1500 + 500)^{1/2} = 4.4$ percent, whereas, the error of a sample counting 500 cpm or equal to the background would be $\frac{1.96}{2}\ (1000 + 500)^{1/2} = 38$ cpm or 7.6 percent.

ATTACHMENT III

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Measurement of Low-Level Radioactivity in Water

Lloyd R. Setter and Abraham S. Goldin

A paper presented on Dec. 15, 1955, under the sponsorship of AWWA, at the EJC Nuclear Eng. & Science Congress, Cleveland, Ohio, by Lloyd R. Setter, In Charge of Radiological Investigations, and Abraham S. Goldin, In Charge of Radiochemical Methods, both of the Radiological Health Program, Robert A. Taft San. Eng. Center, USPHS, Cincinnati, Ohio.

DEVELOPMENTS in the useful application of nuclear energy and its byproducts have focused increasing attention upon the public health problems connected with the pollution of water by radioactive material. Liquid wastes from the operation of nuclear reactors, wastes from the uses of radioisotopes, and fallout from the detonation of nuclear weapons already have added quantities of radioactivity measurable above the natural or background radioactivity in water. The USPHS is collaborating with the AEC and other agencies in delineating the problems and assisting in their solution. One of the responsibilities of the Robert A. Taft Sanitary Engineering Center is to investigate and develop methods for the measurement of radioactivity in water, sewage, and industrial wastes. The purpose of this article is to describe one reliable method.

Permissible Radioactivity Levels

The levels at which radioactive materials in water may present a health hazard are extremely low. According to the National Committee on Radiation Protection (*1*), the maximum permissible concentration of radium-226 in drinking water is 40 micromicrocuries per liter ($\mu\mu$c/l), while for strontium-90 it is 800 $\mu\mu$c/l. These levels of radium and strontium radioactivity represent only 4×10^{-8} and 5×10^{-9} mg/l, respectively. For either alpha or beta activity of unknown source, the maximum permissible concentration is only 100 $\mu\mu$c/l, or 222 disintegrations per minute per liter.

The maximum permissible concentrations (MPC) of the most hazardous beta-emitting isotopes (*1*) are as follows (in μc/l): strontium-90 (including daughter product yttrium-90), 0.0008; iodine-131, 0.03; strontium-89, 0.07; copper-64, 0.1; phosphorus-32, 0.2; calcium-45, 0.5; argon-41, 0.5; and iridium-192, 0.9. Sometimes these concentrations must be modified to allow for possible damage to the digestive tract by radioactive materials which are not absorbed by the body (*2*). To detect possible increases in the radioactivity of waters at an early stage, methods of assay must be able to detect and to measure, with a fair degree of accuracy, levels well below the maximum permissible concentration.

Because of the wide range of maximum permissible concentrations, the hazard of contaminated water can be evaluated accurately only by determining the concentration of specific isotopes. In water contaminated with young mixed fission products, for ex-

ample, a very large portion of the total activity is caused by short-lived isotopes of low ingestion hazard, and a relatively high concentration of gross beta activity may, therefore, be tolerable. Thus, the AEC has stated (*3*) that a drinking water contaminated to 5 μc/l by 3-day-old mixed fission products would be safe for continued use. In such a water, although the gross radioactivity decreases rapidly, falling to 0.3 μc/l in 30 days, the change in the hazard is insignificant, as the more hazardous radioisotopes have slow decay rates.

Low-Level Counting

Although gamma radiation is much more penetrating than other forms, it is usually detected with low and variable efficiencies; furthermore, some important isotopes do not emit gamma radiation. Alpha radiation is entirely absorbed by less than 0.1 mm depth of water and (depending upon the energy) beta radiation is absorbed by 1 cm or less of water. As a consequence, it is necessary in low-level assays to circumvent water absorption by evaporating the water or removing the radioactivity by precipitation or comparable means. Even after the separation of the radioactivity from water by evaporation or precipitation, there remain sample solids which absorb radiation. To minimize this loss (self-absorption) the residues are deposited in thin layers for counting. The volume of the sample taken depends upon the radioactivity content, the solids content, and the area of the counting dish.

The small amount of radioactivity being measured makes it necessary to use an instrument which is highly efficient in detecting the radiations from the sample. This efficiency depends upon four factors:

1. *Geometry,* or that fraction of the radiation which shines into the sensitive volume of the detector
2. *Backscatter,* or that fraction of the radiation which is reflected into the sensitive volume of the detector
3. *Absorption,* or that fraction of the radiation which is absorbed before reaching the sensitive volume (includes self-absorption, window absorption, and air absorption)
4. *Counter efficiency,* or that fraction of radiation entering the sensitive volume which is counted.

It is obvious that the overall efficiency is greatest with high geometry, high backscatter, high counter efficiency, and low absorption losses. Each of these factors must be known in order to convert the results into curie values.

Instrumentation

A critical study of methods for low-level beta assay indicated disadvantages inherent in conventional end-window counting (*4*). These disadvantages were: [1] low and variable overall efficiency (5–10 per cent) in counting beta radiations (very low overall efficiency for beta activity of low energy); [2] variation of counter geometry as a result of difficulty in obtaining uniform deposition of radioactivity; and [3] high self-absorption losses for a given volume of sample. An additional instrument is required for assaying alpha activity, thus involving an important cost factor in small laboratories.

The method selected by the Radioactivity Investigations Unit of the Sanitary Engineering Center (*5*) utilizes an internal proportional counter which accepts dishes or planchets 2 in. in diameter with a maximum height of $\frac{3}{8}$ in. Compared to the end-window

counter, approximately 4 times as much sample volume can be used with the same self-absorption loss. Positioning and uniform deposition of the sample are less important, as the sample is placed within the counter chamber. The counter detects alpha radiations and all but extremely low-energy beta radiations. Excluding self-absorption, 50 per cent of the alpha, and about 65 per cent of the beta disintegrations are recorded. The efficiencies are lowered by self-absorption losses. For 2-year-old mixed fission product in 100 mg of sample solids, self-absorption reduces the beta efficiency from 65 to 48 per cent.

Alpha activity is counted, without beta interference, by adjusting the voltage to an appropriate level. Combined alpha and beta activity is counted at a voltage suitable for beta counting. Somewhat more than one count is recorded for each alpha particle, but, because the beta activity greatly exceeds the alpha activity, this apparent disadvantage is not usually serious.

The beta instrument background of the unshielded proportional counter varies from 50 to 65 counts per minute (cpm). During a single working day, the background rarely varies more than ± 5 per cent, which is within the expected statistical variation. Although the level of background is higher than that of shielded end-window counters, this disadvantage is of minor importance compared to the overall high detection efficiency of the internal proportional counter (*6*, *7*).

Contamination of the counter chamber is rare, but, when it occurs (as evidenced by high background counts), it can readily be corrected by cleaning the chamber with a jet of clean compressed air, wiping with lens paper, or cleaning with a metal-polishing compound.

Preparation of Samples

Most of the studies of the Sanitary Engineering Center have been concerned with the uptake of radioactive wastes by sewage treatment processes, the chemical treatment of radioactive aircraft wastes, the distribution of radioactive rainout in surface waters, and amounts of natural radioactivity in surface and well waters. In each of these studies it has appeared desirable to differentiate between the insoluble and soluble radioactivity—that is, between that portion which is particulate or associated with particulate matter and is retained on molecular filter membranes and that portion which passes such filters. For this reason, the insoluble matter in an appropriate sample of 50–1,000 ml or more is collected on a filter disc which is then transferred to an aluminum dish (bottle cap) 48 mm in diameter. After drying and weighing, the sample is counted. Using this method, the insoluble activity of water samples can be ready for counting in less than 30 min. As a very high percentage of rainout radioactivity of young (1–10 day) fission product is removed by filtration, this single determination becomes a quick estimate of the magnitude of radioactive rainout.

The filtrate or soluble activity is determined by evaporating an appropriate sample volume (usually 250 ml), containing preferably less than 100 mg of solids, in a pyrex beaker. The residue is transferred to a weighed aluminum dish with the aid of a few drops of 1-*N* HCl and 10–15 ml distilled water, dried in an oven at 103°C, weighed, and counted.

Precision of Analysis

In assays of radioactivity in water and wastes, the precision of analysis is dependent upon the collection of rep-

resentative samples, the care taken in sample preparation, the variability of self-absorption losses as a result of variable radiation energy, and the reproducibility of counting random radioactive disintegrations. The latter (usually referred to as the "statistical error of counting") is important in deciding how long a sample should be counted to secure the desired degree of reproducibility of counting. For very low radioactivity levels, the counting error may be allowed to become a substantial fraction of the total error to secure speed of analysis. Ordinarily, at the Radioactivity Investigations Laboratory, the longest practical period for counting consists of two 16-min periods whose results agree within counting statistics.

As an example of the kind of precision attainable, replicate 250-ml samples of a cistern water were evaporated in 250-ml pyrex beakers. After the dry solids were transferred to counting dishes, the replicate beakers were washed with concentrated hydrochloric acid and rinsed with water to determine additional activity recoverable from the empty beakers. Similar replicate tests were made on a turbid rain water, and on a rain water containing settleable radioactivity. The results are presented in Table 1. The table shows that, for total and dissolved activity at a level of 700–1,000 $\mu\mu c/l$ of cistern water, the standard deviation was 17–30 $\mu\mu c/l$. The ratio of the standard deviation to the mean (coefficient of variation) was 2–4 per

TABLE 1

Precision of Radioactivity Analyses

Type of Sample	Sample No.	Beta Activity—$\mu\mu c/l$			Standard Deviation $\mu\mu c/l$	Coef. of Variation %	Error* %
		Min.	Max.	Mean			
Cistern water							
Suspended activity	1–5	117	158	134	20	15	8–11
Dissolved activity	1–5	713	781	746	30	4	3–6
Total activity	1–5	855	898	880	17	2	
Total activity	6–20	869	936	904	25	3	3–5
Total activity	1–20	855	936	898	21	2	
Acid wash†				4			
Turbid rain							
Coated beakers‡	21–26	1,830	2,330	2,047	175	8	4–5
Uncoated beakers	27–32	1,775	2,350	2,137	215	10	4–5
All beakers	21–32	1,775	2,350	2,092	194	9	4–5
Acid wash†				18			
Rain with sediment							
Radioactivity	33–38	14,300	19,600	16,800	2,075	12.3	
Acid wash†				30			
Radioactivity	33–38	43.7§	48.2§	45.7§	1.6§	3.5	1.8–2.1
Total solids	33–38	314‖	430‖	368‖	17‖	4.6	

* Statistical counting error at 95 per cent confidence level.
† Average activity recovered by concentrated HCl from empty beakers used for evaporations.
‡ Treated with water-repellent silicone compound.
§ In $\mu\mu c$/mg of solids.
‖ In ppm of total solids.

cent, as compared to the statistical counting error of 3–6 per cent at the 95 per cent confidence level. The analyses of the suspended radioactivity at a mean level of 134 $\mu\mu$c/l showed a similar standard deviation but a higher coefficient of variation (15 per cent). It is interesting to note that an average of 4 $\mu\mu$c/l or 0.5 per cent of the accountable radioactivity could be recovered from the empty beakers by a stringent acid wash.

The turbid rain water contained some organic floc which made representative sampling more difficult. Half of these samples were evaporated in pyrex beakers treated with a water-repellent silicone compound,* the remainder in untreated beakers. The silicone coating did not improve the recovery efficiency or the precision of the test. On the basis of the 12 turbid-water samples, the standard deviation for a mean of 2,092 $\mu\mu$c/l was 194, or a coefficient of variation of 9 per cent. Only 0.8 per cent of the accountable activity was recovered in the stringent acid wash.

Representative sampling of water containing settleable radioactive solids is more difficult. This is shown by the variable total solids from equal volumes of sample (250 ml). Consequently, the precision of the total solids (in parts per million) and the radioactivity per unit weight of solids, and per unit volume of water, were determined. The results of activity per milligram of solids show a smaller percentage of error (coefficient of variation of 3.5 per cent) than when based on the volume of sample (coefficient of variation of 12.3 per cent). Only 0.18 per cent of the accountable activity was recovered in the acid wash.

* Desicote, a product of Beckman Instrument Co., Fullerton, Calif., was used.

Discussion

The determination of gross alpha and beta activities at levels below the maximum permissible concentrations for the most hazardous isotopes is useful for pollution control purposes as well as for establishing background information.

Further analysis is required to evaluate the potential health significance of sustained levels of radioactivity near the maximum permissible levels. A rapid decay rate may establish the absence of significant quantities of the more hazardous long-lived emitters. On the other hand, the presence of substantial activity of long-lived isotopes will require more time-consuming analyses for such materials as radium-226 (*8*), total strontium (9) or strontium-90 (*10*).

Where waters are grossly contaminated, it will be necessary to determine the individual isotopes responsible for such contamination. The development of procedures for the determination of the more hazardous radioisotopes is currently under study.

Summary and Conclusions

The determination of the extent and significance of radioactive water pollution requires a prior knowledge of the natural radioactive background and the current levels of gross alpha and beta radioactivity as well as some knowledge of the source of contamination (with respect to the kinds of radioisotopes). Methods have been developed and surveys made on both natural background and artificial radioactivity (*11–24*). Some of the methods, though involved, have high specificity. Other methods are simple but less specific.

The gross alpha and beta measurements can be assayed at high overall efficiencies. Such a method for low-

level measurements has been described. The method is sensitive to 10–30 per cent of the maximum permissible concentration for unknown isotopes. With ancillary data on the kind of radioisotopes present, this measurement should be quite adequate for estimating the current hazards. Eventually, when the history of contamination becomes obscure or more uncertain, it will be necessary to develop accurate, discriminatory techniques for separately measuring the more hazardous radioisotopes. Studies in this direction are desirable. Fortunately, the more hazardous water contaminants deriving from nuclear-energy operations consist of only a few isotopes in mixed fission products such as strontium-90, strontium-89, and iodine-131. Further studies can be expected to evolve quantitative methods for assaying low levels of specific isotopes in wastes and waters for a more reliable interpretation of the overall significance of radioactive contamination.

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ATTACHMENT IV

SEC-54
(2-58)

DEPARTMENT OF HEALTH, EDUCATION, AND WELFARE
PUBLIC HEALTH SERVICE
Robert A. Taft Sanitary Engineering Center
4676 Columbia Parkway
Cincinnati 26, Ohio

FORM APPROVED:
BUDGET BUREAU
NO.: 68-R654

SAMPLE COLLECTION DATA
(To Be Returned With Each Sample)

MILK SAMPLE NUMBER

PLANT	ADDRESS (Street or Rural Route, City, County, State)
DATES SAMPLE PRODUCED:	AMOUNT OF MILK FROM WHICH SAMPLE WAS TAKEN (LBS.)

NUMBER OF SHIPPERS CONTRIBUTING TO SAMPLE

NAME OF SHIPPERS STARTING SINCE LAST SAMPLE	NAME OF SHIPPERS STOPPING SINCE LAST SAMPLE

REMARKS:

DATE	SIGNATURE

ATTACHMENT V

SEC-55 (2-58)

DEPARTMENT OF HEALTH, EDUCATION, AND WELFARE
PUBLIC HEALTH SERVICE

FORM APPROVED: BUDGET BUREAU NO.: 68-R655

Robert A. Taft Sanitary Engineering Center
4676 Columbia Parkway
Cincinnati 26, Ohio

DHIA HERD CODE NO.

FARM DATA RECORD

PRODUCER (NAME)

PRODUCER'S ADDRESS (Street or Rural Rt., City, County, and State)

PLANT TO WHICH MILK IS SHIPPED (NAME)

PLANT ADDRESS (Street or Rural Rt., City, County, and State)

MILKING HERD

TOTAL NO. OF CATTLE	AVERAGE DAILY PRODUCTION	PREDOMINANT SOURCE OF HERD REPLACEMENTS
		___Raised on Premises ___Purchased Locally ___Other Sources (Specify in Remarks)

PREDOMINANT BREED (Record breed including grades, representing more than 50% of herd, otherwise record mixed)

HOLSTEIN	AYRSHIRE	JERSEY	GUERNSEY	RED DANE	MIXED
BROWN SWISS		DUTCH BELTED		MILKING SHORTHORN	

WATER SUPPLIES
(Check Sources of Supply)

	Municipal	Well Over 50 Ft.	Well under 50 Ft.	Spring	Cistern (Rainwater)	Stream	Pond	OTHER*
DRINKING WATER FOR HERD IN BARN AREAS								
DRINKING WATER FOR HERD IN PASTURES								
WATER FOR WASHING & SANITIZING DAIRY UTENSILS								

* INDICATE OTHER SOURCES OF WATER UNDER REMARKS

SOURCES OF FEED

CLASS	TYPE*	SOURCE		
		LOCAL	SHIPPED IN	WHERE PRODUCED**
LEGUME HAY				
LEGUME HAY				
LEGUME HAY				
GRASS HAY				
GRASS HAY				
CORN SILAGE	XXXXXXXXXX			
OTHER SILAGE				
PULP				
PULP				
SEED MEAL				
SEED MEAL				
SEED MEAL				
TANKAGE				
TANKAGE				
GRAIN				
GRAIN				
GRAIN				

* Indicate types such as oats
** If known, or manufacturer

USUAL FEEDING PRACTICES
(Check Sources of Feed for Each Month)

MONTH	PASTURE	LEGUME HAY	GRASS HAY	CORN SILAGE	GRASS SILAGE	OTHER* SILAGE	CONCENTRATES			
							PULP	SEED MEAL	TANKAGE**	GRAIN
JAN.										
FEB.										
MAR.										
APR.										
MAY										
JUNE										
JULY										
AUG.										
SEPT.										
OCT.										
NOV.										
DEC.										

* INDICATE UNDER REMARKS

** PRODUCTS OF FISH OR ANIMAL ORIGIN.

REMARKS (If additional space required, use reverse side.)

DATE SIGNATURE

Fallout
Jan. Feb. Mar. 1958

Sample number	Date collected	Precipitation-cm Lunken AP	SWC	Beta Radioactivity - $\mu c/m^2$ Date Collected Dissolved	Date Collected Total	End of month Dissolved	End of month Total	One-year-old** Dissolved	One-year-old** Total	Assumed detona. date	Age when collected (days)	Exponential neg. decay slope Dissolved	Exponential neg. decay slope susp.	Remarks
January														
R-141	1/1-3/58	T	0.24(S)		0.56		0.51		0.17	11/22/57	42		0.8	
142	3-6	T	T(S)		2.30		1.58		0.55	9/28/57	102		1.05	
143	6-7	T	T(S)		0.80		0.46		0.11	11/11/57	47		0.95	
144	7-8	T	T(S)		0.30		0.20		0.02	11/11/57	58		1.4	
146	8-13	2.3	15.2	3.26	7.89	2.62	6.18	0.56	1.44	9/28/57	109	1.50	1.35	
150	13-16	16.8	1.6	0.32	0.66	0.28	0.51	0.09	0.15	10/10/57	98	1.22	1.25*	
151	16-21	20.3	15.8		8.20		5.70		0.59	12/1/57	41		1.20	
154	21-22	1.3	3.3(S)		2.65		2.24		0.34	12/1/57	66		1E	
156	22-24	18.0	15.0		10.0		8.50		0.70	12/1/57	58		1.2	a
157	24-28	1.5	8. (S)	2.54	5.76	2.44	5.48	0.42	1.01	12/1/57	52	0.92E	0.92*	
158	28-31	T	1.5	1.38	3.64	1.38	3.64	0.32	0.86	10/10/57	112	1.2	1.2	
Sub-total	1-15	19.1	17.04		12.51		9.47		2.44		Av. 76.0			
Sub-total	15-30	41.1	43.60		30.3		25.6		3.50		Av. 59.8			
Total		60.2	60.64		42.8		35.0		5.94		Av. 68.6			
February														
161	2/1-3	3.4	1.5(S)	0.42	4.48	0.33	3.50	0.10	1.05	10/10/57	116	1.25E	1.25*	
162	3-13	1.5	1.0(S)	1.16	5.26	0.90	4.44	0.20	1.24	11/22/57	83	1.2	0.91*	
163	13	none	none		1.15		0.97		0.28	11/22/57	83		1.0 E	b
164	13-15	1.5	1.4(S)		1.33		1.21		0.47	10/10/57	128		1.0 E	
169	15-19	T	(S)		0.64		0.58		0.16	11/22/57	89		1 E	
170	19-26	7.4	2	1.02	3.35	1.02	3.35		0.90	11/22/57	98		1 E	
171	26-27	6.1	6		3.0		3.0		0.81	11/22/57	98		1 E	
172	27-28	0.3	1		1.62		1.62		0.43	11/22/57	98		1 E	
Sub-total	1-15	6.4	3.90		12.22		10.12		3.04		Av. 100.8			
Sub-total	15-28	13.8	9.0		8.61		8.55		2.30		Av. 95.8			
Total		20.2	12.9		20.83		18.67		5.34		Av. 99.1			
March														
175	3/3	0.3	0.75(S)		1.45		1.05		0.31	12/28/57	65		0.92	
179	3-6	0.3	0.70	0.31	1.19	0.23	0.90	0.08	0.30	1/20/58	45	1.2 *	0.7 *	
180	6-10	0.8	0.13	1.20	11.20	0.68	5.48	0.11	0.72	2/22/58	16	0.75*	0.9*	
181	10-13	T	Dust		2.7		1.35		0.44	2/22/58	19		0.75	
182	13	13.5	9.5(S)	14.0	20.3	7.2	10.05	1.05	1.71	2/27/58	14	0.95	1.0*	
183	14	0.8	7 (S)	7.0	22.2	4.4	14.4	0.53	2.45	1/20/58	53	1.25	1.05*	
187	14-18	T	Dust	—	2.97		2.12		0.55	1/20/58	56		1.0*	
188	18-20	0.5	1.2(S)	0.94	4.04	0.80	3.30	0.18	0.78	1/20/58	59	0.95	0.95*	
190	20-22	T	Dust		2.25		1.85		0.47	1/20/58	61		1.0	
191	22-24	19.3	7.0	5.50	14.5	4.30	10.2	0.89	1.27	2/27/58	25	0.6	1.2*	
192	24		10.0	3.80	5.1	3.16	4.3	0.65	0.99	2/27/58	25	0.8	0.8	
193	25	2.8	9	4.45	6.39	3.70	5.36	0.58	0.84	2/27/58	31	1.2	0.85	
194	25-28	T	T		2.50		2.35		0.30	2/22/58	14		0.90	
195	28-31	0.5	0.8	3.80	9.10	3.80	9.10	0.21	0.66	3/14/58	17	1.0	1.15*	
Sub-total	1-15	15.7	18.08		62.01		35.35		11.43					
Sub-total	15-31	23.1	28.0		43.88		36.45		5.31					
Total		38.8	46.08		105.69		71.81		16.74					

* - decay of total activity

** - from assumed detonation date

a - $2m^2$ tray in service

b - $2m^2$ tray washings (susp)

Fallout
Apr-11 May 1958

Sample number	Date collected	Precipitation-mm Lurken AP	Precipitation-mm SEC	Beta Radioactivity - mμc/M² Date Collected Dissolved	Date Collected Total	End of month Dissolved	End of month Total	One-year-old** Dissolved	One-year-old** Total	Assumed detona. date	Age when collected (days)	Exponential neg. decay slope dissolved	Exponential neg. decay slope susp.	Remarks
April														
R-198	4/1-2	T	0.15	2.50	6.5	1.28	3.28	0.22	0.49	2/27/58	34	1.0	1.15	
199	2-4	0.3	0.40	5.0	12.1	3.0	7.7	0.50	1.30	2/27/58	36	1.0v	1.0v	
200	4-7	25.6	25.0	12.5	80.0	9.1	59.1	1.8	8.90	2/27/58	39	1.0-0.82	1.0	c
201	7-10	11.1	9.8	9.8	15.8	8.0	12.2	1.15	1.85	2/27/58	42	1.0	1.6-0.8	d
202	10	11.0	9.0	4.6	6.7	3.9	5.65	0.40	0.68	2/27/58	42	0.8-1.2	0.95	e
203	10-11	0.8	4.5	10.5	15.2	8.4	12.2	1.05	1.46	2/27/58	43	1.07	1.13	f
206	11-15		Dust		6.3		5.7		0.90	2/27/58	47		1.0*	
207	15-18		Dust		2.0		1.6		0.30	2/27/58	50		1.0*	
208	18-21	17.0	20.0	19.6	32.4	16.2	27.6	3.70	6.9	2/27/58	53	1.15-0.85	0.70	g
209	21-23	2.3	4.5	6.2	14.4	5.6	13.0	0.9	2.4	2/27/58	54	1.0	0.95	g
210	23-24	5.6	6.8	3.3	9.2	3.1	8.6	0.86	2.46	1/20/58	94	0.95	0.95	g
211	24-27	21.1	24.5	7.4	16.1	7.1	15.6	1.37	4.42	1/20/58	97	1.28	1.1v	
214	27-29	11.7	12.0	12.5	30.75	5.55	23.8	2.03	6.45	1/20/58	99	1.58	1.28	h
215	29-30		Dust		3.70		3.70		0.98	1/20/58	100		1.0-1.2	
Sub-total	1-15	49.1	48.85		142.40		105.8		21.70		Av. 40.4			
Sub-total	15-30	57.7	67.80		108.91		93.9		23.83		Av. 78.1			
Total		106.8	116.65		250.31		199.7		45.53		Av. 59.3			
May														
218	5/1-3	19.8	14.0	6.47	14.56	4.53	10.15	1.42	3.33	1/20/58	103	1.3-1.05	1.05	i
219	3-5	23.1	25.0	13.44	26.64	9.48	19.75	3.20	7.12	1/20/58	105	1.4	1.15	
220	5-6	12.9	14.0	17.0	21.91	11.65	15.17	2.09	3.01	2/27/58	68	1.24	1.02	
221	6-7	0.3	0.4	1.77	8.93	1.30	6.41	0.31	1.55	2/27/58	70	1.02	1.02	
226	7-12	2.0	0.8	2.17	6.24	1.59	4.75	0.44	1.35	2/27/58	74	1.1	0.98	
229	12-16	6.3	8.0	2.35	16.83	1.84	5.55	0.43	4.14	2/27/58	78	1.1	0.98	
230	16-19	8.8	8.0	17.3	47.8	15.1	42.0	3.9	11.52	2/27/58	81	1.0	0.95	
231	19-23	41.2	38.0	61.0	100.0	52.0	86.5	6.6	13.6	3/26/58	58	1.2	0.93	
234	23-28	2.3	0.4		11.5		11.0		3.5	3/26/58	63		0.65	
Sub-total	1-15	58.10	54.20		78.28		56.23		46.33		Av. 84.0			
Sub-total	15-31	58.60	54.40		176.13		145.05		32.76		Av. 78.3			
Total		116.7	108.60		254.41		201.28		79.09		Av. 70.0			

* - decay of total activity

** - from assumed detonation date

c - gamma in 212
d - gamma in 222 (Spl of 0910)
e - gamma in 222 (Spl of 1400)
f - gamma in 222 (Spl of 0900)
g - gamma in 223
h - gamma in 224
i - gamma in R-225
v - variable

Fallout
June July 1958

Sample number	Date collected	Precipitation-mm Linken AP	SEC	Beta Radioactivity - μμc/M² Date Collected Dissolved	Date Collected Total	End of month Dissolved	End of month Total	One-year olds** Dissolved	One-year olds** total	Assumed detona. date	Age when collected (days)	Exponential neg. decay slope dissolved	Exponential neg. decay slope susp.	Remarks
June														
R-236	6/1-2	T	0.06		4.6		3.25		0.90	3/26/58	68		1.0	
239	2-9	20.8	22.5	17.4	45.4	12.5	33.7	2.2	7.80	3/26/58	75	1.25	1.0	
240	9-11	31.7	34.8	22.2	47.0	16.0	35.4	2.0	6.60	3/26/58	77	1.52	1.08	
241	11-13		Dust		5.52		4.29		0.94	3/26/58	79		1.15	
242	13-16	34.3	23.0	17.7	74.2	13.8	59.6	1.6	12.7	4/28/58	49	1.25	0.85	
246	16-18	0.8	0.8	3.54	7.54	3.16	6.56	0.82	1.72	3/26/58	84	1.05	1.02	
249	18-20	13.7	12.2	9.90	18.7	8.30	15.5	1.90	3.80	3/26/58	86	1.05	1.0	
250	20-23	4.6	4.3	6.0	11.36	5.3	10.15	0.98	1.96	4/28/58	56	0.96	0.90	j
251	23-26	19.8	18.0	12.0	28.52	5.1	13.35	1.10	4.11	5/26/58	31	1.0	0.8	k
256	26-30		Dust		5.25		5.25		1.20	3/26/58	96		1.13	
Sub-total	1-15	86.8	80.36		176.72		136.24		28.94		Av. 69.6			
Sub-total	15-30	38.9	35.30		71.37		50.80		12.79		Av. 70.6			
Total		125.7	115.66		248.09		187.05		41.73		Av. 70.1			
July														
258	7/1-7	9.9	9.5	15.9	49.1	9.8	29.05	1.25	5.35	5/26/58	42	1.17	1.17	l
260	7-8	22.1	19.5	54.1	77.4	18.2	25.26	0.86	1.72	6/27/58	11	1.0	1.0	m
262	8-10	4.8	2.5	10.9	20.28	4.03	8.21	0.25	1.25	6/27/58	13	1.1	0.8	n
264	10-11	22.2	13.5	55.2	89.3	23.5	37.74	1.20	3.40	6/27/58	14	1.15	0.9	o
266	11-14	20.0	10	17.07	40.07	9.11	20.81	0.80	2.86	6/27/58	17	1.05	0.85	p
268	14-15	2.0	Dust		2.8		1.79		0.27	6/11/58	34		1.05	
272	15-16	28.7	19.5	53.6	86.9	25.7	42.3	1.31	3.59	6/27/58	19	1.24	1.0	q
276	16-18	5.8	4.0	21.5	45.0	11.23	24.83	0.47	2.57	6/27/58	21	1.35	0.85	
278	18-21	50.5	52.0	12.2	26.15	6.9	17.1	1.32	2.78	6/27/58	24	1.6	0.9	r
281	21-22	8.5	10.0	23.7	45.9	16.3	33.8	0.83	3.49	6/27/58	25	1.25	0.8	
282	22-23	17.1	20.0	17.0	31.76	11.8	23.53	0.52	2.12	6/27/58	26	1.32	0.85	
285	23-25	0.8	1.0		17.5		14.4		1.40	6/27/58	28		1 E	
286	25-28	11.4	12.8	17.4	49.3	15.7	44.0	1.07	2.40	6/27/58	31	1.13	1.13	
287	28-31	49.0	52.0	41.17	80.67	41.17	80.67	6.8	11.10	6/27/58	34	0.77	0.92	s
291	31	18.8	19.5	18.4	37.8	8.4	20.1	0.84	3.44	6/27/58	34	1.34	0.86	
Sub-total	1-15	81.0	55.0		278.95		122.86		14.85		Av. 21.8			
Sub-total	15-31	109.6	190.8		420.98		300.73		32.53		Av. 26.9			
Total		190.6	245.8		699.93		423.59		47.38		Av. 26.2			

* - decay of total activity

** - from assumed detonation date

j - R-252-gamma scan of 2 l

k - R-253-gamma scan of 2 l

l - R-259-2 l for gamma, beta- 3300 μμc/l on 7/8

m - R-261 - 2 l for gamma, beta- 3056 μμc/l on 7/11

n - R-263-2l for gamma, beta-3750 μμc/l on 7/29

o - R-256- 2l for gamma, beta-5700 μμc/l 7/11

p - R-267 - 2 l for gamma

q - R-273 - 2 l for gamma, beta - 1810 μμc/l on 7/29

r - R-279 - 2 l for gamma, beta - 1462 μμc/l on 7/28; also R-283 - (10l l)

s - R-290 - 2 l for gamma, beta - 1480 μμc/l on 8/4

Fallout
Aug. Sept. Oct. 1958

p - R-267 = 2 1 for gamma
r - R-279 = 2 1 for gamma, beta = 3462 μμc/1 on 7/28; also R-283 = (104 1)
q - R-273 = 2 1 for gamma, beta = 1610 μμc/1 on 7/29
s - R-290 = 2 1 for gamma, beta = 1480 μμc/1 on 8/4

Fallout
Aug. Sept. Oct. 1958

Sample number	Date collected	Precipitation-mm Lunken	SEC	Beta Radioactivity - μμc/m² Data Collected Dissolved	Data Collected Total	End of month Dissolved	End of month Total	One-year-oldee Dissolved	One-year-oldee Total	Assumed detons. date	Age when collected (days)	Exponential neg. decay slope dissolved	Exponential neg. decay slope susp.	Remarks
August														
R-293	8/1-4	4.0	0.75		54.5		32.5		5.6	6/27/58	38		1.0	t
296	4-8	T	1.0	2.25	12.25	1.36	8.36	0.19	1.69	6/27/58	42	1.15	0.9	
297	8-11	2.5	3.5	5.63	12.94	3.0	8.2	0.27	1.45	7/5/58	37	1.34	0.8	
298	11-12	12.4	27.0	17.6	52.6	10.5	35.4	1.07	5.67	7/5/58	38	1.24	0.9	
299	12-15	11.7	5.0	6.62	16.24	4.68	12.00	0.96	3.22	7/5/58	41	1.30	0.75	
304	15	4.8	10.5	13.2	21.0	8.4	14.3	0.62	1.71	7/5/58	41	1.40	0.92	u
306	15-18		1.8	4.47	12.25	3.2	9.16	0.31	1.04	7/2/58	47	1.28	1.0	
310	18-22	2.8	8.5	4.70	10.40	3.50	8.50	0.32	1.30	7/2/58	51	1.40	0.89	
311	22-25	20.6	17.0	8.8	14.62	8.15	13.48	1.45	3.13	6/11/58	76	0.90	0.65	
312	25-31	3.6	0.5		17.2		17.2		3.2	6/11/58	82		1.08	w
Sub-total	1-15	35.41	47.75		169.54		110.66		19.34		Av. 39.5			
Sub-total	15-31	27.00	27.80		54.47		48.34		8.67		Av. 64.0			
Total		62.41	75.55		224.01		159.00		28.01		Av. 49.3			
September														
318	9/1-3	5.1	8.75	5.10	7.30	3.15	4.67	0.40	0.77	7/11/58	54	1.20	0.93	
320	3-10	2.8	1.5	10.5	10.5		8.1		1.45	7/11/58	61		1.1*	
323	10-15	T	0.15		3.58		2.98		0.67	7/2/58	76		1.07*	
324	15-17	47.0	13.3	2.15	3.50	1.71	2.85	0.35	0.67	8/10/58	46		0.87*	x
325	17		9.0	1.90	4.30	1.45	3.50	0.20	0.78	8/10/58	46	1.07	0.7	y
327	17-22	50.0	52	1.60	8.50	1.19	6.89	0.23	1.99	8/10/58	51	0.98E	0.7 E	z
328	22-25	T	0.15		3.0		2.7		0.38	8/10/58	54		1.05*	
329	25-29	7.8	4.0		41.5		41.0		8.60	7/2/58	89		0.8-1.2*	
330	29-30	1.0	1.0		8.1		9.5		2.36	7/2/58	90		1 E	aa
Sub-total	1-15	7.90	10.40		21.38		15.75		2.89		Av. 63.6			
Sub-total	15-31	105.80	79.45		68.90		66.44		14.78		Av. 62.7			
Total		113.70	89.85		90.28		82.19		17.67		Av. 63.0			
October														
336	10/1-9	5.8	8.5	3.35	6.70	2.6	5.35	0.65	1.60	6/27/58	104	1.27	1.0	
337	9-10	1.0	1.25	0.16	0.61	0.11	0.41	0.02	0.08	8/10/58	58	0.94	0.94	
338	10-14		Dust		1.50		1.3		0.45	7/2/58	104		0.92	
346	15-21	1.5	Dust		12.5		7.05		0.30	10/5/58	16		1.2	
347	21-23	14.0	13.8	17.1	43.9	11.1	28.1	0.44	1.32	10/5/58	18	1.2	1.25	ab
349	23-27	12.9	14.5	22.8	41.65	20.1	35.28	1.73	2.09	10/5/58	22	0.9	1.42	ac
Sub-total	1-14	6.80	9.75		8.81		7.06		2.13		Av. 88.7			
Sub-total	14-31	28.40	28.30		98.05		70.43		3.71		Av. 18.7			
Total		35.20	38.05		106.86		77.49		5.84		Av. 53.7			

* - decay of total activity

** - from assumed detonation date

t - shower
u - R-305 = 2 1 for gamma, beta = 2140 μμc/1 on 8/21
w - includes dust to 9/2
x - see columns "Lunken" and "SEC"
y - raining when R-324 was collected, spl. collected 1520
z - subsample to HK
aa - includes dust, 10/1 and 2
ab - R-348 = 27/27.5 of spl., beta = 44.1 μμc on 10/31
ac - R-350 for relative comp. of isotopes

Fallout
November December 1958

Sample number	Date collected	Precipitation-mm Lucknow AP	Precipitation-mm SEC	Beta Radioactivity - mμc/M² Date Collected Dissolved	Date Collected Total	End of month Dissolved	End of month Total	One-year-old** Dissolved	One-year-old** Total	Assumed detona. date	Age when collected (days)	Exponential regr. decay slope dissolved	Exponential regr. decay slope susp.	Remarks
November														
R-356	11/1-3	4.1	4.0	45.6	100.5	17.5	36.55	1.55	4.47	10/15/58	19	1.15	1.10	ad
358	3-5	0.3	Dust		9.4		3.6		0.4	10/15/58	21		1.1E	
359	5-6		0.5		4.8		2.0		0.2	10/15/58	21		1.1E	ae
360	6-10	9.1	10.0	30.6	74.1	14.9	37.5	1.05	3.43	10/15/58	26	1.27	1.10	af
366	10-14		Dust		33.3		20.8		2.32	10/15/58	30		1.05	
368	14-17	12.2	11	18.0	43.0	10.9	25.33	0.98	1.78	10/25/58	23	1.11	1.15	ag
371	17-19	6.1	7.2	5.5	11.5	4.5	8.9	0.90	1.34	10/15/58	35	0.84	1.12	ah
382	19-24	0.3	0.6		8.8		7.5		0.76	10/15/58	40		1.1E	
384	24-26	4.1	5.4	11.0	17.9	9.4	15.5	0.93	1.53	10/15/58	41	1.1E	1.1E	
385	26-28	23.1	23.1(S)	5.8	32.8	5.5	31.5	0.56	3.16	10/15/58	44	1.1E	1.1E	
395	28-30	T	(S)		2.5		2.5		0.3	10/15/58	46		1.0 *	ai
Sub-total	1-14	13.5	14.5		222.1		100.5		10.82		Av. 29			
Sub-total	14-30	45.8	47.3		116.5		91.9		8.60		Av. 38			
Total		59.3	61.8		338.6		192.4		19.42		Av. 33			
December														
396	12/1-5	20.4	16.7		48.0		29.5		4.5	10/15/58	51		1.2 *	aj
404	5-15	T			3.2		2.6		1.2	10/15/58	61		0.94*	ak
411	15-33	1.8RS	0.08		27.0		24.0		4.6	10/15/58	68		1.05	
415	22-30	1.1	Dust		9.5		9.3		1.7	10/15/58	76		1.1	
Sub-total	1-15	20.4	16.7		51.2		32.1		5.7		Av.56			
Sub-total	15-31	2.9	0.1		36.5		33.3		6.3		Av.72			
Total		23.3	16.8		87.7		65.4		12.0		Av.64			

* - decay of total activity

** - from assumed detonation date

ad - 8.5/9/in R-357 for gamma-beta on 11/6 = 160 mμc/M² ae - Cr-11-10-59 = per liter af - Cr-11-10-59-1400: R-361=20 λ for gamma; beta = 75mμc/on 11/26

ag - 11-17-1330: R-369=2 λ for gamma; beta=6.9 mμc/λ ah - R-383 assigned for gamma ai - 0.12M² collector

aj - 0.12M² collector ak - traces of precipitation on 9, 12, 14, and 15 but no sample taken.

Representative HOLIFIELD. Dr. Langham, you were listed first as a witness at 11, and we did not relegate you to second place for any particular reason except that Dr. Straub seemed to have a prepared statement. I have a statement that has just been handed me with your name on it.

STATEMENT OF DR. WRIGHT LANGHAM,[1] LOS ALAMOS SCIENTIFIC LABORATORY

Dr. LANGHAM. That is the summary statement.

Representative HOLIFIELD. You may proceed.

Dr. LANGHAM. Thank you, sir.

Mr. Chairman, it was my assumption that my job for this subcommittee was to summarize the potential biospheric contamination from weapons tests to date. In doing this job, I would like to emphasize that I am talking primarily about other people's data, which if I remember the last subcommittee hearings, makes the difference between a reporter and an expert.

What do we mean by biospheric contamination? The biosphere refers to all living matter, and the potential biosphere would refer to contamination in the stratosphere as well as in the soil, because this is potential contamination that can enter man's food chain and man himself. At the present time the extent of the levels in man's biosphere are approximately as follows:

The average strontium 90 milk level for 1958, which is the principal source of calcium in the diet of western man, was 8 micromicrocuries per gram of calcium. The cesium 137 in milk was about 50 micromicrocuries of cesium per gram of potassium.

Let me explain why this ratio is used. Cesium follows potassium in the biosphere and man very much as strontium follows calcium. So we can talk of both of these two potentially harmful substances

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[1] Received a bachelor of science degree in science from the Oklahoma A. & M. College at Goodwell, Okla., in 1934, and a master's degree from the Oklahoma A. & M. College at Stillwater, Okla., in 1935. From 1935–37 he held a position of research chemist with the agricultural experimental station at the Oklahoma A. & M. College at Goodwell. From 1937–38 he did graduate work at Iowa State College, Ames, Iowa, working in the field of organic chemistry. From 1939–41 he returned to the agricultural experimental station at Goodwell, Okla., as acting experiment station director. In 1941 he entered the University of Colorado, from which he was awarded a doctor of philosophy degree in biochemistry with a minor in biology in 1943. In the same year he joined the staff of the Manhattan District's metallurgical laboratory at the University of Chicago, where he worked on the chemistry of plutonium. In 1944 he was transferred to the Los Alamos Scientific Laboratory, Los Alamos, N. Mex., to study the toxicology of plutonium and to develop methods of diagnosing exposure of personnel to this material. In 1946 be became the group leader of the biological and medical research group of the laboratory. Since then he has investigated a number of major problems concerning the biological and medical aspects of atomic energy development. These include rates of retention and excretion of internally deposited radioactive isotopes, toxicology, and physiology of tritium in man, relative biological effects of ionizing radiations of different types and energies, including bomb gamma rays and neutrons, incapacitating effects of massive doses of radiation, hazards from accidental noncritical detonation of atomic weapons, and the biological and medical implications of worldwide radioactive fallout. He is at present group leader of the biomedical research group and assistant division leader (for research) of the Los Alamos Scientific Laboratory's Health Division, and is an associate professor of biochemistry with the University of California at Los Angeles. He is a member of the following committees and organizations: Subcommittee on Internal Tolerances of the International Commission for Radiological Protection; Subcommittee on Internal Radiation Tolerances of the National Committee for Radiation Protection; Subcommittee on Incineration of Radioactive Wastes of the National Commission for Radiation Protection; Subcommittee (Chairman) on the Relative Biological Effects of Ionizing Radiation of the National Commission for Radiation Protection; Subcommittee on Toxicity of Internal Emitters of the National Academy of Sciences and National Research Council; the American Association for the Advancement of Science; Radiation Research Society; Health Physics Society; Sigma Xi Honorary Scientific Research Society. (Submitted by witness.)

in terms of their ratios with elements which are a physiological requirement for normal growth and health.

Foods have been analyzed by the Public Health Service. They have been analyzed by the Health Research Laboratory of New York, and by others. It would look as if at the present time cereals are averaging about 15 to 30 micromicrocuries per gram of calcium, meat about the same—I mean meat and vegetables about the same—and milk, of course, as I said previously, about 8. This, then, would give an average dietary strontium 90 concentration, as of 1958, of about 8 micromicrocuries per gram of dietary calcium.

We know by discrimination considerations that the concentration of strontium 90 at equilibrium with the diet, the concentration that is in man's bones, should run approximately one-fourth of the concentration in his diet, which would mean, as of late 1958 or the middle thereof, the concentrations in man's bone that would be in equilibrium with his diet should be of the order of 8 divided by 4, which is 2—2 micromicrocuries per gram of calcium.

Representative HOLIFIELD. Will you relate that to the maximum permissible concentration?

Dr. LANGHAM. Yes, sir. The maximum permissible concentration, of course, is 66 micromicrocuries per gram of calcium, if you assume the latest values of the International Commission. or it is 200 if you assume the value given by the National Commission on Radiation Protection.

Representative HOLIFIELD. In relation to the 2 micromicrocuries, is that an average between adults and children?

Dr. LANGHAM. This is the equilibrium level, which means it is that level that a child's bones would have if its bones were formed in equilibrium with the diet. If you want to know what the level is at the present time, it is much less. The average in children is about 1 to 2 micromicrocuries. The average in adults is about 0.2 micromicrocuries, giving a world average for all ages of about a half a micromicrocurie per gram of calcium as of mid-1958.

Representative HOLIFIELD. The reason between the equilibrium dose and the actual readings of the material in the bones is due to the factor of discrimination?

Dr. LANGHAM. It is due to the fact that there is a delay in reaching equilibrium. So if we wish, sir, to predict the maximum hazard, then we try to predict what the level will be at equilibrium because it is at that time that our hazard is a maximum. In other words, at the present time the concentration in the bones of people, if it is hazardous, is not as great as it will be a few years from now when people come into equilibrium more nearly with the present rate of testing or with the amount of strontium 90 that has been formed from weapons tests to date.

Representative HOLIFIELD. As a person becomes older and after he passes the skeletal forming period, which is, I believe, ordinarily about 30 years——

Dr. LANGHAM. Twenty, sir.

Representative HOLIFIELD. Then the factor of importance is really the absorption by the children, rather than the adults.

Dr. LANGHAM. Yes. In other words, the adult bone is perhaps only changing about 3.5 percent per year. So if one is an adult and has formed his skelton prior to nuclear weapons testing, it will take a long, long time before he comes to equilibrium at the adult rate of skeletal change. Children of about 2 years of age are probably closer to being in equilibrium with their diet than anyone at any time. They may be as much as 80 percent of equilibrium with the diet they are eating.

Representative HOLIFIELD. If we accept the figures, then, that you give for children as being 2 micromicrocuries for equilibrium on the material to date——

Dr. LANGHAM. No, that would be the equilibrium level as of mid-1958.

Representative HOLIFIELD. That would be before the Russian test in October.

Dr. LANGHAM. That is right. So what I would like to do is to project these into the maximum level we will have when children, or for that matter anyone, is in equilibrium with the material that is still in the stratosphere, and still to fall out. In other words, the maximum concentration in people's bones from weapons tests to date will be reached in about 1964 to 1967, depending on the stratospheric mean time of fallout.

Representative HOLIFIELD. Provided there are no additional tests.

Dr. LANGHAM. Provided there are no additional tests. To obtain a world average we have to population weight and diet weight the 1958 equilibrium level because the derivation of our original two micromicrocuries was on the basis of the Western diet. We do know that the Far Eastern countries will have a diet that is somewhat different. So when we completely weight everything populationwise and dietwise, we can come up with a prediction that the average maximum permissible level in the United States as a result of weapons tests to date—and that assumes no more testing—will be 6 micromicrocuries of strontium 90 per gram of calcium.

For the Western civilizations in general, because the United States is slightly higher, we would expect 5 micromicrocuries per gram of calcium. The average for the north temperate population belt, which lies between 20 north and 60 north latitude, and which includes a great segment of the oriental population, may average as high as 8 micromicrocuries per gram of calcium. The Far East alone—that portion of the Far East which happens to lie between 20 north and 60 north latitude—the point of maximum fallout—will have perhaps as high as 10 micromicrocuries per gram of calcium. If we population weight these values to come out with a world population weighted average in which we have taken into consideration diet as well as population distribution, then we come out with a value which averages seven. This is to say that the population weighted world average concentration of strontium 90 in the bone calcium of the world population as a result of weapons tests to date may reach 7 micromicrocuries per gram of calcium.

Representative HOLIFIELD. To interpret this, it would be approximately one-tenth of the maximum permissible level which is set by the

International Committee on Radiation Protection and Measurements, and about one-thirtieth of the maximum permissible concentration set by the National Commission on Radiation.

Dr. LANGHAM. Yes. It would be roughly that. In other words, it would be 7 as compared to 66 or 7 compared to 200.

Let us try to interpret this, at least vaguely, into background dose. In other words, this would mean that the background dose, rather than the bone marrow dose as a result of the strontium 90 accumulation, would be from 5 to 10 percent of the natural background dose. It would mean that the dose received to the bone as a result of strontium 90 would be roughly 10 percent of the dose that the bone is going to receive over 70 years from natural background. So we are talking about, Mr. Holifield, levels as a result of weapons tests to date, which dosewise and concentrationwise are reaching approximately the 10-percent mark.

Let us dispose of cesium 137 rather readily. We know the concentrations of cesium 137 in milk and in people, large segments of them. Milk from 55 to 60 stations are sampled weekly. We can come up with the amounts in people and their milk diet. In this case we find that the average level of cesium 137 in people, as of 1958, was 67 micromicrocuries per gram of potassium. This calculated in terms of internal dose is of the order of 1 to 2 percent of our maximum permissible levels, or 1 to 2 percent of natural background. We are still, by throwing in internally deposited cesium, talking in the vicinity of bone marrow and bone doses of the order of 10 percent of the maximum permissible levels and of background events.

I have projected present testing to its maximum. The next question, of course, is projection of this into the future with a continuation of tests. I think it is very significant that three major nations, since this committee held its hearings 2 years ago have essentially increased the production of these fissionable materials by 80 percent, have doubled the stratospheric reservoir and doubled the deposition on the soil. In essentially 2 years they have doubled the extent of biospheric contamination.

With this pattern of testing, it is rather ridiculous to try to project into the future on the basis of a constant test rate. I was one who in the last hearings proposed the assumption of a constant test rate of 10 megatons per year until we reached equilibrium. All one can really do is make some assumption as to what future test patterns will be and realize that the validity of that projection is only as good as the assumptions upon which it is based.

Let us take an example and say that in the next 5 years we repeat the past 5 years. Then what we will essentially do is double the values I have given you minus the amount of decay which will be roughly 2.5 percent. So if we wish to get the population weighted average, if we repeat the last 5 years' performance in the next 5 years, is to multiply 7 by 1.8, which would mean under that circumstance we would have roughly 12.5 micromicrocuries of strontium 90 per gram of calcium. As we go on and repeat the past 5-year pattern in succession we must decay each 5-year pattern amount by 2.5 percent, depending on the elapsed time. This is a series that I can't solve in

my head. But what will happen, of course, is this: If we continue this repetition of 5-year test patterns until we have repeated it eight times, we will now be in equilibrium with that situation much as we used to project our 10-year constant test rate. This being the case, then, we can say that if we repeat the past performance for the next 40 years, we will multiply everything by 8 and 10 percent multiplied by gives 80 percent of the maximum permissible level we have talked about as recommended by the International Commission.

We have been talking to date of projected averages, and have said nothing about the spread of these data in the populations themselves. If the population spread were even perfectly normal in distribution about the mean, we would expect many of our population to be over the maximum permissible levels if tests continue at the past rate for 40 years. If we allow for the fact that the distribution curve is not normal, but is skewed, then we may have even more of the people over the maximum permissible levels that are recommended. This is the same as saying, sir, that if we wish to hold to the maximum permissible levels that have been prescribed by those people who feel the responsibility of protecting the world's population from radiation, then the present test rate cannot be continued indefinitely.

Representative HOLIFIELD. Thank you very much, Dr. Langham, for your very clear statement on this matter. We will probably call upon you to do some mathematical calculations for our hearings in June which go from the testing data to the theoretical pattern of a full-scale nuclear war, using megaton weapons that may run into scales of a thousand, two thousand, three thousand megaton weapons in an exchange between the nuclear weapon nations. I am sure that will show a completely different picture.

Would you care to comment on the effects of the short lived isotopes about which we have had some discussion as a result of the increased rate of descent? Does that constitute an appreciable hazard?

Dr. LANGHAM. If we assume that weapons tests have stopped, then it will add only about 10 percent to the gamma ray hazard from externally deposited cesium. I would say under these circumstances—certainly under the circumstances of no more tests—it is not terribly important.

Mr. RAMEY. How about if tests were continued?

Dr. LANGHAM. If tests were continued, I would say it is possible that it still will not add very much. Only of the order of another 50 percent.

Representative HOLIFIELD. If the maximum permissible concentration of strontium in the levels of 66 micromicrocuries or 200 micromicrocuries were to be deposited in bone, would there be any type of observable deleterious effect on the bone cells?

Dr. LANGHAM. If I understand your question you are interested in having the levels I have given, either present or predicted, interpreted in some terms of biological effect—in other words, how much effect would we expect to see. Dr. Shelton made the remark yesterday that the physical data were much more certain than the biological data. I am sure this is what he had in mind, even though he hurt my feelings. He was correct. The recent findings of the Russells that the extrapolation to zero dose, which was the basis of allowing the gene-

ticist and the somaticist, the person who is arguing that bone cancer and leukemia can be caused by radiation of any dosage level, is dose rate dependent casts some doubt on the extrapolation as we have used it in the past.

So there has been a recent change in this field which in my opinion suggests that what present and future strontium 90 levels mean in terms of biological effect must await the collection of considerably more data. I don't think we can make such an evaluation at the moment.

Representative HOLIFIELD. Thank you very much for your testimony.

Dr. LANGHAM. Thank you, Mr. Holifield.

Representative HOLIFIELD. The meeting will adjourn until 2 o'clock this afternoon.

(The formal statement of Dr. Langham and his associate, Dr. Anderson, together with several papers and statements on this subject, follows:)

BIOSPHERIC CONTAMINATION FROM NUCLEAR WEAPONS TESTS THROUGH 1958

Wright Langham and E. C. Anderson

SUMMARY

PRODUCTION AND DISTRIBUTION

Weapons tests to January 1, 1959 by all nations have produced about 10 megacuries of Strontium 90 and 18 megacuries of Cesium 137. Total deposition as long range fallout through January 1959 has amounted to about 4 megacuries of Strontium 90 and 7 megacuries of Cesium 137. Although estimates of the stratospheric reservoir are highly uncertain, the partition model of Libby indicates a stratospheric content of about 4.5 megacuries of Strontium 90 and about 8 megacuries of Cesium 137. Assumption of a shorter fallout half-time will reduce considerably the stratospheric inventory, hence the magnitude of the future increase of surface deposition in the event of no more testing. The production of Strontium 90 and Cesium 137 has increased by 80% since the fallout hearings in May of 1957. The stratospheric reservoir and total surface deposition have increased 100% in the same period, largely due to the USSR program. Average surface deposition levels of Strontium 90 and Cesium 137 as of January 1, 1959, were 57 and 103 mc/mi^2 respectively for the U.S. For the 20-60^o N latitude belt the corresponding values are 42 and 72 mc/mi^2. Deposition levels in the N temperate population belt are at least a factor of 2 higher than the world average.

Assuming no more weapons tests, the Strontium 90 and Cesium 137 deposition levels as a result of material supposedly still in the stratosphere will reach a maximum in about 1967. At this time the estimated levels for the U.S. will be 92 and 166 mc/mi^2, and for the N temperate population belt 68 and 122 mc/mi^2.

Since tests by the three present nuclear powers essentially doubled the biospheric contamination in a single year, prediction of levels with continued testing is fruitless. The extent of future biospheric contamination will depend largely on the number of nations that choose to become independent nuclear powers, the time sequence of their development and probably on the locations of their respective test sites.

Since the last hearings Strontium 90 and Cesium 137 content of milk and other foods has increased, but not as rapidly as total fallout indicating a certain degree of direct uptake dependent on the rate of fallout rather than on cumulated levels.

The average levels in milk during 1958 were 7.5 $\mu\mu c$ Sr^{90} per gm of Ca and 50 $\mu\mu c$ Cs^{137}/gK. Some samples were found ranging up to 5 times the average. The weighted average Sr^{90} for the U.S. during 1958 is estimated to be about 10 $\mu\mu c/g$ Ca. The Oriental diet may be as much as twice this value.

Sr^{90} levels in human bones have been measured by Kulp and co-workers at the Lamont Geological Observatory. From these analyses it is possible to estimate the average Sr^{90} in the

bones of the world population at the present as well as to predict what present and future average equilibrium levels will be. Equilibrium levels are important because they are an indication of what the maximum Sr^{90} hazard will be when the bones of the world population are in equilibrium with biospheric contamination. The present population weighted Sr^{90} content of people of the whole world is about 0.5 μμc/g of calcium. Adults average only about 0.19 μμc/g Ca. The present age differential is expected to disappear as equilibrium is approached. The average maximum equilibrium bone levels for the world population will be about 5 and 9 μμc Sr^{90}/g calcium as of January 1959 and 1967 (maximum for weapons tests to date) respectively. The respective values for the U.S. are 4.8 and 7.7 μμc/g. The far eastern countries, because of the nature of their diet, may be the highest. Allowing a spread of 5 to 10 for individual variability it is possible that (in 1967) some individuals may run as high as 50 μμc/g Ca and a few, especially in the Oriental cultures, may approach the level of 100 μμc/g calcium, the recommended maximum for the general population. These estimates are conservative in that they assume an upper limit of the probable stratospheric inventory.

Present levels of Cs-137 in people has been measured and found to average 67 μμc/g of potassium. People in the U.S. and

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Europe average about the same. Levels may not go much higher if weapons tests stop because Cs-137 seems to be contaminating the biosphere largely by direct fallout on vegetation and not through soil integration.

INTRODUCTION

Congressional Subcommittee Hearings on the Nature of Radioactive Fallout and Its Effects on Man (1), held May 27-29 and June 3-7, 1957, by the Joint Committee on Atomic Energy, pointed out the extent of biospheric contamination from nuclear weapons tests and its possible implications to the health and well-being of the world population. Since that time, the United Nations' Scientific Committee on the Effects of Atomic Radiation (2) and the Atomic Energy Commission's Advisory Committee on Biology and Medicine (3) have issued official, comprehensive statements on the possible hazards to man of nuclear radiation and radioactive fallout. Prior to the Hearings and again afterward, in the form of commentaries on the United Nations' report, the Medical Research Council of the United Kingdom (4, 5) and the National Academy of Sciences-National Research Council of the United States (6, 7) have come forth with statements on the subject, making seven official statements in all. All statements, including that of the Congressional investigating subcommittee, are in general agreement on the following points:

(a) Radiation exposure of the world population from fallout (including strontium-90) as a result of tests through mid-1957 was and will be small compared to natural background exposure and exposure from other man-made radiations such as diagnostic X ray.

In fact, fallout exposure from tests up to that time may indeed be small compared to normal variations in natural background.

(b) Any amount of radiation, however small, may carry a small but finite risk of producing an increase in the genetic mutation rate of the population.

(c) In the light of present information, it is not possible to say with any degree of certainty whether there is or is not a threshold radiation dose for production of somatic effects, including leukemia, bone cancer, and general life shortening.

(d) Calculations of biospheric contamination levels in the event of continued testing are intelligent guesses at best and must be made on the basis of certain assumptions as to continued testing rate. The soundness of the conclusions reached necessarily depends on the validity of the initial assumptions.

(e) Continued testing of nuclear weapons will increase biospheric contamination and consequently the probability of risk to the world population. Accelerated testing as more nations become nuclear powers and especially the touching off of nuclear war could result in a serious radiation hazard to world health.

Despite the above general agreement on the potential hazards of fallout and recommendations that tests be limited to 5 (8) to 10 MT (9) of fission energy release per year, tests by the three nuclear powers since that time may have amounted to approximately 40 MT of fission yield.

This paper attempts to summarize some aspects of the biospheric contamination problem at the present time (January 1959) and to show some of the changes that have come about as a result of nuclear weapons tests since mid-1957. Consideration is further restricted to biospheric contamination from strontium-90 and cesium-137. These radionuclides are considered the most important in long-range fallout because of their long physical half-lives and their chemical similarities to natural body constituents, respectively calcium and potassium, which result in relatively high uptake in man and animals.

PRODUCTION AND DISTRIBUTION OF STRONTIUM-90 AND CESIUM-137 FROM NUCLEAR WEAPONS TESTS (JANUARY 1959)

Production

A general estimation of the fission yield of nuclear weapons detonated by all nations to date was made available by the Department of Defense and the Atomic Energy Commission at the present Hearings. These data are given in Table 1 and show a total of over 90 MT of fission energy release by all nations. A major portion of the USSR contribution was made in October 1958. Forty MT, or about 45 per cent of the world total, was detonated during 1957-1958 for an average yearly rate of 20 MT. This is twice the maximum average yearly test rate recommended if testing is to continue indefinitely (9).

Table 1. Fission Yield of Nuclear Weapons Detonated by All Nations through 1958

Year	Fission Yield (Kilotons)		
	US + UK	USSR	Total
Prior to 1952	700	60	760
1952-1954	37,000	500	37,500
1955-1956	9,200	4,000	13,200
1957-1958	19,000	21,000	40,000
Total	65,900	25,560	91,460

One MT of fission energy release produces approximately 0.1 MC of strontium-90 and 0.18 MC of cesium-137. The rate of production of these two important potential biospheric contaminants is given in Table 2.

Table 2. Rate of Production of Strontium-90 and Cesium-137 from Nuclear Detonations by All Nations through 1958

Year	Total Production (Megacuries)	
	Strontium-90	Cesium-137
Prior to 1952	0.08	0.14
1952-1954	3.75	6.75
1955-1956	1.32	2.38
1957-1958	4.00	7.20
Total	9.15	16.47

Nuclear weapons tests to date have produced between 9 and 10 MC of strontium-90 and between 16 and 17 MC of cesium-137, over 90 per cent of which was produced by weapons in the megaton class. This is important to the problem of world-wide fallout because detonations of less than one MT do not penetrate the tropopause and the fission debris not deposited in the immediate vicinity of the test site is contributed directly to the troposphere, to be distributed around the world in the general latitude of the detonation. Megaton detonations, however, have enough energy to penetrate the tropopause and contribute a large fraction of their radioactivity to stratospheric contamination (10). Fission debris injected into the stratosphere is rather widely distributed over the surface of the earth, with preferential deposition in the hemisphere in which the detonation occurred.

Distribution of Strontium-90 and Cesium-137

Distribution of fission debris from nuclear weapons tests is not only dependent on total yield of the detonation but also on the conditions and location of testing. In the absence of adequate direct measurements, Libby (11) postulated that megaton surface detonations over land deposited 80 per cent of their fission debris locally, while surface bursts over water deposited only 20 per cent locally. Of the activity not deposited locally,

99 per cent was assumed to be injected into the stratosphere and 1 per cent into the troposphere. Ninety-nine per cent of the fission products from high air bursts of weapons in the megaton class was assumed to be injected into the stratosphere and 1 per cent into the troposphere. Air and tower bursts of kiloton weapons were assumed to contribute essentially all their radioactivity to the troposphere. Re-evaluation of local fallout data collected during Operation Castle and consideration of new data collected by the Department of Defense and the Naval Radiological Defense Laboratory during Redwing and Hardtack operations suggest strongly that the Libby model overestimated the size of the stratospheric reservoir and the potential biospheric contamination from weapons tests to date. In view of the later considerations, the best estimate of fission product partitioning from megaton detonations is as follows:

Air burst - 99 per cent injected into the stratosphere
1 per cent into the troposphere

Coral surface detonation - 80 per cent deposited locally
20 per cent injected into the stratosphere

Water surface shot - 70 per cent deposited locally
30 per cent injected into the stratosphere

General information on conditions of firing of weapons detonated to date was released recently by the Department of Defense (Table 3).

Table 3. Conditions of Firing of Nuclear Weapons Detonated to Date

Inclusive Years	Fission Yield (Kilotons)		
	Air Burst	Ground Surface	Water Surface
1945-1951	190	550	20
1952-1954	1,000	15,000	22,000
1955-1956	5,600	1,500	6,000
1957-1958	31,000	4,400	4,600
Total	37,790	21,450	32,620

When the more recent partitioning factors are applied to the data, they indicate that stratospheric injection by all nations has been about 50 MT equivalents. This value is considerably less than that estimated by Libby (12) on the basis of earlier partition factors. Insufficient information on conditions of firing of weapons of less than one megaton has been released to permit an estimation of their contribution to long-range tropospheric fallout. As presented in Table 3, the tropospheric component is probably included in the values for local fallout. During a seminar held prior to the recent Hearings, it was generally agreed that uncertainties in yield estimations, conditions of firing, etc., would make an evaluation on the basis of partition factors highly unreliable.

It was agreed, however, that stratospheric plus tropospheric injection from all weapons tests to date had been about 60 MT equivalents and local fallout in the vicinity of test sites (which does not contribute to general biospheric contamination) had been about 30 MT equivalents. It may be inferred also from the data in Tables 2 and 3 that UK and USSR tests during 1957-1958 were predominantly air bursts, which contribute heavily to long-range fallout. Libby has estimated that stratospheric injection by the USSR October series alone was 12.5 to 15 MT equivalents.

If it is assumed that USSR tests have been essentially all air bursts, it is possible to estimate the total fission yield of air bursts by the US plus UK as about 12 MT equivalents. On this basis, application of the more recent partition factors to the data in Tables 2 and 3 suggests that the USSR and US plus UK contribution to stratospheric contamination may be approximately equal. Fission debris injected at far northern latitudes appears to have a shorter mean residence time and falls out predominantly in the northern hemisphere, while some of the material injected near the equator is deposited in the southern hemisphere. This suggests that the US and UK contribution to long-range fallout in the north temperate latitudes no longer predominates over that of the USSR. Because of location and conditions of firing, Russian tests through 1958 may have now contributed as much or more than the US and UK tests to the potential exposure of the 80 per cent

of the world's population who live between 20°N and 60°N latitude.

Estimates of the total amount of strontium-90 deposited as long-range fallout and the amount remaining in the stratospheric reservoir at any given time are dependent on the mean residence time of the material in the stratosphere. The mean residence time of stratospheric debris has been estimated as 5 to 10 years (13). More recent estimates (12, 14, 15, 16) suggest a somewhat shorter fallout time (3 to 6 years). Martell (17) has suggested a mean-time of one year or less for material injected at far northerly latitudes by the USSR. Taking into consideration the various estimates of stratospheric mean-times and the rate of injection, the seminar held prior to the Hearings estimated the stratospheric reservoir immediately after the October USSR test series at about 30 MT equivalents and the amount deposited on the earth's surface as long-range fallout at approximately 30 MT equivalents also. The estimate of the stratospheric content is consistent with preliminary direct measurements reported recently by the Department of Defense and the Atomic Energy Commission. The surface deposition level is consistent also with the 1958 direct soil measurements reported by Alexander and extrapolated to October 1, 1958. He estimated a total surface deposition of 3.2 MC of strontium-90, including the higher than

average deposition levels occurring in the United States as a result of tropospheric fallout from Nevada tests.

Assuming no fractionation and the production of 0.1 and 0.18 MC of strontium-90 and cesium-137, respectively, per megaton of fission energy release, the potential level of strontium-90 and cesium-137 biospheric contamination from weapons tests to date may be estimated (Table 4).

Table 4. Production and Distribution of Strontium-90 and Cesium-137 from Weapons Tests through 1958

Distribution	Megacuries	
	Strontium-90	Cesium-137
Total Production	9	16
Local Deposition[a]	3	5.4
Tropospheric Deposition	1	1.8
Stratospheric Deposition	2	3.6
Tropospheric plus Stratospheric	3	5.4
Stratospheric Reservoir	3	5.4
Total Potential Contamination	6	11

[a]Assumed not to contribute to general biospheric contamination.

Compared to June 1957 estimates (18), these data suggest that weapons tests during the last 18 months may have increased potential biospheric contamination with strontium-90 and cesium-137 by 80 to 100 per cent.

The latitudinal distribution of long-range fallout is demonstrated by the 1958 strontium-90 soil data reported by Alexander (19) and shown in Fig. 1. These data give the integrated strontium-90 soil levels in 10-degree latitudinal bands resulting from long-range fallout as measured during the first half of 1958. Adjustment of the soil data to October 1, 1958, and integration with respect to world surface area showed a total world deposition of about 3.05 MC of strontium-90, excluding the higher surface deposition levels in the US and USSR brought about by the influence of location of their respective continental test sites on tropospheric fallout. Including the extra fallout in the US and USSR, he estimated a total surface deposition as of October 1, 1958, of 3.2 MC, giving a world average of approximately 16 mc/mi^2. Approximately 80 per cent of the world population, however, lives between 20°N and 60°N latitude where (according to the 1958 soil data) the integrated deposition level is about 2.2 times the world average. This would suggest an average soil deposition level for this area (excluding the extra deposition in the US and USSR) as of October 1958 of about 35 mc/mi^2. The United States average may be higher by about

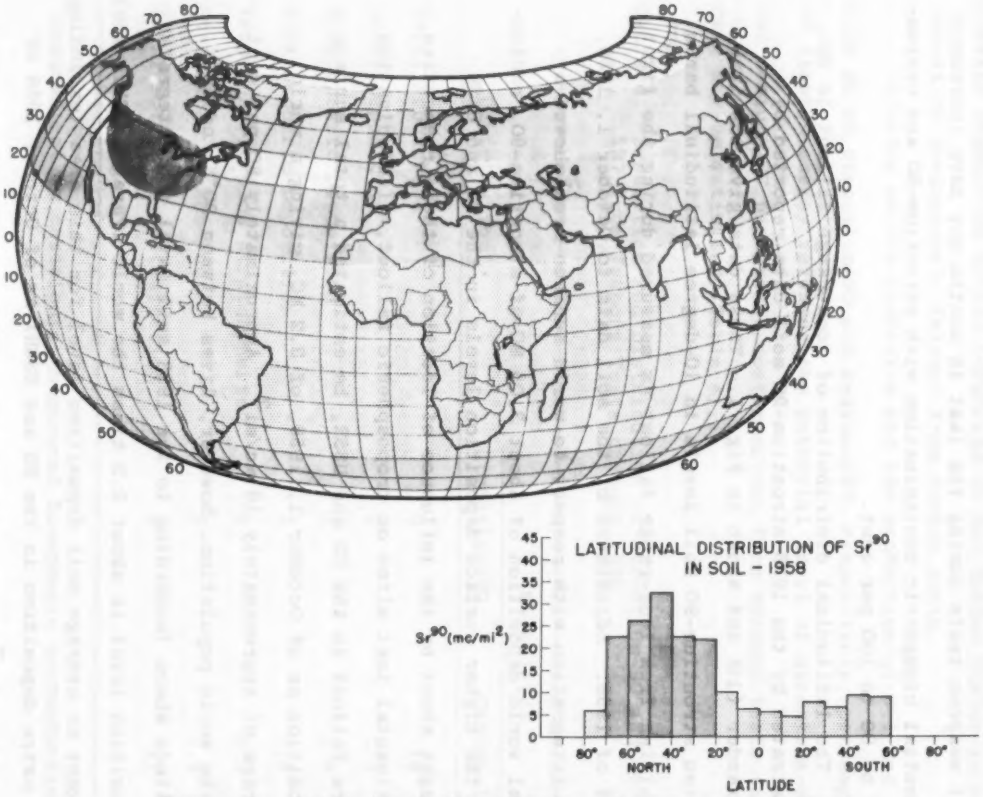

Figure 1. Latitudinal distribution of long-range fallout as of October 1, 1958 (from 1958 strontium-90 soil data).

15 mc/mi^2 (19), giving an integrated level of 50 mc/mi^2, which is consistent with the New York Health and Safety Laboratory's pot collection data on strontium-90 fallout in New York City (20). A few isolated local areas may be found in the US that are about 5 times the global average (e.g., Rapid City, South Dakota, 74 mc/mi^2).

Few data are available on cesium-137 integrated soil deposition levels. If it is assumed, however, that no fractionation occurs and that the cesium-137 to strontium-90 deposition ratio is the same as their ratio of production, the cesium-137 deposition levels may be obtained by multiplying the respective strontium-90 levels by 1.8. The assumption of no fractionation of cesium-137 and strontium-90 between time of production and deposition is not supported strictly by experimental observation. Stewart et al (21) reported an average cesium-137 to strontium-90 ratio of 1.6 in rainwater samples collected in various parts of the world during 1955-1957. Although the ratios varied from place to place, the observed average compares reasonably well with their ratio of production. Since November 1956, the Atomic Energy Commission's Project Ash Can has been collecting fission product samples by means of air filters carried to high altitudes by balloons. The cesium-137 to strontium-90 ratios varied greatly (less than 1, to greater than 10). As pointed out by Libby (12), however, these data are subject to great analytical difficulty and the mean value tends to reduce toward the expected ratio of

as of October 1, 1958 (from 1958 strontium-90 soil data).

fission yield. On this basis and in the absence of more specific soil data, the assumption of no fractionation may be acceptable. The estimates of surface deposition levels of strontium-90 and cesium-137 as of October 1958 are summarized in Table 5.

Table 5. Integrated Strontium-90 and Cesium-137 Surface Deposition Levels (October 1958)

Region	mc/mi^2	
	Strontium-90	Cesium-137
Average USA	50	90
Average 20°N - 60°N Latitude[a]	35	63
Average 20°S - 60°S Latitude	10	18
Average Rest of World	7	13
World Average	16	29
Stratospheric Reservoir	15	27

[a]A region in which 82 per cent of the world population lives.

Future Surface Deposition Levels (Assuming No More Tests)

If no more weapons tests are held, surface deposition levels will continue to increase as a result of fallout from the stratospheric reservoir. The maximum surface deposition level will occur when the rate of stratospheric fallout just equals the

rate of radioactive decay of the material already on the surface. In order to estimate maximum surface deposition levels from weapons already tested, it is necessary to know the stratospheric content of fission debris, its distribution, and rate of return to the troposphere. At least three models of stratospheric transport, mixing, and removal of fission products have been proposed to explain the observed patterns of biospheric contamination.

The first, proposed by Dr. Libby, assumes that nuclear debris injected above the tropopause is mixed rapidly throughout the stratosphere and falls back uniformly into the troposphere with a mean-time of 5 to 10 years. As it falls back into the troposphere, it is deposited over the earth's surface in relation to the earth's precipitation pattern. He attributes the high deposition levels in the north temperate latitudes to the rainfall pattern and to the influence of location of the US and USSR continental test sites on tropospheric fallout.

The second model (advanced by Machta and List in this country, and by Stewart and others in England) assumes that the ascending air currents in the tropical regions are the principal source of stratospheric air which circulates slowly poleward (especially in the winter hemisphere), returning to the troposphere by downward drift in the temperate and polar latitudes. This circulation cycle, the discontinuity of the tropopause in the

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vicinity of the jet streams, and the world precipitation pattern are used to account for the nonuniformity of total fallout, the apparent increase in rate of fallout during the spring season, and the peak deposition levels in the region of 40°N to 50°N latitude.

Based on observations of fallout from the recent USSR test series, Martell (17) has proposed a modification of the Libby model to allow for a shorter mean stratospheric residence time for fission debris injected at far northern latitudes. His model proposes that material injected into the stratosphere near the equator contributes uniformly to world-wide fallout, while material injected at northerly latitudes falls out in the northern hemisphere with a mean-time of one year or less before stratospheric mixing is uniform. This nonuniformity of USSR fallout would account for the higher deposition levels in the northern hemisphere.

Figure 2 is an attempt to illustrate the qualitative features of the various models. Quantitatively, the preferential fallout model of Machta and List predicts greater stratospheric deposition levels at 40°N to 50°N latitude than does Libby's uniform fallout model. Their scheme predicts that the maximum surface deposition level at approximately 40°N latitude may be a factor of 2.5 greater than the world average.

Considerable question still exists as to the mean-time of

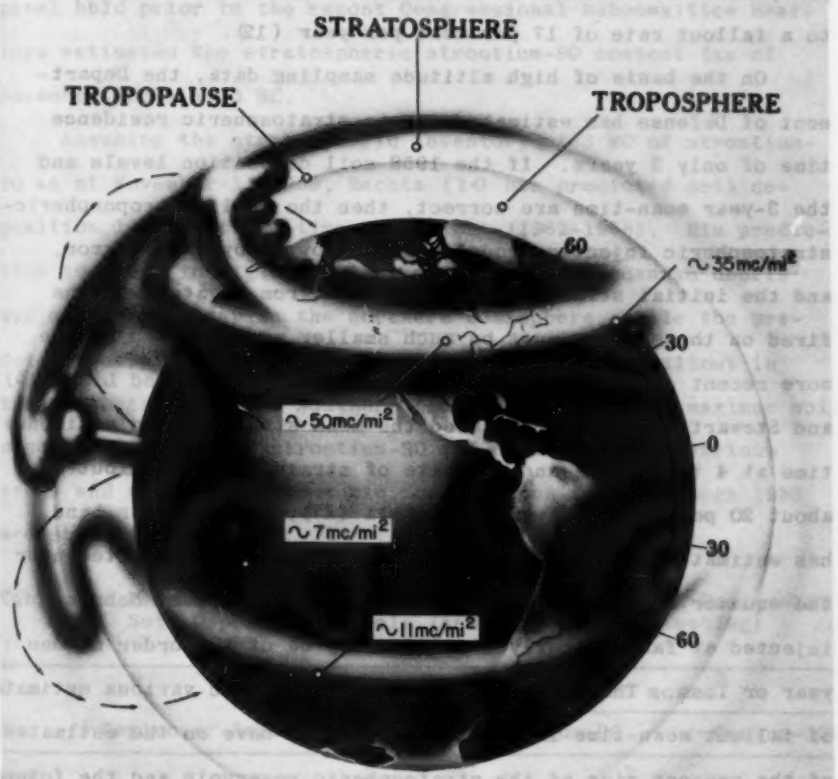

Figure 2. Transport, mixing, and deposition of fission products from megaton nuclear detonations (November 1958).

stratospheric fallout. Libby (11) originally estimated 5 to 10 years, corresponding to a fallout rate of 10 to 20 per cent per year. His most recent estimate is 6 years, corresponding to a fallout rate of 17 per cent per year (12).

On the basis of high altitude sampling data, the Department of Defense has estimated a mean stratospheric residence time of only 3 years. If the 1958 soil deposition levels and the 3-year mean-time are correct, then the earlier tropospheric-stratospheric injection model proposed by Libby is in error, and the initial stratospheric injection from megaton weapons fired on the surface must be much smaller as indicated by the more recent data from direct measurements. Machta and List (15) and Stewart (21) have estimated the mean stratospheric residence time at 4 to 5 years and the rate of stratospheric fallout at about 20 per cent per year. Martell (17), on the other hand, has estimated the mean residence time of material injected near the equator at about 6 years, while the mean-time of debris injected at far northerly latitudes may be of the order of one year or less. The principal significance of the various estimates of fallout mean-time is the influence they have on the estimates of the present size of the stratospheric reservoir and the future build-up of surface deposition levels in the event of no more testing. Attempts have been made by the Department of Defense and the Atomic Energy Commission to measure the stratospheric

reservoir directly. From these measurements and estimates of the USSR injection during October 1958, the fallout prediction panel held prior to the recent Congressional Subcommittee Hearings estimated the stratospheric strontium-90 content (as of November 1958) at 3 MC.

Assuming the stratospheric inventory of 3 MC of strontium-90 as of November 1, 1958, Machta (14) has predicted soil deposition levels at the time of maximum (1962-1965). His prediction is shown in Fig. 3. All the October 1958 Russian debris was assumed to stay in the northern hemisphere, while the pre-October debris was partitioned with twice as much fallout in the northern as in the southern hemisphere. Average maximum soil deposition levels of strontium-90 and cesium-137 for various areas and regions of the world from weapons tests through 1958 are shown in Table 6.

Table 6. Predicted Average Maximum[a] Strontium-90 and Cesium-137 Soil Deposition Levels (Assuming No More Testing)

Region or Area	mc/mi^2	
	Strontium-90	Cesium-137
USA	79	142
Between 20°N - 60°N Latitude[b]	64	115
Between 20°S - 60°S Latitude	17	31
Rest of World	12	22
World Average	26	48

[a]Maximum levels will occur 1962 to 1965.

[b]Includes 82 per cent of the world population.

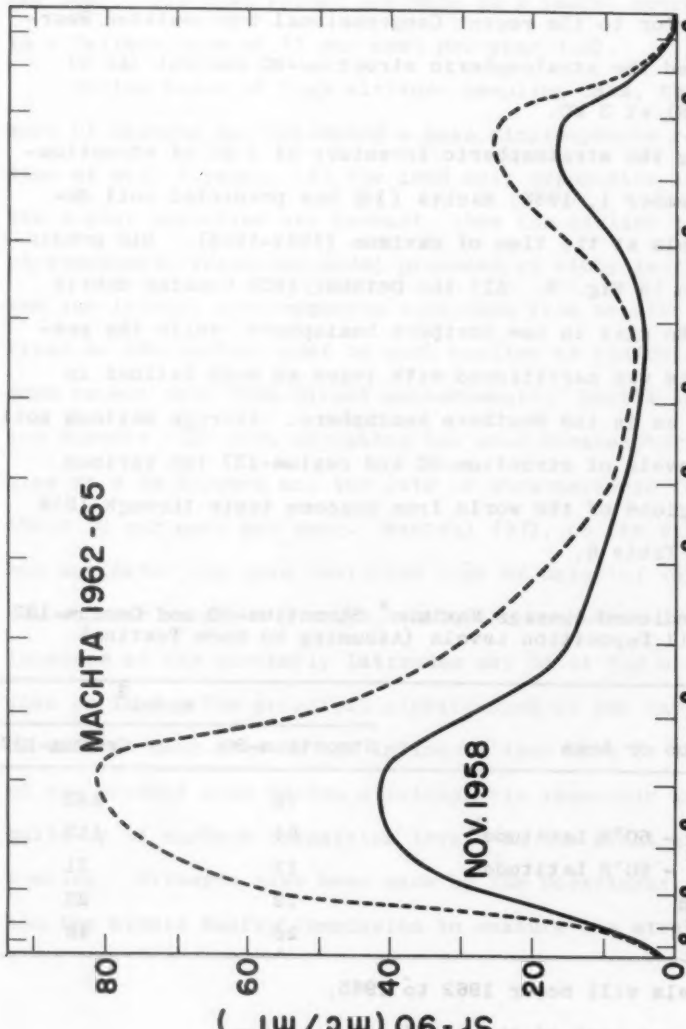

Figure 3. Present and predicted future strontium-90 surface deposition levels as a function of latitude [from weapons tests to date (Machta)].

Since the size of the stratospheric reservoir, the fallout mean-time, and the mechanism of stratospheric distribution are still open to question, these estimates are subject to considerable uncertainty. At the time of maximum surface deposition, the north temperate population belt (where about 82 per cent of the world population lives) may average about 1.8 times the October 1958 level, even if weapons tests are not resumed.

Future Biospheric Contamination (With Continued Testing)

Weapons tests during the past two years by only three nuclear powers have essentially doubled the potential strontium-90 and cesium-137 biospheric contamination. Recent measurements have indicated also that the contribution various nations may make to long-range fallout will be quite dependent on test site location and conditions of weapons detonation. As more nations join the nuclear fraternity, these factors will seriously complicate predictions of resultant contamination levels.

The soundness of the conclusions reached from predictions based on such assumptions as a constant test rate of 10 MT of fission per year or repetition of the past 5-year pattern of nuclear weapons development necessarily depends on the validity

deposition levels as a function of latitude [from weapons tests to date (Machta)].

of the initial assumptions. The test pattern during the past two years seems to invalidate any assumption of a constant test rate, and the possibility of other nations becoming nuclear powers compromises the assumption of repetition of the past 5-year pattern. It is reasonable, however, to expect all nations who choose to become independent nuclear powers to go through somewhat similar developments in which the rate of fission energy release increases exponentially, reflecting the rate of production of fissionable material.

Eventually, the rate of fission product contamination may be expected to reach a maximum and begin decreasing as the techniques of thermal fusion are mastered and the weapons and delivery systems are properly mated. Smaller, subsidiary peaks may occur from time to time as a result of new ideas in weapons technology or additional requirements imposed by delivery systems. It may require from 30 to 60 MT equivalents of fission for a nation to become an independent, fully developed, nuclear power. In this case, the level of biospheric contamination from future tests will be dependent on the number of nations that independently become nuclear powers and the time sequence of their development. Since it is not possible to estimate the number of nations that eventually will become nuclear powers, the time sequence of their development, their conditions of testing, and the locations of their respective tests sites, we can only assume that the test pattern

of the past 5 years will be repeated at 5-year intervals. Strontium-90 and cesium-137 decay at the rate of approximately 2.5 per cent per year. If, therefore, the past 5-year test pattern is repeated in the next 5 years, the potential biospheric strontium-90 and cesium-137 contamination levels will be 1.8 times the projected levels from tests through 1958. If the pattern is repeated every 5 years for the next 40 years, equilibrium will be established at about 8 times the anticipated levels from tests to date. Predicted average maximum strontium-90 and cesium-137 soil deposition levels, assuming the past 5-year test pattern is repeated during the next 5 years, are given in Table 7.

Table 7. Predicted Average Maximum[a] Strontium-90 and Cesium-137 Surface Deposition Levels, Assuming Repetition of Past 5-Year Biospheric Contamination Pattern during the Next 5 Years

Region or Area	mc/mi^2	
	Strontium-90	Cesium-137
USA	142	256
Between 20°N - 60°N Latitude	115	207
Between 20°S - 60°S Latitude	31	56
Rest of World	22	40
World Average	48	86

[a]Maximum levels may occur in about 1968.

These predictions suggest that surface deposition levels in the north temperate population belt may exceed 100 mc/mi^2, if weapons tests continue at the past 5-year rate for another 5 years. Such estimates, however, are little more than guesses since they are predicated on the assumption that future tests will be held at the same sites and in essentially the same manner as in the past.

INCORPORATION OF NUCLEAR DEBRIS INTO THE BIOSPHERE AND MAN

Radionuclides from fallout may enter the body through inspiration of the contaminated atmosphere and by ingestion of contaminated food and water. It is generally agreed that intake of fission products through inhalation of contaminated air and ingestion of contaminated water are of minor significance, and (at the present time) ingestion of food contaminated with strontium-90 and cesium-137 seems to present the major potential hazard.

When strontium-90 and cesium-137 reach the earth's surface, they enter man's food chain largely through contamination of vegetation. Fallout may contaminate vegetation in a variety of ways. That which deposits directly on leaf surfaces may be partially incorporated into plants through foliate absorption or may remain as surface contamination to be ingested by man and his food-producing animals. Strontium-90 and cesium-137 deposited

on the soil surface may enter plants through stem-base absorption. In this case, they will not have time to be diluted by the soil calcium and potassium, and soil-to-plant discrimination factors will not apply. Present indications are that the majority of cesium-137 and an appreciable fraction of the strontium-90 present in man's foods are entering by the above mechanisms. With time, however, the radionuclides are diffusing into the soil and being taken into plants via root absorption.

Only where entry of strontium-90 and cesium-137 into plants is predominantly through root absorption will their levels in man and his foodstuffs bear a consistent relationship to integrated fallout and conform to a true ecological model. Even in this case, a direct relationship between integrated fallout and biospheric contamination may be transient, since diffusion below the root feeding zone may be expected eventually. In any event, when contaminated plants are eaten by animals, the strontium-90 and cesium-137 incorporated in them or deposited on their surfaces are absorbed and retained by the animal according to the specific metabolic characteristics of the individual nuclides. When plant and animal products are eaten by man, the strontium-90 and cesium-137 they contain are absorbed and incorporated into his tissues, again in accordance with their specific metabolic properties.

Strontium-90

Ecological Incorporation and Discrimination.--As stated above, the entry of strontium-90 into man and his food chain will not conform to a pure ecological model until the strontium-90 has become incorporated with soil calcium to the average depth of the plant feeding zone and until root absorption becomes its major route of entry into vegetation. There are many other serious objections to an ecological interpretation of the entry of strontium-90 into the biosphere and to the use of ecological discrimination factors to develop an over-all discrimination ratio for the estimation of equilibrium levels in the bones of man. The concept, however, is still a very useful one in that it provides some understanding of the broad general picture of strontium-90 biospheric contamination.

In earlier papers (9, 18, 22), we developed the over-all discrimination ratio, ($OR_{bone-soil}$), for strontium-90 over calcium in going from soil to human bone using the average US diet as an example (Fig. 4). Recent data have not justified the use of a discrimination factor of less than unity in going from soils to plants, and Fig. 4 has been modified to take these observations into account. The most recent research has failed to indicate any change in the other discrimination factors along the food cycle. Since the over-all discrimination ratio is dependent

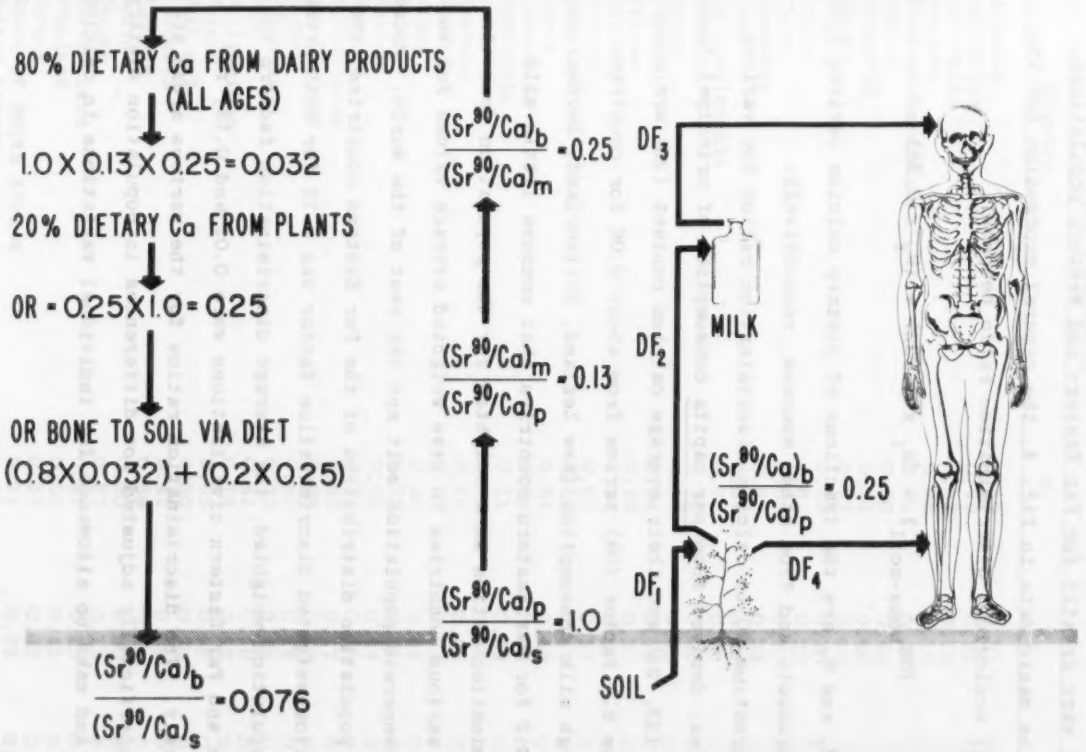

Figure 4. Over-all discrimination ratio for strontium-90 over calcium in going from soil to human bone (US diet).

on the relative proportions of milk-to-plant calcium in the diet, it will vary greatly for Far Eastern and Western populations. Using the basic data in Fig. 4, the general expression for the over-all ecological discrimination ratio becomes

$$(OR_{bone\text{-}soil}) = (M_f \times 0.032) + (R_f \times 0.25)$$

where M_f and R_f are the fractions of dietary calcium derived from dairy products and from other sources, respectively.

Strontium-90 to calcium discrimination ratios for various countries, derived from *per capita* consumption of principal foodstuffs (23, 24) and their average calcium content (25) are given in Table 8. Factor (OR) varies from about 0.06 for countries with high milk consumption (New Zealand, Switzerland, Sweden) to about 0.2 for Far Eastern countries that consume little milk. Discrimination ratios were weighted for the population densities of the various countries to give weighted average values for the north temperate population belt and the rest of the world. Because of the population distribution of the Far Eastern countries, the population-weighted discrimination factor was 0.15 for both areas. When population-weighted, the average discrimination factors for Western and Far Eastern civilizations were 0.09 and 0.18, respectively. The discrimination ratios for the various areas are only superficially adjusted for differences in population dietary habits and make no allowance for individual variations in calcium

Table 8. Strontium-90 to Calcium Discrimination Ratios for Various Countries Derived from per capita Consumption of Principal Foodstuffs

Country	M_f	R_f	OR
Algeria	0.69	0.31	0.099
Argentina	0.79	0.21	0.078
Australia	0.82	0.18	0.071
Austria	0.85	0.15	0.065
Belgium-Luxemburg	0.75	0.25	0.087
Brazil	0.60	0.40	0.119
Bulgaria	0.54	0.46	0.132
Burma	0.33	0.67	0.178
Canada	0.85	0.15	0.066
Chile	0.67	0.33	0.104
China	0.23	0.77	0.200
Columbia	0.67	0.33	0.104
Cuba	0.67	0.33	0.104
Czechoslovakia	0.71	0.29	0.094
Denmark	0.79	0.21	0.078
Egypt	0.57	0.43	0.126
Finland	0.84	0.16	0.067
France	0.75	0.25	0.087
Germany	0.74	0.26	0.089
Greece	0.63	0.37	0.113
Hungary	0.53	0.47	0.134
India	0.51	0.49	0.138
Indochina	0.16	0.84	0.215
Indonesia	0.11	0.89	0.226
Italy	0.62	0.38	0.115
Ireland	0.75	0.25	0.087
Israel	0.73	0.27	0.091
Japan	0.18	0.82	0.211
Malaya	0.19	0.81	0.209
Mexico	0.56	0.44	0.128
Morocco	0.75	0.25	0.087
Netherlands	0.83	0.17	0.069
New Zealand	0.88	0.12	0.058
Norway	0.86	0.14	0.063
Pakistan	0.72	0.28	0.093
Peru	0.41	0.59	0.161
Philippines	0.18	0.82	0.211
Poland	0.55	0.45	0.130
Portugal	0.30	0.70	0.185
Rhodesia	0.41	0.59	0.161
Rumania	0.59	0.41	0.121
Spain	0.50	0.50	0.141
Sweden	0.87	0.13	0.060
Switzerland	0.87	0.13	0.060
Thailand	0.55	0.45	0.130
Turkey	0.37	0.63	0.169
Union of South Africa	0.71	0.29	0.094
United Kingdom	0.81	0.19	0.073
United States	0.80	0.20	0.076
Uruguay	0.82	0.18	0.071
Venezuela	0.75	0.25	0.087
Yugoslavia	0.67	0.33	0.104

metabolism and for the fraction of strontium-90 entering the food chain through direct fallout on vegetation or via stem-base absorption.

Assuming an average of 20 grams available calcium per square foot of soil to a depth of 2.5 inches, 1 mc of strontium-90 per square mile is equivalent to 1.8 μμc strontium-90 per gram available soil calcium (13). If all the strontium-90 is in available form, multiplication of the surface deposition levels by 1.8 gives the strontium-90 activity per gram of available soil calcium. Multiplication of the specific activity of available soil calcium by the strontium-90 discrimination ratio should give the average maximum specific activity of calcium laid down in the adult skeleton through exchange and bone remodeling during the period of environmental contamination and the average maximum strontium-90 concentration in a skeleton at equilibrium with the integrated surface deposition levels. Average equilibrium bone levels for the various regions and populations (derived from discrimination factors and the data in Tables 5 and 6) are shown in Table 9. Bone levels derived in this way have only the broadest general significance and may be conservative, since they are derived on the basis of complete equilibrium and availability of the deposited strontium-90 in the top 2.5 inches of soil and on the assumption that all man's dietary calcium comes only from this depth.

Table 9. Average Maximum Strontium-90 Equilibrium Bone Levels Derived from Ecological Considerations[a]

Region or Population	μμc/g Calcium	
	1958	At Maximum[b]
Average USA	7	11
Western Populations[c]	6	10
Average 20°N - 60°N Latitude[c]	9	16
Far Eastern Populations	11	20
Weighted World Average[c]	7	13

[a]Assuming no more tests after 1958.

[b]Maximum will occur about 1962 to 1965.

[c]Weighted for diet and population.

Cultivation will distribute the strontium-90 to a greater depth, and soil analyses show that 10 to 20 per cent of the strontium-90 in some areas has already diffused below a level of 3 inches.

Another method of estimating strontium-90 bone equilibrium levels from ecological considerations is to use the geochemical observations of Thurber et al (26) regarding transport of common strontium. Using the average stable strontium content of soils and rocks and the average strontium content of human bones from 16 localities throughout the world (including Asia), they estimated

an over-all discrimination ratio from soil-to-bone of 0.07. The data showed relatively uniform world-wide distribution of stable strontium in human bone, which reflects uniformity of dietary intake. From this observation, they concluded that variations in strontium-to-calcium discrimination ratios in different areas may not be important in determining strontium-90 bone levels. The population-weighted world average maximum equilibrium bone level in 1962 to 1965, calculated on the basis of their stable strontium data, would be about 7 μμc/g calcium. This is a factor of approximately 2 lower than that estimated by the use of the individual diet-weighted discrimination factors for the various regions and populations.

The use of element-pair discrimination factors and transport ratios in evaluating the potential hazard of a radioactive member of the pair has been questioned by Kornberg (27). In the first place, their use in the above manner implies that such ratios are constants, which is not borne out by experiment. In addition, the use of ratios has led frequently to reporting of strontium-90 content of foods only as μμc per gram calcium. In the future, it may be desirable to report both concentration and strontium-90 to calcium ratios. These criticisms, however, do not invalidate the use of ecological discrimination ratios and discrimination factors in considering the broad general aspects of biospheric contamination.

Strontium-90 in Foods.--It seems doubtful that strontium-90 is entering man's food chain at the present time entirely through the soil. A fraction of the strontium-90 concentration in vegetation may be derived from leaf pick-up and base-stem absorption. Analysis of principal foodstuffs responsible for dietary calcium intake may give a better indication of man's strontium-90 burden than the ecological considerations discussed previously. In the US and other Western populations, milk is the principal source of body calcium. This is particularly true during childhood, which is the period of skeletal accretion.

The New York Health and Safety Laboratory began analyses of milk samples from the New York area early in 1954. The program in the US and other countries now includes analyses of milk from about 100 locations. The U. S. Public Health Service also initiated a milk sampling program, which began in 1957 and which has now expanded to include 10 sampling points well spaced to give a general coverage of the United States. Their data show a general average strontium-90 content for US milk of approximately 5.3 μμc/g calcium during 1957 and 6.7 μμc during 1958, with an occasional sample in 1958 running as high as 18 to 20 μμc. The average milk value (for the same period) from four stations, including the very high area of Mandan, North Dakota, reported by the Health and Safety Laboratory was 10 μμc per gram calcium. The average for 1957 for the same four stations was 7.2. The average

strontium-90 milk concentration in England during 1957 was 5.4 μμc per gram calcium (28). The average strontium-90 content of United States and English milk is summarized in Table 10.

Table 10. Yearly Average Strontium-90 Milk Values

Year	Strontium-90 in μμc per gram Calcium	
	U. S. A.	England
1955	3.5	4.1
1956	4.4	4.4
1957	6.3[a]	5.4
1958	8.2[a]	---

[a]Average of USPHS and HASL results. The value may be somewhat high because of the relative weight of the Mandan, North Dakota, data.

The change in strontium-90 content of US milk with time and its relationship to integrated fallout level from soil analyses and pot collection data are indicated in Fig. 5. In this figure the strontium-90 content of milk from the New York City milk-shed since June 1954 is compared with the integrated fallout (New York City pot collections and soil analysis data) in the same area. Although both integrated fallout and strontium-90 in milk have increased with time, there is a general indication that the increase in milk fluctuates to some extent with the rate of fallout. The

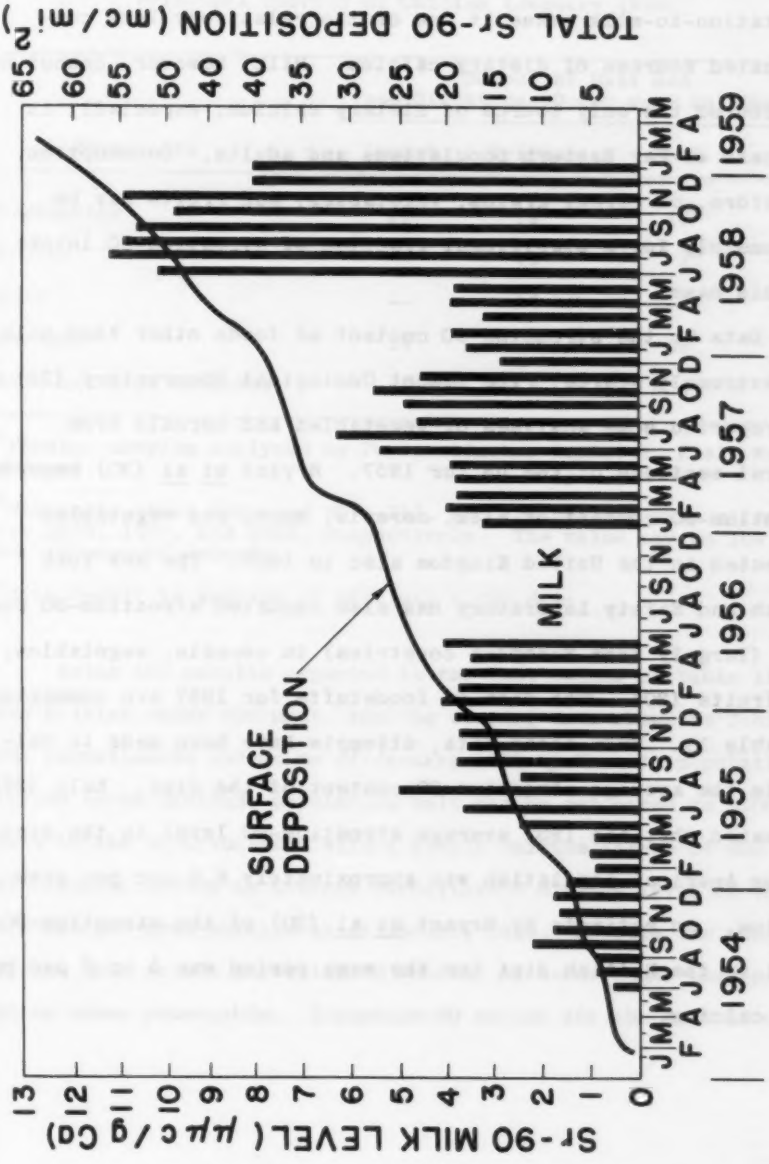
TOTAL Sr-90 DEPOSITION (mc/mi²)
65 60 55 50 40 40 35 30 25 20 15 10 5
Sr-90 MILK LEVEL (μμc/g Ca)
13 12 11 10 9 8 7 6 5 4 3 2 1 0
SURFACE DEPOSITION
MILK
J M M J S N J M M J S N J M M J S N J M M J S N J M M
F A J A O D F A J A O D F A J A O D F A J A O D F A
1954 1955 1956 1957 1958 1959

Figure 5. Change in strontium-90 content of milk with time and its relationship to integrated fallout level.

high discrimination factor against strontium-90 in going from vegetation-to-milk makes it one of the relatively less contaminated sources of dietary calcium. Milk, however, cannot be considered the only source of dietary calcium, especially in the case of Far Eastern populations and adults. Consumption, therefore, of cereal grains, vegetables, and fruits may be responsible for a significant fraction of strontium-90 intake in this case.

Data on the strontium-90 content of foods other than milk are extremely scarce. The Lamont Geological Observatory (29) has reported some analyses of vegetables and cereals from several sections of the US for 1957. Bryant et al (30) reported strontium-90 content of milk, cereals, meat, and vegetables collected in the United Kingdom also in 1957. The New York Health and Safety Laboratory has also reported strontium-90 content (largely from European countries) in cereals, vegetables, and fruits (20). The data on foodstuffs for 1957 are summarized in Table 11. From these data, attempts have been made to calculate the average strontium-90 content of the diet. Kulp (29) estimated that the 1957 average strontium-90 level in the diet in the American population was approximately 6.5 μμc per gram calcium. An estimate by Bryant et al (30) of the strontium-90 level in the British diet for the same period was 5 to 6 μμc per gram calcium.

Table 11. Average Strontium-90 Content of Foodstuffs Providing Principal Sources of Calcium (January 1958)

Foodstuffs	Source of Data and μμc Strontium-90 per gram Calcium		
	Lamont	British	HASL
Vegetables	10	14	15[a]
Fruits	--	15	7[a]
Meat	--	15	--
Cereals	14	6	29[b]
Milk and Milk Products	6	5.4	6.3[c]

[a]Foreign samples analyzed by Physikalisches Institut, Kiel, West Germany.

[b]Minnesota wheat averaged 107, 217, and 155 μμc per gram calcium for 1956, 1957, and 1958, respectively. The value may be low for average US cereals.

[c]Milk result is average of HASL and USPHS data.

Using the results reported in the last column of Table 11, the British value for meat, and the dietary data shown in Table 12, the strontium-90 intake as of January 1958 by Western populations in the north latitude population belt can be estimated as 3700 μμc. This intake is associated with a yearly calcium intake of 450 grams of calcium, giving an average strontium-90 dietary level of about 8.2 μμc per gram calcium as of January 1958. Despite the inadequacy of the data on the strontium-90 content of foods other than milk, this value seems reasonable. Strontium-90 values for the diet of Oriental

populations may be expected to run somewhat higher than the above estimates.

Table 12. Average Calcium and Strontium-90 Intake for Western Populations (1957)

Source	Yearly Consumption[a]	Calcium (g/kg)	Yearly Calcium Intake (g)	Strontium-90 Intake (μμc)
Milk	322 qts	1.15 (qt)	370	2294
Cereals	97 kg	0.16	16	464
Meats	90 kg	0.11	10	150
Fruits	104 kg	0.15	8	56
Vegetables	156 kg	0.32	50	750
	Total		454	3714

[a]Based on US diet; total calcium and strontium-90 intake may be lower for other Western populations. The calcium to strontium-90 ratio, however, should be relatively the same.

Kulp et al (31) also investigated the maximum variability of strontium-90 concentration in a number of foodstuffs from various parts of the world. A set of 50 US milk samples, collected from untilled farms or poor lands where maximum strontium-90 to calcium levels would be expected, showed maximum values 3 to 5 times the US mean value. Milk samples collected from five individual areas in the vicinity of Mandan, North Dakota, in May 1958 (an area chosen for its very high milk values) showed an average

strontium-90 content of 19.1 μμc per gram calcium, with a range from 8.7 to 35.8. The maximum was only 5 times the US average milk value for 1958.

As part of a study of possible correlations between the strontium-90 and cesium-137 levels in milk, a series of 1957 milk samples collected and analyzed for cesium at LASL were sent to the Lamont Geological Observatory for strontium-90 analysis. Three hundred and sixty-two weekly samples from 19 localities were composited into 112 monthly samples. The strontium-90 analyses indicated an average strontium-90 in US milk for 1957 of 5.1 SU, with a range from 0.8 in Arizona to 11.3 in North Dakota.

Kulp et al (31) also found that other foodstuffs from various parts of the world did not vary more than 3 times the world average. A special study was made of the food supply of an isolated Indian tribe in the upper Amazon Basin, an area chosen because of the high rainfall and severely leached soil, and again the maximum strontium-90 levels were only 4 to 5 times the world average.

The above results are consistent with the conclusion that a factor of 2 above the mean will include the variation in strontium-90 concentration of foods in many areas and that a factor of about 5 will cover most variations.

The high strontium-90 content of Minnesota wheat has received considerable attention recently (32). Since cereals provide only 16 out of 450 grams of the yearly dietary calcium intake,

occasional high values in localized areas will not have a proportionate effect on the general US average strontium-90 intake. Taking the average strontium-90 content of Minnesota wheat as the average for all cereals would have increased the January 1958 dietary strontium-90 level to 10.5 μμc per gram calcium, an increase of about 30 per cent. Wheat is not the only cereal in the diet, and it is seldom that cereals grown in a specific area will be consumed locally. Processing may be expected to remove some of the strontium-90 also.

Strontium-90 Levels in People.--Since the ultimate purpose of all fallout studies is the assessment of its potential hazard to man, direct measurements of strontium-90 accumulation in the skeleton are extremely important. Investigations of the actual levels of strontium-90 in human bone have been carried out since 1955 by laboratories in the United States and Great Britain (33, 34, 35, 36, 37). The most recent report from the Lamont Geological Observatory (35) summarizes the results of 1709 analyses (1339 adults, 370 children) from a collecting network which includes 35 stations with a world-wide distribution weighted in favor of Western populations. As a result of these and other studies, the distribution of strontium-90 through the adult skeleton is well established, and the variation of strontium-90 burden with age is shown to be in agreement with predictions based on skeletal growth and fallout rates.

One of the most difficult problems encountered in evaluating the strontium-90 hazard is the present nonequilibrium state between strontium-90 levels in bone and those in the environment. This lack of equilibrium is brought about by the long growth period of the human skeleton (20 years) relative to the period of weapons testing and the low rate of exchange and remodeling of adult bone [3.8 per cent per year (35)]. Children are nearest to being in equilibrium and hence have the highest strontium-90 bone values. As the period of weapons testing, however, approaches that of skeletal formation, the discrepancy between children and adults will disappear.

That the variation of strontium-90 content of human bone with age could be explained on the basis of the rate of skeletal formation and the fallout pattern was first shown by Langham and Anderson (38). More detailed analyses based on later data have been made (35, 39). Closest approach to equilibrium occurs at about age 2 years, at which time the skeleton has a strontium-90 concentration that is about 80 per cent of dietary equilibrium. Adult skeletons, which contain only that strontium-90 entering by exchange and remodeling, show no age effect and presently average about 8 per cent of equilibrium. Analytical data reported by Kulp *et al* (35) showed the population-weighted world average strontium-90 bone level was 0.52 μμc per gram calcium as of January 1958, and the weighted average in the adult population was 0.19.

The values for the area between 20°N and 60°N latitude would be essentially the same, since about 80 per cent of the world's population lives in that region. No great difference was found between the average bone levels in the northern and southern hemispheres. This was attributed to the transport of food from the northern to the southern hemisphere. The data also suggest that bone levels in rice-diet areas are higher than those in Western populations by a factor of about 2, which is at variance with the observations based on data for common strontium (26). A factor of 2 difference is also indicated by the comparison of the strontium-90 content of fetal bone from Asia and North America plus Europe (1.3 and 0.62 μμc per gram calcium, respectively), and by comparison of the few data for Far Eastern adult bone with that for Western populations. It should be kept in mind also that the majority of the bone data, regardless of area, comes almost exclusively from an urban environment, necessitated by the method of collection. Strontium-90 levels in bones from Great Britain have been reported by Bryant et al (28). Their results are in good general agreement with those obtained by the US and have been combined and included in the report by Kulp et al (35).

The observed strontium-90 concentrations in bone and in diet and soil (discussed in the previous sections) can be used to estimate average maximum bone concentrations at equilibrium. Equilibrium levels are important because they represent the

strontium-90 concentration in newly formed bone of children and adults and supposedly the concentration in all bone laid down in equilibrium with the diet and eventually in equilibrium with the level of biospheric contamination. Average bone concentrations at equilibrium are a measure of the average maximum potential hazard of strontium-90 biospheric contamination to the population.

Kulp et al (35) have used the strontium-90 bone data for all ages to calculate the average maximum equilibrium bone levels for Western populations in the region between 20°N and 60°N latitude from weapons tests to date. Their estimates are based on the strontium-90 content of the average Western diet for the past 5 years, the rate of deposition of skeletal calcium with age (40), an average bone-to-diet discrimination ratio of 0.25 (30, 41, 42), and a value of 3.8 per cent per year for the average rate of bone turnover (35). The calculations show average maximum equilibrium bone levels of approximately 1.8 and 4.1 μμc strontium-90 per gram calcium as of January 1958 and 1966 (the time of maximum concentration from weapons tests to date), respectively.

Using the 1957-1958 average bone data for the various ages and a similar method of calculation (38), we estimate the average maximum strontium-90 bone level for Western populations between 20°N and 60°N latitude at 2 μμc per gram calcium as of January 1958.

These estimates may be compared with that of Bryant et al (30), who estimated the average equilibrium bone level in Great Britain as of mid-1957 at between 1 and 2 μμc per gram calcium. Extrapolated to January 1958, their value would be between 1.2 and 2.4. The average maximum equilibrium bone level for Western populations (for January 1958) may be estimated also from the dietary data given in Table 12. These data show an average dietary intake for the United States, and presumably for Western populations, of 3700 μμc strontium-90 associated with a calcium intake of 454 grams. Assuming a bone-to-diet discrimination ratio of 0.25, the equilibrium bone level as of January 1958 would be 2 μμc per gram calcium. Our calculation of the equilibrium bone level (as of January 1958) from the British bone data gives a value of 2.2 μμc per gram calcium.

Taking 2 μμc per gram calcium as the average maximum strontium-90 equilibrium bone level for Western populations as of January 1958 and assuming the corresponding average strontium-90 surface deposition level between 20°N and 60°N latitude was 25 mc/mi^2, the equilibrium concentration per mc/mi^2 is 0.08. Using this factor and assuming bone equilibrium levels scale directly with integrated surface deposition, the data in Tables 5 and 6 can be used to calculate the average maximum equilibrium bone levels for the various regions as of October 1958 and at the time of maximum in 1962 to 1965. Equilibrium levels calculated

in this way are shown in Table 13.

Table 13. Estimated Average Maximum Strontium-90 Equilibrium Bone Levels Derived from Bone Analyses

Region	μμc per gram Calcium	
	1958	At Maximum[a]
USA	4	6
Western Population	3	5
20°N - 60°N Latitude[b]	5	8
Far Eastern Population[c]	6	10
Weighted World Average[d]	4	7

[a]Assuming no more testing after 1958, maximum will be reached in 1962 to 1965. If tests continue at past 5-year rate, bone levels will increase by a factor of about 1.8.

[b]Weighted for population and diet of Far Eastern populations.

[c]Assumed to be 2 times Western population for dietary reasons.

[d]Dietary-weighted.

In making these computations, it was assumed that the equilibrium bone levels derived from the analytical data represented Western populations and not the north temperate population belt. It was further assumed that Far Eastern populations would have bone levels twice those of Western populations. In computing the population-weighted world average, the dietary habits of the various

populations were considered.

These data show world population-weighted average maximum strontium-90 bone equilibrium levels of 4 and 7 μμc per gram calcium for October 1958 and at maximum from weapons tests to date (1962 to 1965), respectively. The latter value is approximately 10 per cent of that suggested recently by the International Commission on Radiological Protection as the maximum permissible level for the general population. It should be kept in mind, however, that the numbers in Table 13 were predicated on the assumptions that the stratospheric reservoir contains 3 MC of strontium-90 from past tests, and that the concentration in the world's diet (and consequently in human bone) will integrate with the surface deposition level. Analyses of milk and other foods indicate that some strontium-90 is entering the food chain via direct contamination of vegetation, and it is not possible at present to say whether man's dietary strontium-90 concentration will follow exactly the integrated surface deposition level. If it does not, the projected 1962 to 1965 bone levels in Table 13 will be conservative. Average maximum equilibrium bone levels in about 1968, assuming weapons tests continue and the past 5-year test pattern is repeated, may be obtained by multiplying the values in Table 13 by a factor of 1.8.

It is of interest to compare the average maximum equilibrium bone levels estimated from bone analyses with those derived from

strontium-90 ecological discrimination ratios given in Table 9. Invariably, the ecological predictions are higher by approximately a factor of 2. A number of postulates can be made as to the reason for this discrepancy, most of which involve strontium-90 soil-to-plant relationships. It is possible that the plant-to-soil discrimination factor is about 0.5 or that about 50 per cent of the strontium-90 becomes unavailable in the soil, or that the over-all average depth of the feeding zone of plants that contribute man's dietary calcium is about 5 inches instead of 2-1/2. It is possible also that the world average available soil calcium on which man's food is produced is 40 grams per square foot instead of 20. It is, of course, possible also that a time-lag in the soil-to-plant equilibrium level is making the bone values low. Since so many uncertainties exist in the soil-to-plant relationship and the bone values are based on direct measurement, they would seem to be preferred in estimating the potential hazard of strontium-90 fallout.

The estimated values given in Table 13 are general averages for the maximum equilibrium strontium-90 bone levels in large segments of the world population and make no allowances for local variations in fallout due to meteorological factors, proximity to test sites, geographic and local variations in available soil calcium, and individual variations in dietary habits, nutritional states, and metabolic factors. Great uncertainty

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exists as to the expected individual variation of strontium-90 bone levels from the regional and population averages. The point in question is the ultimate distribution curve for the world population, when equilibrium with biospheric contamination is attained. A distribution curve of all bone data collected from subjects of all ages would be meaningless because of the relatively short period of environmental contamination and the age-dependence of strontium-90 contamination. As would be expected, however, the data for adult bone show no age-dependence. These facts led Kulp et al (35) to consider a frequency plot of all 1957-1958 adult bone data as an empirical basis for estimating strontium-90 distribution in the world population. The frequency distribution plot was clearly not normal but rather approached a log-normal pattern, and the actual data extended to a maximum of 10 times the mean. The true distribution at equilibrium with biospheric contamination will undoubtedly be narrower as deduced by the Lamont group from the distribution patterns of adult bone data from more homogeneous populations. Adult bone data for New York City showed a maximum value that was only 3 times the mean, while the highest observed value in the data from the northeastern United States was about 4 times the mean. The data from rice-diet areas clearly showed a wider spread than that from the United States by a factor of about 2. The maximum measured value in this area was about 7 times the mean.

This was attributed to the possible influence of more restricted food distribution and the greater variability of strontium-90 content of vegetation as compared to that of milk, the principal source of dietary calcium in the US population.

On the basis of these and other considerations, Kulp et al (39) suggested that 10 per cent of the population may exceed twice the mean (for their age group), 1 per cent may exceed 5 times the mean, and none will exceed 20 times the mean value. Schubert (43), who believes the Kulp data do fit a log-normal distribution, has estimated that 5 per cent of the world population may reach levels of 2 times the mean and 0.05 per cent may reach levels of as much as 4 times the mean value. The above estimates of population spread may be conservative, since they make no allowance for a narrowing of the distribution as complete biospheric equilibrium is approached and for the contribution of analytical error in the observed spread in the bone data. For practical considerations of the potential hazard of strontium-90 fallout to the world population, a spread of 5 times the mean with an upper limit of 10 would seem to be realistic. Application of these values to the 1962 to 1967 weighted world average maximum strontium-90 bone level of 7 μμc per gram calcium suggests that a small segment of the world's population may approach 70 μμc strontium-90 per gram calcium as a result of nuclear weapons already tested. A few people in the Far Eastern populations may

approach the level of 100 μμc per gram calcium, which is above the permissible level of 66 μμc recommended for the general population in the latest report of the International Commission on Radiological Protection (44).

Cesium-137

When cesium-137 falls to the earth's surface, it is taken into plants through foliate and stem-base absorption. Incorporated into the soil, it will be absorbed via the root system according to its specific soil-plant characteristics. Its entry into vegetation at the present time is believed to be principally by the first two mechanisms.

Cesium is chemically and metabolically similar to potassium, which is an essential constituent of plants and animals. It is reasonable, therefore, to expect that to a first approximation it will follow potassium in the biosphere much as strontium-90 follows calcium, and to express cesium-137 levels in terms of its ratio to potassium. Metabolically, cesium is enough like potassium that it concentrates in the same tissues and organs (predominantly the muscle). Chemically and biochemically, however, it differs enough from potassium that pronounced discrimination with respect to potassium occurs all along the ecological cycle.

Ecological Incorporation and Discrimination.--Although it appears probable that cesium-137 is not entering the food chain via the soil, the ecological transport mechanism (Fig. 6), which was proposed in an earlier paper (45), includes a soil-to-plant step. The model is based on the best cesium-137 to potassium discrimination factors that could be found at the time. It should be pointed out, however, that they may not be very accurate, especially the factor from soils-to-plants which (as in the case of strontium-90 and calcium) is highly uncertain for a variety of reasons. If cesium-137 is entering the food cycle entirely via the soil, the model indicates that the cesium-137 to potassium ratio in the human body should be about 3 per cent of the ratio in the available soil potassium. Deposition of one mc cesium-137 per square mile is equivalent on the average to 30 $\mu\mu c$ per gram of available soil potassium. The surface deposition level in mc per square mile multiplied by 30 x 0.03 gives the predicted body cesium-137 in $\mu\mu c$ per gram potassium. Predicted body cesium-137 levels for the various regions of the world at the end of 1958 and at maximum (assuming no more tests) are given in Table 14.

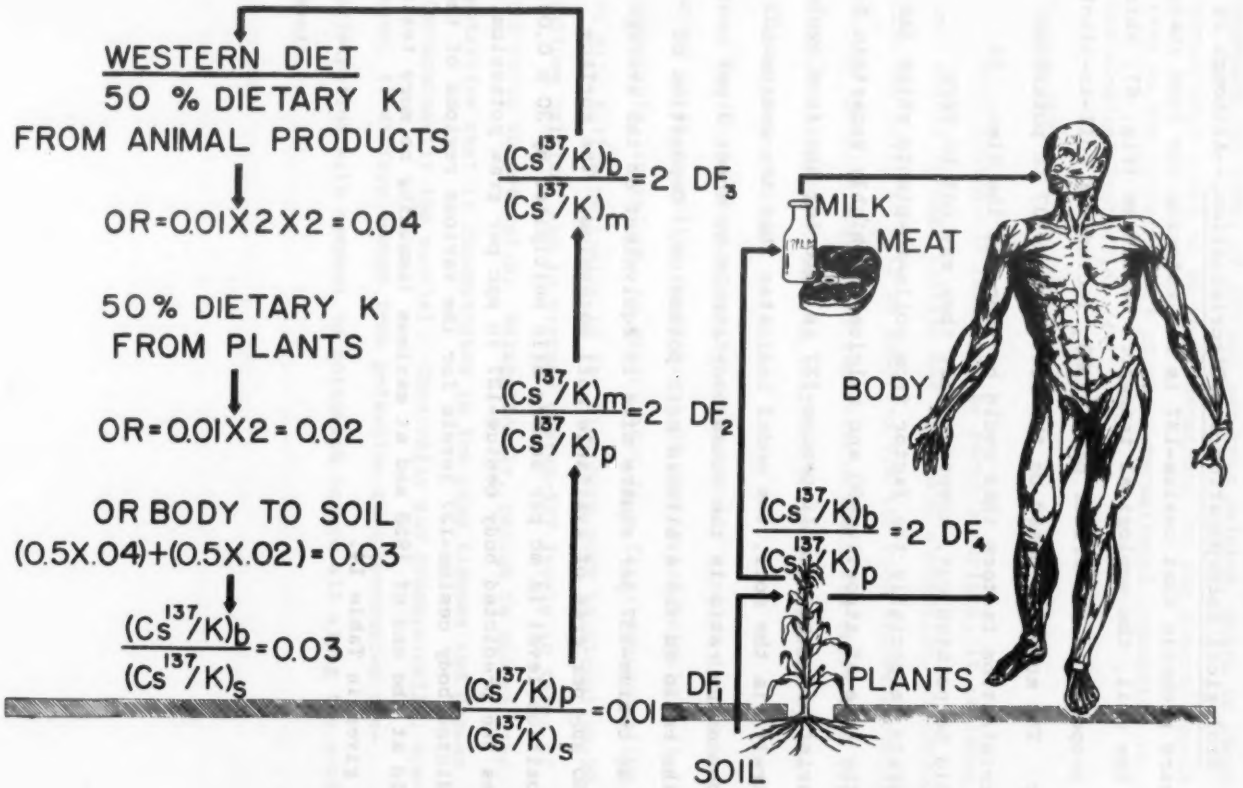

Figure 6. Possible ecological transport mechanism for cesium-137 biospheric contamination.

Table 14. Ecological Prediction[a] of Present and Future Cesium-137 Levels in the Population (Assuming No More Weapons Tests after 1958)

Region	μμc per gram Potassium	
	1958	At Maximum[b]
Average USA	81	128
Average 20°N - 60°N Latitude	57	103
Average Rest of World	12	20
Weighted World Average	48	85

[a]Assumes cesium-137 is entering food chain entirely through the soil.

[b]Maximum level will occur in about 1962 to 1965.

Since cesium-137 has not had time to come into equilibrium with soil potassium and is probably entering plants largely via direct contamination, the predicted values are useful only to give some indication of what might be expected in the equilibrium state. Obviously, if cesium-137 is entering the food chain only via direct contamination of vegetation, people would be in equilibrium with the fallout rate and body cesium-137 (with no more testing) will begin dropping immediately with a half-time corresponding to the half-time of stratospheric fallout. In this case, the projected levels in Table 14 will have no meaning.

Cesium-137 in Foods.--In the spring of 1956, the Los Alamos Scientific Laboratory began an extensive survey of the cesium-137 to potassium ratio of the United States milk supply. The purpose was not primarily monitoring per se but rather the study of fallout mechanisms. Measurements are made by gamma assay in a large volume 4π liquid scintillation counter, simultaneously measuring the 0.661-Mev gamma ray of cesium-137 and the 1.46-Mev gamma ray of potassium-40. The same method was used also to measure cesium-137 to potassium ratios in people and some foods other than milk. The results of the 1956 and 1957 surveys have been reported previously (45, 46, 47), and a complete listing of all 1958 results has been issued (48).

The average cesium-137 content of dried or fresh milk samples collected from 11 stations in various sections of the United States was 24 $\mu\mu$c per gram potassium (46) during 1956. The number of samples and locations, however, were too small to constitute a good general average of the nation's milk supply. During 1957, dried milk was routinely sampled from 33 locations within the United States, and a few spot-checks were made on foreign milk (47). A total of 887 measurements were made. The 1957 levels of cesium-137 in milk from the various parts of the United States are shown in Fig. 7. Changes in cesium-137 in milk from a given locality throughout the year showed indications of seasonal variations on which was superimposed transient peaks due to tropospheric

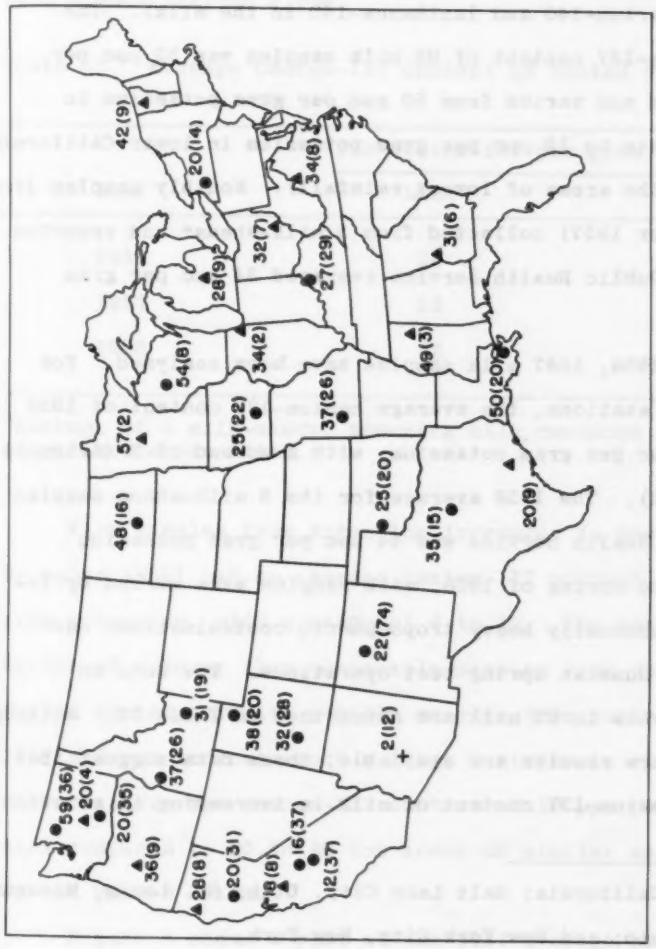

Figure 7. Geographic distribution of cesium-137 levels in United States milk during periods of 1957 which were free from tropospheric fallout, expressed as μμc per gram potassium (numbers in parentheses represent total samples analyzed).

contamination during periods of weapons testing (proved by the presence of barium-140 and lanthanum-140 in the milk). The average cesium-137 content of US milk samples was 32 μμc per gram potassium and varied from 59 μμc per gram potassium in Washington state to 12 μμc per gram potassium in lower California and Arizona (the areas of lowest rainfall). Monthly samples (from May to December 1957) collected from 5 milk-sheds* and reported by the U. S. Public Health Service averaged 34 μμc per gram potassium.

Through 1958, 1887 milk samples have been analyzed. For United States stations, the average cesium-137 content of 1958 milk was 42 μμc per gram potassium, with a spread of 2 (Arizona) to 70 (Vermont). The 1958 average for the 5 milk-sheds sampled by the Public Health Service was 44 μμc per gram potassium. During the late spring of 1958, milk samples were marked by indications of unusually heavy tropospheric contamination, due presumably to Russian spring test operations. The data on cesium-137 levels in US milk are summarized in Table 15. Although only three years results are available, these data suggest that the average cesium-137 content of milk is increasing in relation

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*Sacramento, California; Salt Lake City, Utah; St. Louis, Missouri; Cincinnati, Ohio; and New York City, New York.

to the level of integrated fallout (as in strontium-90) but with a much flatter slope.

Table 15. Average Cesium-137 Content of United States Milk

Year	μμc Cesium-137 per gram Potassium	
	LASL	USPHS[a]
1956	24	--
1957	32	34
1958	42	44

[a]Average of 5 milk-sheds, assuming milk contains 1.4 grams of potassium per liter.

Five samples from Argentina (produced in November and December 1956) had an average cesium-137 content of 9 μμc per gram potassium, with a range of 4 to 12. The average was a factor of 4 lower than comparable samples from the northern United States. Twelve monthly samples from Australia during 1957 did not indicate such a large hemispheric effect and showed an average cesium-137 content of 18 μμc per gram potassium compared to 20 to 40 for areas of similar annual rainfall in the United States.

Figure 8 shows a plot of the 1957 average cesium-137 milk levels at the various sampling points against total precipitation. The milk average is computed for those periods in which

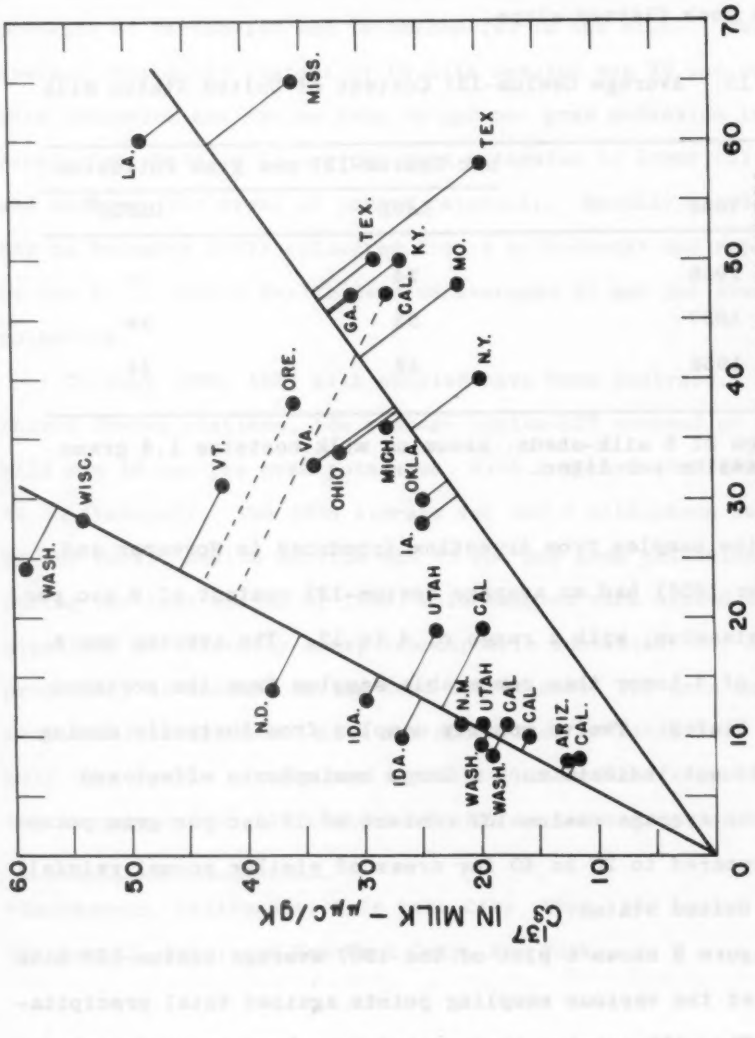

Figure 8. Correlation of 1957 cesium-137 levels in milk with total rainfall.

tropospheric contamination was absent as indicated by the absence of barium-140. The months of June, July, September, and October were principally affected. The data appear to divide into two groups, each group showing a fair correlation between cesium level and rainfall, but with pronounced difference in slope. One group includes the far western and northernmost states; the other the midwest, south, and east.

Figure 9 shows the same data expressed as μμc cesium-137 per gram potassium per inch of precipitation superimposed on a map of the United States. A line from El Paso, Texas, to Minneapolis, Minnesota, and then east divides the country into two regions, one having a higher value of cesium-137 per gram potassium per inch of rainfall and the other having a lower value. (Two outstanding anomalies in the former region are Portland, Oregon, and Fernbridge, California.*) The two regions suggest a possible reflection of different meteorological patterns. The manner in which the tropospheric air masses principally responsible for precipitation approach or traverse the latitudes of high

with total rainfall.

*The anomalous results in these regions may be explained on the basis of orographic precipitation. The prevailing onshore winds are lifted by the coastal ranges, causing clouds and precipitation. Such clouds do not normally extend to as great heights as those associated with the cyclonic storms dominating the precipitation patterns elsewhere in the western states. This should result in less scavenging per inch of rainfall of radioactivity from the higher concentrations found aloft. (We are indebted to Dr. Robert J. List of the U. S. Weather Bureau for this interpretation.)

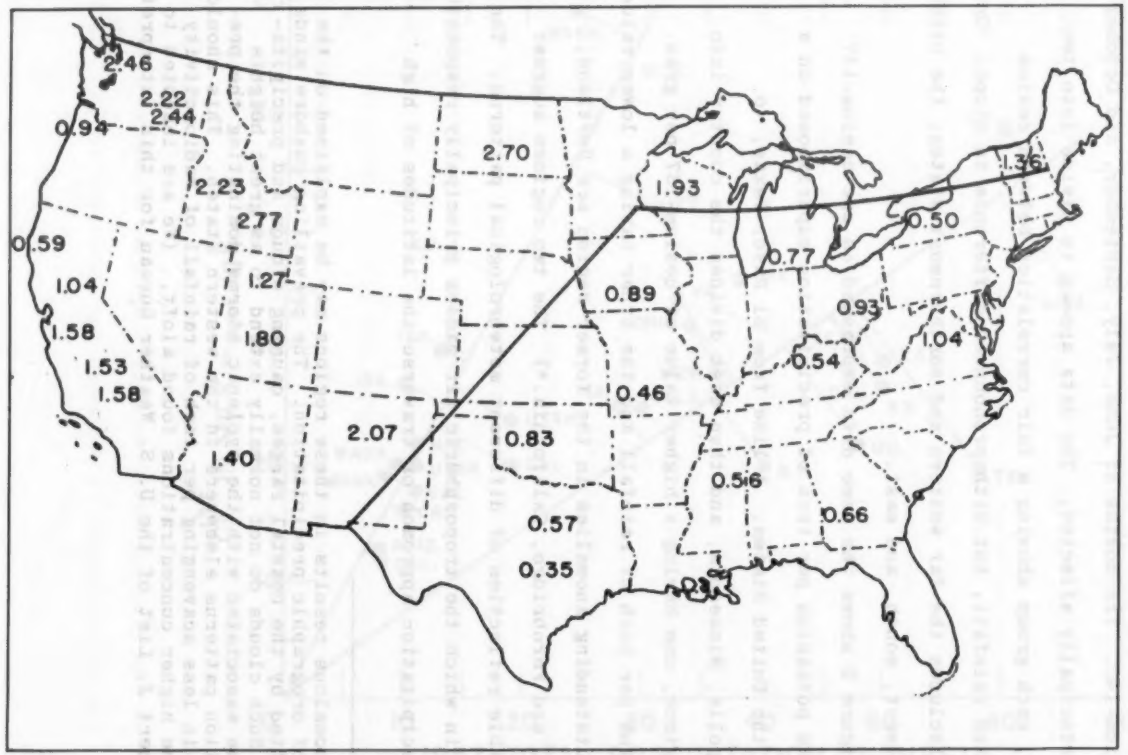

Figure 9. Geographic distribution of cesium-137 levels in United States milk during 1957 (μμc cesium-137 per gram potassium per inch of rainfall).

stratospheric leakage and the sizes of the air masses which mix to produce a given quantity of precipitation may be factors. A similar correlation between strontium-90 fallout and rainfall has been pointed out by Machta (49) and Collins and Hallden (50).

No new data are available for the cesium-137 content of foods other than milk since the last Hearings (1). Some indication of the cesium-137 content of the general 1957 United States diet, excluding milk, may be inferred from a plot of 1957 milk values against the average cesium-137 content of people from the same areas for the same time period (Fig. 10). These data show a definite positive correlation between the cesium-137 activity in milk and in people. The slope of the regression line is 0.7. If about 40 per cent of dietary potassium is derived from milk, then the discrimination factor [i.e., the ratio of $(\text{Cesium-137 to Potassium})_{man}$ to $(\text{Cesium-137 to Potassium})_{milk}$] for cesium-137 from milk-to-man is 1.8. Extrapolation of the line to no cesium in milk indicates that about 23 μμc cesium-137 per gram potassium in people must have been derived from the rest of the diet. Assuming 60 per cent of dietary potassium from non-milk sources and a discrimination factor of 1.8, the cesium-137 to potassium ratio in the rest of the diet must have been about 21 μμc per gram.

Another estimate of the discrimination factor can be obtained from the estimate of average dietary cesium to potassium given in

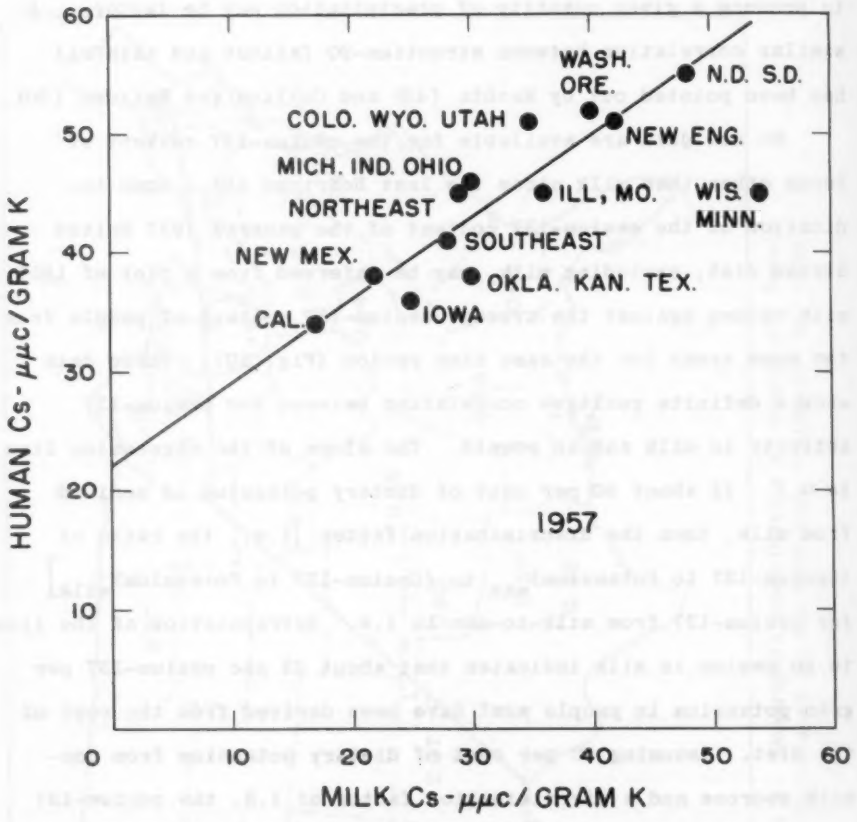

Figure 10. Correlation of cesium-137 levels in the United States population with levels in milk (1957).

Table 16. Since some uncertainty attaches to the average cesium to potassium ratios given in column three, the resulting dietary average is only approximate. However, combining this dietary value with the observed human average of about 40 μμc per gram during the same period indicates a discrimination factor of about 2. The data also show that about 60 per cent of body cesium-137 comes from milk and about 25 per cent from meat.

Table 16. Estimated Cesium-137 Content of Average Diet

Source	Dietary Potassium (per cent)	Average Cs/K 1956-1957 (μμc/g)	Contribution to Dietary Cesium (μμc/g K)	Contribution to Dietary Cesium (per cent)
Dairy Products	38	32	12	60
Meat	15	32	.5	25
Flour and Cereals	6	24	1.4	7
Vegetables	18	5	0.9	5
Citrus Fruits	3	25	0.7	3
Potatoes	19	0	0	0
Average Cesium-to-Potassium			20	100

Miller and Marinelli (51) estimated a dietary discrimination factor of about 2 on the basis of the ratio of excretion to body burden. The dietary discrimination factor calculated from the

ratio of the biological half-times of cesium and potassium (discussed later) is about 3.5.

Metabolism of Cesium-137 in Man and Animals.--The absorption, retention, and excretion of orally administered tracer doses of cesium-134 or cesium-137 were studied in five animal species using *in vivo* whole-body counting methods. Absorption of radiocesium by mice, rats, dogs, monkeys, and man was essentially 100 per cent. The retention equations for cesium in the various species are not simple exponentials but are multiple rate functions with three principal components (only two established so far for man). The radiocesium retention equation for man over at least 650 days was

$$R_t = 16\, e^{\frac{-0.693}{3}t} + 84\, e^{\frac{-0.693}{143}t}$$

where R_t is the per cent retained, and t is the time after administration in days. From analogy with the other species, a third component (representing a small fraction of the dose) to the rate equation for man may be expected at a later time. The biological half-time of the major component of the cesium retention function is 143 days, and the corresponding half-time for potassium retention has been estimated as about 40 days (52). The discrimination factor for cesium-137 with respect to potassium (under conditions of chronic exposure) in going from diet-to-man should, therefore, be 143/40 = 3.5, which is not consistent

with the value of about 2 derived from the measurements and dietary considerations given earlier.

An explanation for this apparent anomaly is that potassium may not have a unique half-time in the body. The turnover time of potassium, like that of body water, may be governed entirely by the intake rate, the amount retained being homeostated by the minimum and maximum requirements of the body. Cesium-137, on the other hand, appears to be excreted at a constant rate independent of the potassium intake rate so long as the latter does not exceed reasonable limits (53, 54).

Hood and Comar (55) reported retention half-times for cows (~20 days) which were considerably shorter than those observed for man. Their data indicate ruminants have an unusually high fecal excretion rate compared to single-stomach species and lactating cows lose sizeable quantities in milk (~12 per cent in 30 days). In the absence of specific data, there is no choice but to assume that cesium and potassium would act similarly in ruminants and that the discrimination factor for cesium with respect to potassium in going from diet-to-tissues of domestic animals would be comparable to that for other species.

Cesium, like potassium, is retained principally in muscle. Rats sacrificed 5 to 20 days after oral administration of cesium-134 had 80 per cent of their total body burden in the muscle mass (52). Apparently about 80 per cent of the body burden is

confined to the muscle mass in cattle, sheep, swine (55) and man (52) also. Concentration of radiocesium in the gonads of rats two days after administration was about 70 per cent of that in muscle, and its rate of elimination from reproductive tissues was faster than from muscle.

Cesium-137 Levels in People.--The cesium-137 level in the United States population has been studied at the Los Alamos Scientific Laboratory since the spring of 1956 and at the Walter Reed Army Institute of Research since 1958. The method used is *in vivo* whole-body counting in a 4π liquid scintillation counter. Discriminator settings enable simultaneous determination of cesium-137 and potassium-40, and consequently the determination of the total body cesium-137 to potassium ratio. In 1956, 196 determinations were made on people from 27 states and during 1957, 311 determinations were run on people from 30 states. During 1958, a total of 793 persons from 34 states were measured at LASL (48) and 656 at WRAIR (56). Some foreign data were obtained through measurement of 132 persons at WRAIR and 33 at LASL, who were recently arrived in the United States from abroad. Summaries of data of previous years have been published (45, 46, 47). Table 17 recapitulates results previous to 1958. The average cesium-137 to potassium ratio in the United States population was 41 μμc per gram during 1956 and 44 μμc per gram in 1957. The average during 1958 was 56 μμc per gram. There appeared to be no striking

Table 17. Cesium-137 Levels in People (Prior to 1958)

Year	Region	Number of Subjects	Cesium-137 (μμc/g K)	Reference
1955	USA (Chicago)	12	10	(51)
1956	USA	196	41	(46)
1956	USA (Chicago)	12	35	(57)
1957	USA	311	44	(47)
1957	USA (Chicago)	13	35	(57)
1957-1958	USA (Utah)	42	66	(58)
1956	England	2	35	(51)
1956-1957	England	16	34	(59)
1957	England	2	50	(45)
1956-1957	Europe	3	31	(51)
1957	Europe	5	55	(45)
1956	Asia	5	17	(51)
1957	Asia	3	33	(45)
1957	South America	11	15	(51)

difference between the 1956 and 1957 averages. However, there was a significant increase in the 1958 levels. The 1959 data (to April 1) indicate little if any change (53 μμc per gram potassium on 63 subjects from 15 states). The frequency

distribution curves for each of the four years appears normal, with a standard deviation of 36 per cent. The 1957 cesium-137 data for the United States population as a function of geographic location are shown in Fig. 11. The pattern is similar to that observed for milk but with a smaller range, which is in keeping with expectations because of the rather wide food distribution system in the United States. The correlation of 1957 cesium-137 data for people with that for milk was shown earlier in Fig. 10, and a positive correlation is definitely indicated. This also may be expected since milk is unique in its localized consumption.

A similar comparison of different areas in the United States is possible using the 1958 data in Table 18. Because of larger areas over which the data are averaged, the range is further reduced. The WRAIR results are about 20 per cent higher than those reported by LASL. This is probably due to calibration differences and will be easily resolved by intercalibration of the two counters with suitable standards. The possibility of a systematic or absolute error is not eliminated by this procedure. A suggestion of a systematic difference between the crystal spectrometer and the liquid scintillator results appears in Table 17, by comparing the LASL and ANL results.

Data for persons outside the United States are to be found in both Tables 17 and 18. The problem of representative sampling is much greater in this case, since persons traveling to the

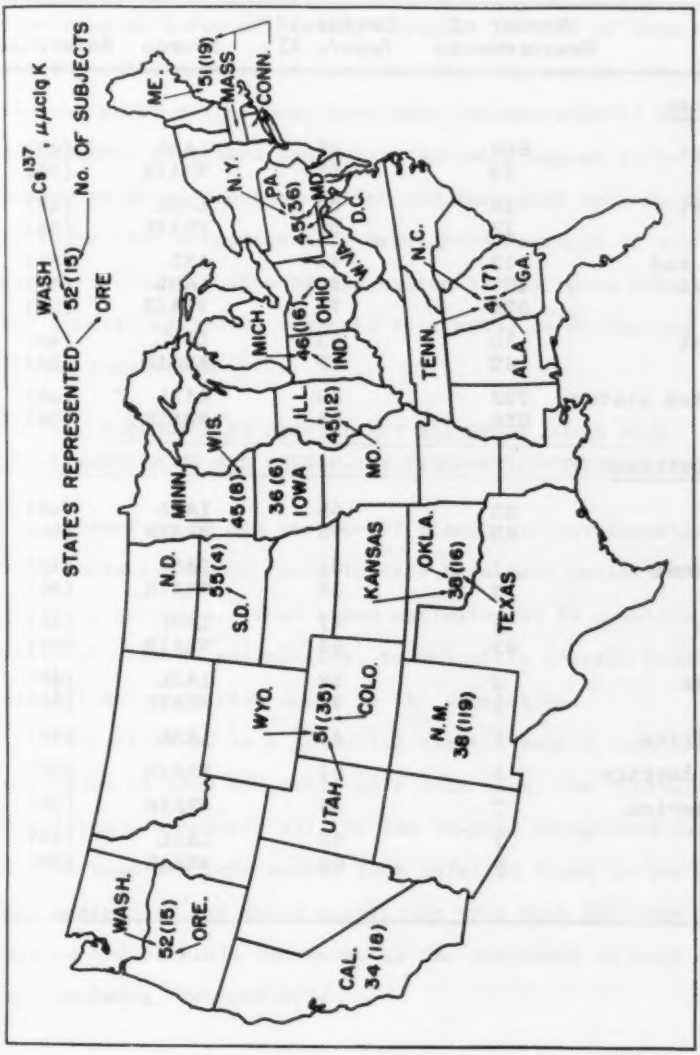

Figure 11. Geographic distribution of cesium-137 levels in the United States population (1957).

Table 18. Cesium-137 Levels in People (1958-1959)

Region	Number of Measurements	Cesium-137 (μμc/g K)	Source	Reference
United States:				
Southwest	658	48	LASL	(48)
	72	63	WRAIR	(56)
Northwest	18	59	LASL	(48)
	13	59	WRAIR	(56)
Midwest and	12	48	ANL	(57)
Northeast	107	57	LASL	(48)
	529	70	WRAIR	(56)
Southeast	10	63	LASL	(48)
	42	73	WRAIR	(56)
All United States	793	56	LASL	(48)
	656	68	WRAIR	(56)
Foreign Countries:				
Europe	23	58	LASL	(48)
	65	78	WRAIR	(56)
Middle East	1	52	LASL	(48)
	3	58	WRAIR	(56)
Far East	3	47	LASL	(48)
	43	64	WRAIR	(56)
Australia	2	58	LASL	(48)
	1	75	WRAIR	(56)
South Africa	2	44	LASL	(48)
Central America	5	62	WRAIR	(56)
South America	7	44	WRAIR	(56)
Canada	2	53	LASL	(48)
	8	68	WRAIR	(56)

United States probably do not represent true averages for their countries in terms of dietary habits. In view of this difficulty and the small numbers of persons measured, one can conclude only that no great differences have been demonstrated in the northern hemisphere. Southern hemisphere subjects appear to be somewhat lower, as pointed out by Miller and Marinelli (51) on the basis of their 1957 measurements. Unweighted averages of the data given in Table 18 show 59 μμc cesium-137 per gram potassium for 165 foreign subjects, compared to a value of 60 for the United States population.

BIOLOGICALLY SIGNIFICANT RADIATION DOSES FROM STRONTIUM-90 AND CESIUM-137 BIOSPHERIC CONTAMINATION

Strontium-90 and cesium-137 biospheric contamination levels from nuclear weapons tests to date have been estimated in the previous sections. From these estimates it is possible, on the basis of several assumptions, to calculate average biologically significant radiation doses to the population.

Strontium-90 is a potential somatic hazard and, since it concentrates in bone and emits only beta rays, the biologically significant exposures will be the 70-year integrated radiation doses to the bone and bone marrow from material fixed in the skeleton. The hazards to the world population from bone and bone marrow exposure are possible increases in the incidence of bone sarcoma and leukemia, respectively.

Cesium-137, however, emits gamma radiation and, when ingested, is absorbed and deposited predominantly in muscle. It contributes, therefore, to radiation exposure both internally and externally. Although it will contribute also to the bone and bone marrow exposure, the 30-year integrated dose to the gonads is considered, for genetic reasons, biologically more significant.

Radiation Exposure from Strontium-90

Homogeneously distributed, 1 μμc of strontium-90 per gram bone calcium delivers a radiation dose rate to the bone of approximately 2.9 mrads per year. The dose rate to the marrow has been estimated as about 1 mrad per year per μμc, or about one-third of that to the bone (2). From the equilibrium bone levels given in Table 13, it is possible to estimate the average maximum strontium-90 70-year bone and bone marrow doses to be sustained by the generation being born at the present time. The 70-year integrated dose to the bone will be about 96 per cent of that calculated for a constant strontium-90 bone concentration equal to the average equilibrium level as of October 1958. The 70-year marrow dose will be about one-third of the dose to the bone.

These estimates (given in Table 19) are predicated on the assumptions of no more weapons tests and that the level of strontium-90 in the diet will be proportional to the integrated

Table 19. Average Maximum Strontium-90 70-Year Bone and Bone Marrow Doses from Weapons Tests to Date

Region or Population	70-Year Dose (mrads)*	
	Bone	Bone Marrow
USA	740	250
Western Population	610	200
20°N - 60°N Latitude**	980	330
Far Eastern Population	1220	410
Weighted World Average**	850	290

*Assuming no more weapons tests.

**Weighted for population and diet.

soil levels. The approximations are maximized, since they do not take into account the change in rate of skeletal growth with age but assume that all bone calcium is in equilibrium with the level of biospheric contamination at all times. The world average natural background radiation dose rate to the skeleton is estimated at 134 mrem per year (60), and the dose rate to the bone marrow at about 100 (2). In round numbers, the 70-year average bone and bone marrow doses from natural background are equivalent to about 10,000 and 7,000 mrads, respectively, of hard beta or gamma radiation.

The data in Table 19 suggest that the population-weighted world average 70-year bone dose from strontium-90 (assuming no more weapons tests) may approach a maximum that is about 8.5 per cent of the natural background dose. Far Eastern populations may approach 12 per cent of natural background. The population-weighted world average 70-year bone marrow dose may approach a maximum that is about 4 per cent of that from natural sources. The average maximum 70-year bone and bone marrow doses in the US population may approach 7.5 and 3.5 per cent, respectively, of that from natural background. If the past 5-year weapons test pattern is repeated in the next 5 years, the values in Table 19 will be increased by a factor of about 1.8. Repetition of the past 5-year pattern indefinitely will result in an equilibrium between biospheric contamination and the testing rate. At equilibrium, the bone and bone marrow doses will be about 8 times those estimated from weapons tests to date.

Radiation Exposure from Cesium-137

The external gamma radiation dose rate from cesium-137 surface deposition may be estimated from the following expression:

$$\text{mr per year} = 5 \times 10^{-2} \times C \times E$$

where C is the cesium-137 deposition level in mc/mi^2, and E is the gamma-ray energy (0.66 Mev). This expression gives the dose rate

at about 3 feet above an infinite plane surface in the open. It does not allow for the effect of shielding from buildings during time spent in-doors or removal of the cesium-137 from the surface by leaching into the soil, weathering, or run-off. It is virtually impossible to consider all parameters involved in making a realistic estimate of an average external population dose from cesium-137 surface deposition. If, however, the shielding, leaching, and weathering factor of 1/10th (2) is used, the above expression and the soil deposition data in Table 6 and Fig. 3 can be used to give a crude estimate of the average external 30-year genetic and 70-year bone marrow doses to be sustained by the next generation as a result of weapons tests to date (Table 20).

Table 20. Predicted Average External Cesium-137 30-Year Genetic and 70-Year Bone Marrow Doses to the Next Generation as a Result of Weapons Tests to Date

Region	mrads	
	30-Year	70-Year
USA	10	16
20°N - 60°N Latitude	8	13
Rest of World	2	3
World Average*	8	12

*Population-weighted but not weighted for dietary differences between Far Eastern and Western cultures.

The genetically significant dose was assumed to be approximately 53 per cent of the infinite dose at the time of maximum fallout. The bone marrow dose was assumed to be 83 per cent of the infinite dose. It should be pointed out that these data apply only to the external exposure from cesium-137 and do not take into consideration external dose from shorter lived fission products in fallout. If weapons tests stop, the shorter lived isotopes may increase the external dose by 10 to 50 per cent. In view of the shorter fallout mean-time for fission debris from USSR tests, continuation of testing may result in larger external doses from short-lived activities than from cesium-137.

The internal radiation dose rate from present and future levels of cesium-137 in people may be estimated from the following expression derived from the Recommendations of the International Commission on Radiological Protection (61):

$$R = \frac{1.86 \times 10^{7} \, f_2 \sum E(RBE)N \, q}{m} = 1.57 \times 10^{2} \, q$$

in which R is the dose rate in mrem per year, q is the cesium-137 body burden in μc, m is the weight of the critical organ (in this case, 7×10^{4} grams), f_2 is the fraction of the total body burden that is in the critical organ (1.0), and $\sum E(RBE)N$ is an average energy term weighted for the relative biological effectiveness and absorption of the radiation (0.59).

The measured average cesium-137 level in the US population during 1958 was about 60 μμc per gram potassium, and the adult human body contains 140 grams of potassium, giving a cesium-137 total body burden of about 0.008 μc. From the above formula, this corresponds to a radiation dose rate of about 1.3 mrads per year at a time when the integrated cesium-137 surface deposition level in the US averaged about 90 mc/mi^2. This corresponds to an internal dose rate of 0.014 mrad per year per mc/mi^2.

Assuming the worst possible condition, that is that body burden is proportional to integrated surface deposition level, the average maximum 30-year genetic and 70-year bone marrow doses from internally-deposited cesium-137 (to be experienced by the next generation) can be predicted from the surface deposition levels given in Table 6. Such estimates are given in Table 21. The method of estimation is the same as that used for external cesium-137 doses, assuming the doses to the gonads and bone marrow are the same as that to the total body.

If, however, the cesium-137 body levels are proportional to the rate of stratospheric fallout and not to the integrated deposition levels, they will start dropping immediately with a half-time of about 2 years, and the respective integrated genetic and bone marrow doses to the present and next generation will be only about 10 and 5 per cent of those given in Table 21. It is not possible at present to determine which of the two conditions is more nearly correct.

Table 21. Predicted Average Internal Cesium-137 30-Year Genetic and 70-Year Bone Marrow Doses to the Next Generation as a Result of Weapons Tests to Date*

Region	mrads	
	30-Year	70-Year
USA	43	65
20°N - 60°N Latitude	35	53
Rest of World	8	12
World Average**	30	45

*Assuming cesium-137 body burden will be proportional to integrated surface deposition levels.

**Population-weighted but not diet-weighted.

Although biospheric contamination was increased about 80 per cent in 1958, the integrated internal doses from cesium-137 are essentially the same as those given in an earlier paper (45), assuming tests stopped in 1957. This apparent discrepancy results from use (in the present report) of the latest estimates of stratospheric content and fallout mean-time.

POTENTIAL HAZARDS OF PRESENT STRONTIUM-90 AND CESIUM-137 BIOSPHERIC CONTAMINATION LEVELS TO WORLD HEALTH

A summary of the predicted population-weighted world average genetic, bone, and bone marrow doses as a result of strontium-90 and cesium-137 biospheric contamination from weapons tests to date is given in Table 22. The data, based on entry of both long-lived nuclides into man's food chain in accordance with integrated surface deposition levels, probably represents maximum potential exposure of the next generation. Under this condition, the genetically significant dose is about 1.3 per cent of the average natural background dose. The potential bone and bone marrow doses are about 9 and 5 per cent, respectively, of those from natural background.

Genetic response to radiation is generally believed to be linear with dose and, when delivered at conventional rates, 30 to 50 r is believed to be the dose required to double the natural mutation frequency (62, 63). The fraction of the natural frequency that results from background radiation is not known. Since the 1957 Hearings, the most outstanding results regarding the potential genetic effects of background and fallout radiation are those reported by Russell (64), who found that mutation frequency in spermatogonia and oocytes of mice was influenced by dose rate. From this extremely important discovery, it may be concluded that the 30-year dose from background and from fallout may have considerably less genetic

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Table 22. Integrated Population-Weighted World Average Genetic, Bone, and Bone Marrow Doses from Weapons Tests to Date*

	30-Year Genetic Dose (mrads)			70-Year Bone Marrow Dose (mrads)			70-Year Bone Dose (mrads)		
Conditions	Internal Cs-137	External Cs-137	Total Cs-137	Cs-137**	Sr-90	Total	Cs-137**	Sr-90	Total
Soil Integrated	30	8	38	57	290	347	57	850	907
Direct Uptake***	3	8	11	17	290	307	17	850	867

*From strontium-90 and cesium-137 only.

**Internal plus external dose.

***Assuming strontium-90 body burden will be proportional to surface deposition and cesium-137 burden will be proportional to deposition rate.

consequences than had been predicted previously. Although biospheric contamination was essentially doubled in 1958, it is possible, in view of these new observations, that the potential genetic risk from tests to date is only about one-half that predicted at the first Hearings for weapons tests to mid-1957. The discovery that mutation frequency is dose-rate dependent, however, does not invalidate the linear hypothesis, and it cannot be assumed from present knowledge that further lowering of the dose rate will result in still less potential genetic risk.

In any event, the predicted dose rate and the integrated 30-year gonadal dose from present levels of biospheric contamination are between 1 and 2 per cent of that from natural background, and it may be concluded that the potential genetic risk to the world population from weapons tests to date will not be more than 1 to 2 per cent of the unavoidable risk from natural sources of radiation.

Estimation of the potential somatic consequences of biospheric contamination from weapons testing is even more difficult and uncertain than the estimation of genetic effects. The absolute somatic hazards imposed on the world population by the bone and bone marrow doses estimated in Table 22 are critically dependent on the shape of the dose-response curves for leukemia and bone sarcoma and, as with mutation frequency, the extent to which they are dose-rate dependent. If somatic response shows a curvilinear

relationship with dose, a factor (or factors) other than 70-year integrated exposure must be important, and biological effect may drop to insignificant levels at very low doses and dose rates. Although the existence of an absolute threshold may never be proved empirically (65), the existence of a practical one may be inferred from a curvilinear dose-response relationship. If, however, the linear proportionate hypothesis is accepted, any amount of radiation (no matter how small) carries a finite probability of damage. In this case, a fraction of the natural population incidence of all radiation-induced somatic diseases must be attributed to natural background.

Lewis (66) and Court-Brown and Doll (67) have advanced the hypothesis that leukemogenic response of man to radiation is linear with dose. Lewis' conclusions were based largely on reports of leukemia incidence in the Japanese following the bombings of Hiroshima and Nagasaki, and Court-Brown and Doll's were based on studies of leukemia incidence in several hundred cases of X-ray treated ankylosing spondylitis. The latter authors suggested that a radiation dose of 30 to 50 r to the red bone marrow may double the leukemia rate and that the probability of developing leukemia lies between 1×10^{-6} and 2×10^{-6} per year per r of exposure.

The linear hypothesis of somatic radiation damage has been questioned recently by Mole (65, 68), Brues (69), and Finkel (70).

Mole pointed out the possible presence of a systematic error (the size of which becomes greater with increasing dose) in the mean population dose estimates to the Japanese which, if corrected, makes the dose-response relationship more likely curvilinear. Court-Brown himself (71), Brues, and Mole have pointed out that the data from the spondylitic cases are compatible with a marked curvilinear relationship, if unselected for those cases receiving spinal irradiation only.

Mole (68) has reported preliminary data which suggest also that radiation induction of leukemia in mice may be dose-rate dependent, and pointed out that the applicability of the data collected from the Japanese survivors and the spondylitic patients (in which the dose rates were of the order of a million times greater than that from fallout) to the prediction of the potential leukemia hazard from slightly increased background depends on whether dose rate is a relevant parameter. Brues (72) has especially made a point of the fact that the linear proportionate hypothesis for radiation-induced cancer requires a simple cause-effect (point mutation, single hit) relationship. Such a simple explanation of cancer production is incompatible with current theories and a vast amount of experimental evidence reported by specialists in cancer research during the past several years. For more detailed discussions of the potential somatic hazards of world-wide fallout, see the testimonies given during the present Hearings by Brues (72), Law (73), and Schubert (74).

It is not possible at present to estimate the increased number of cases of leukemia and bone cancer that may be expected from weapons tests to date. If indeed a fraction of the normal incidence of these diseases in the world population is caused from natural background radiation, it is possible to say from the data in Table 22 that weapons tests to date may increase their incidence by 5 to 10 per cent of that due to the unavoidable average natural radiation level.

SUMMARY

1. Nuclear weapons tests through 1958 have produced between 9 and 10 MC of strontium-90 and between 16 and 17 MC of cesium-137. Of the total production, about one-third had been deposited as long-range fallout, about one-third was still in the stratospheric reservoir, and about one-third had been deposited locally (where it does not contribute to generalized biospheric contamination) as of November 1958.

2. The mean-time of stratospheric fallout of debris injected in the mid-latitudes is now believed to be about 3 years. Material injected at temperate and polar latitudes (as with the USSR tests) may have a fallout mean-time of a year or less.

3. Because of the location of major test sites, long-range strontium-90 and cesium-137 fallout has been and is being preferentially distributed in the north temperate latitudes, where

about 80 per cent of the world population lives. Present and predicted average levels of deposition in the area between 20°N - 60°N latitude from tests to date are between 2 and 2.5 times the world average. Maximum deposition in this area will occur in 1962-1965, at which time the strontium-90 level may average about 64 mc/mi^2 and the cesium-137 level about 115 mc/mi^2. The respective average levels in the United States may be about 80 and 140 mc/mi^2.

4. If the past 5-year test pattern is repeated exactly during the next 5 years, the potential strontium-90 and cesium-137 biospheric contamination levels (including the levels in man and his diet) may be increased by a factor of approximately 1.8. If the pattern is repeated every 5 years for the next 40 years, an equilibrium level of biospheric contamination will be approached that will be about 8 times the level predicted from weapons tests to date.

5. Analyses of milk and other foods suggest that hot spots and other factors may result in strontium-90 and cesium-137 concentrations in principal foodstuffs from localized areas that are 3 to 5 times the average for large general and latitudinal areas.

6. Based on analyses of human bone samples to date, the predicted average strontium-90 equilibrium level (from weapons tests to date) in the bones of the world population, weighted for dietary factors and population distribution, may approach 7 μμc per gram calcium. This value is approximately 10 per cent of the level

suggested as a maximum for the general population by the International Commission on Radiological Protection. The actual bone data suggest a log-normal distribution with a few values that are 5 to 10 times the mean. Inherent in the data, however, are problems of distribution, sampling, and analytical errors, which tend to increase the variability of the observed results. For practical considerations of the potential hazard of strontium-90 bone deposition to world health, the assumption of a spread of 5 times the mean with an upper limit of 10 would seem to be realistic.

7. The 1958 average cesium-137 level in people living between 20°N - 60°N latitude (constituting about 80 per cent of the world population) was about 60 μμc per gram potassium, or a total of about 0.008 μc in the adult male. Assuming cesium-137 in people will increase in proportion to the integrated surface deposition, the cesium-137 level in the north temperate population from weapons tests to date may reach a maximum of about 100 μμc per gram potassium in 1962-1965. It is not possible at the present time, however, to decide whether cesium-137 body burden is directly proportional to integrated deposition level or to rate of stratospheric fallout. In the latter case, the cesium-137 levels in people will start dropping immediately with a half-time of approximately 2 years, if weapons testing is not resumed.

8. The 70-year population-weighted world average strontium-90 plus cesium-137 bone and bone marrow doses to be incurred by the next generation as a result of weapons tests to date may approach levels that are 10 and 5 per cent, respectively, of those received from average natural background radiation.

9. The population-weighted world average 30-year genetic dose from both internally and externally deposited cesium-137 as a result of weapons tests to date may be between 1 and 2 per cent of the dose from average natural background.

10. Estimation of the potential genetic and somatic consequences of these levels of exposure of the world population cannot be made with any reasonable degree of certainty. The recent discovery that mutation frequency is affected by dose rate suggests that the genetic consequences of fallout and background radiation exposure may be considerably less than previously supposed. It can be said, however, that the number of genetic abnormalities from weapons tests to date will not be more than 1 to 2 per cent of those produced by the unavoidable average natural background exposure. The question of whether such somatic radiation diseases as bone cancer and leukemia show a linear or curvilinear relationship to radiation dose is still unanswered, as well as the question of their dose-rate dependency. It is not possible, therefore, to convert the predicted strontium-90 plus

cesium-137 bone and bone marrow doses into the number of tumors, cases of leukemia, etc., that will result from past and future weapons tests. It is possible only to say that if a fraction of the natural incidence of these diseases in the world population indeed is caused by natural background radiation, weapons tests to date may increase their incidence by 5 to 10 per cent of that due to the unavoidable average natural radiation level.

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SUMMARY

This report deals with the principal work conducted on strontium-90 from nuclear fallout at the Lamont Geological Observatory since the previous hearings in 1957. A separate report dealing with the carbon-14 aspects of the problem is being submitted by Professor Wallace Broecker of our laboratory. Four documents are attached as follows:

1. Reprint of paper entitled 'Strontium-90 in Man, II', Walter R. Eckelmann, J. Laurence Kulp, Arthur R. Schulert: Science 127, 266-274 (1958)

2. Reprint of article 'Comparative Metabolism of Strontium and Calcium in Man', Arthur R. Schulert, Edwin A. Peets, Daniel Laszlo, Herta Spencer, Martin Charles and Joseph Samachson: International Journal Applied Radiation and Isotopes 4, 144-153 (1959)

3. Manuscript of a paper scheduled for appearance in the June 1959 issue of Health Physics entitled 'Strontium-90 Distribution in the Human Skeleton', Arthur R. Schulert, E. J. Hodges, E.S. Lenhoff and J.L. Kulp

4. Summary of a paper presented at the 135th meeting of the American Chemical Society, Boston, Massachusetts, April 7, 1959, entitled 'Comparative Hazards of Radioisotopes in Nuclear Fallout', Arthur R. Schulert

A fifth document entitled 'Strontium-90 in Man, III', J. Laurence Kulp, Arthur R. Schulert, Elizabeth J. Hodges, will appear in this week's issue of Science, May 8, 1959. Copies of this issue will be available during the Hearings.

A sixth relevant document is 'Current Strontium-90 Level in Diet in United States', J. Laurence Kulp and Rieta Slakter: Science 128 85-86 (1958). Reprints of this article have been exhausted.

I Review of Principal Findings

A. Bone Concentrations

1. Data are now in hand so that the annual increase in strontium-90 in human bone over the past few years is well defined. The highest values are found in the 0 - 4 year age group (which incidentally are the most difficult samples to obtain). The average concentration

in 1957-58 in the bones of these young children living in North America was 1.38 S.U. (micromicrocuries Sr^{90}/gram Ca), compared to 0.56 for 1955-56. The comparative adult values were 0.19 and 0.07 S.U. respectively.

2. It is noted that the 1957-58 concentration in young children exceeds that in adults by a factor of seven.

3. Whole body analysis of New York City adults from 1953 to 1957 show approximately a 50% increase in strontium-90 per year. These are the best samples from an analytical point of view because (a) their large size affords highest analytical precision, (b) being total body ash, the intraskeletal variations (which give higher error to the individual bone analyses) are avoided.

4. The higher concentrations are found in the Far East, with Thailand at 0.36 being the highest, this value for adults is 90% above that of North America average. Analytical data on children from Thailand is lacking, but it would be presumed they would now have an average value of about 2.6.

5. It is predicted that on the basis of tests to date, the peak level in young children will occur in 1966 at which time the average child will have 4 S.U. strontium-90. It is concluded from an evaluation of the adult data from 1956 through 1958 that approximately 10% of the population have concentrations twice the mean, 1% five times the mean, and none exceeding twenty times the mean.

6. If testing continues at a rate such that 10 millicuries of strontium-90 per square mile are deposited (this corresponds to

the amount deposited in northeastern United States in 1956-57), the average bone level of population will ultimately equilibrate at 21 S.U.

B. Intraskeletal Variation

1. Further investigation of strontium-90 in the adult skeleton is nonuniform, with vertebrae being 1.8 times body average, rib being 1.1 times body average and long bone shaft being one-half body average.

2. The concentration of joint areas in adults is generally higher than in shaft areas.

3. This is a temporal situation which will disappear in time were the diet kept constant as evidenced by the fact that stable strontium is uniformly distributed throughout the skeleton.

4. The distribution of strontium-90 in young children appears to be uniform.

C. Sr^{90} in Food

1. The average dietary level of strontium-90 in the United States in the fall of 1957 was approximately 6.5 S.U. This is based on values of 6, 10, and 15 S.U. for milk, vegetables and cereals respectively.

2. It is predicted on the basis of tests to date that the concentrations will rise to a peak value of 16 in 1966.

3. Analyses have just been completed on a large number of native foods from South America. The Sr^{90} concentration is generally a factor of 2 - 4 lower than U.S. foods.

4. The foods from South America show wide variation reflecting large differences in rainfall and soil calcium. Some foods from

high rainfall areas exceed 100 S.U.

D. Mechanism

1. It is concluded that the discrimination factor against strontium relative to calcium in going from diet to human bone is 4 on the average. This value best fits the present strontium-90 data in food and bone. It is substantiated by radiotracer studies in man (document 2). It is also the conclusion of Bryant et al. on the basis of stable strontium assay of diet and human bone (British Medical Journal 1, 1371 (1958).

2. Uptake of strontium-90 into the plant from the environment is presumed to be primarily through the soil. This conclusion is arrived at indirectly on the basis of the fact that strontium-90 is increasing and nearly somewhat in proportion to total fallout in contrast to cesium-137 which has not markedly increased in the past two years.

Both of these problems require further investigation.

II Present Research Emphasis

Increased emphasis at the present time is being placed on the assay of young children and on a broader, systematic coverage of dietary foodstuffs.

The young children are of chief interest, not only because they have the highest values but because they are probably more susceptible to radiation damage than adults. Highest concentrations at the present time are in the one to two-year old group. These samples are very difficult to obtain in quantity. We are, therefore, making a great effort to obtain a substantial number of stillbirths which are more readily

available. Adult samples are still useful in determining trends in time, but they are no longer analyzed individually except in places where high activity is found or expected such as Thailand and North Dakota.

Domestic and imported foods are being investigated in cooperation with the Food and Drug Administration and consist of two aspects. Through their network of local offices, representative samples of primary non-dairy foodstuffs are obtained in the key production areas for analysis. These samples give the best average dietary level. We are also seeking foods particularly high in strontium-90. The FDA does a refined gross beta count on a large number of samples which can give a very rough index of strontium-90 activity. Samples which appear "hot" on this basis are then sent to us for strontium-90 assay.

Studies of strontium-90 in milk from both United States and foreign locations are continuing. An important phase of this work which I feel should be expanded is the correlation of strontium-90 with cesium-137 concentrations. This is important since the γ-emitting cesium-137 can be assayed very quickly. Therefore if this can give even a rough index to the strontium-90 content, it would be a tremendous aid to large-scale monitoring. The findings of this study are included in a report by the Los Alamos group.

Dietary content of strontium-90 in particular foreign areas is being investigated in cooperation with the Interdepartmental Committee on Nutrition for National Defense of the National Institutes of Health. They obtain representative diets for their nutritional studies and we analyze these diets for strontium-90. Samples from Peru are currently under investigation; a study of Thailand is projected for this fall which

will be of extreme interest since as previously noted, Thailand gives the highest average strontium-90 concentration in adult human bone.

Metabolic studies on strontium and calcium are continuing in cooperation with Montefiore Hospital. The distribution of strontium-90 now naturally present in the diet is under current investigation. Balance studies on human subjects are being conducted in which all input and output of strontium-90 is measured and the effect of dietary and therapeutic variations is noted.

III Areas Which Should Be Investigated

1. The key uncertainty in predicting future levels of strontium-90 and cesium-137 from a given amount of fallout debris in the environment revolves around the question of foliar uptake versus soil uptake of these isotopes by plants. If the isotopes enter the plant primarily through the soil, they then remain available to the plant throughout their period of radioactive decay (except for soil fixation and migration, which is probably not a major factor). On the other hand, if entry is primarily through the leaf, once the isotopes reach the soil they are largely out of the plant growth cycle and thus no longer an ingestion hazard. No doubt both routes of entry exist to some degree. However, since in one case the isotopes would be available throughout their 40 year average life whereas in the other case they would be available merely through one growing season, we are dealing with the maximum uncertainty factor of 40.

2. Quick approximate methods for the estimation of strontium-90 in foodstuffs should be devised. The correlation of cesium-137 with

strontium-90 is a start in this direction. E.C. Anderson of Los Alamos has proposed such a quick gamma counter in which five energy peaks would be determined in the sample. It appears probable that the relationships between strontium-90 concentration and these gamma peaks could be determined for human foodstuffs in given areas such that strontium-90 could be estimated to perhaps 20 - 40% precision. Such a system is essential to large scale monitoring of foodstuffs and the availability of such a quick monitoring system could assume very critical proportions in the event of widespread nuclear debris.

3. Short term isotopes such as strontium-89 should be investigated, particularly in regard to their possible dominant radiation effect on the growing fetus during periods of bomb testing.

4. Plutonium-239 present in the debris should be investigated as potential long range inhalation hazard. Although it is a relatively minor hazard at present, its relative importance in time will continually increase because of its very long half and biological residence time.

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Strontium-90 in Man, II

Walter R. Eckelmann, J. Laurence Kulp, Arthur R. Schulert

The continuation of nuclear tests makes a thorough understanding of the movement and uptake of strontium-90 a necessity while the levels in man and his environment are still relatively small compared with natural background radiation. In a previous publication (*1*) the first data on a world-wide analysis of strontium-90 in human tissue were presented. The study of the geochemical distribution of strontium-90, of its transfer through the food chain, and of its variation in human populations has continued (*2*). This article (*3*) summarizes over a thousand analyses of human bone and interprets these data in terms of present concentrations of strontium-90 in the various critical phases of the geosphere and biosphere. The new data permit a closer definition of the average concentration of strontium-90 in a large part of the human race, the geographical and dietary variation, the increase in the concentration with time, and the distribution in urban populations. From the existing data, an attempt is made to predict future levels under specified conditions.

Experimental Data

The methods employed for the radiochemical separation of strontium-90 from human bone and the low-level radiometry have been described in detail elsewhere (*4*). The vegetable samples are treated in essentially the same way after dry ashing, although a strontium-85 spike is introduced in each sample for yield determination because of the variable character of the material. The bone assays were done at the Lamont Geological Observatory and at two commercial laboratories: Isotopes Incorporated, Westwood, New Jersey, and Nuclear Science and Engineering Corporation, Pittsburgh, Pennsylvania. Frequent intercalibrations assured reliability of the results from the different laboratories.

Autopsy samples of human bone are now being received from about 30 stations in a world-wide network (*5*). During this period of the investigation an attempt was made to obtain samples that were equally distributed with regard to the age of the individual at death. An attempt was also made to obtain maximum spread in geographic and dietary setting, but the restriction in the location of medical centers has limited the samples largely to urban populations.

The results reported in this article include those given in the previous report with the exception of samples which contained less than 1.0 gram of calcium. Very small samples are subject to large statistical errors and are more sensitive to slight contamination. The elimination of such samples at an arbitrary calcium level does not cause preferential selection of the data, reduces the number of samples by less than 5 percent, and does not seriously affect the final averages. Currently, only samples containing more than 1.0 gram of calcium are assayed for strontium-90. Also omitted are data reported in the previous article for the 49-year-old Vancouver (British Columbia) female (died October 1955; 8.3 micromicrocuries of strontium-90 per gram of calcium) and for the 34-year-old Japanese man (died June 1955; 4.05 micromicrocuries of strontium-90 per

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gram of calcium). Each of these two results is greater than the mean value for adults in 1955 by a factor of 50 to 100 and larger than the next nearest adult value by a factor of 5 to 10. They may be real but rare high values. On the other hand, there are cogent reasons for rejecting them as artifacts: (i) Clinical data indicate that the individuals were city dwellers. Data on urban foods make it extremely unlikely that these adults had a sustained diet of 100 times the average strontium-90 concentration. (ii) These samples now lie about 100 standard deviations from the mean (see discussion later in this article on the distribution curve). (iii) These were among the first few batches of samples analyzed commercially. (iv) When the medical history of the Vancouver patient was checked, it was found that she had carried a radium seed. Although this last finding should not affect the strontium-90 analysis if the analysis is properly done, it presents a possible explanation of the high value, particularly since the analysis was made in the early phase of the work. Even if these two anomalous samples were included, this would not affect any of the conclusions which are drawn from the regional averages.

All results are reported in micromicrocuries of strontium-90 per gram of calcium. Samples which were reported by the analysts as "equal to or less than" a certain number are assumed to have that value (that is, "≤ 0.02" is taken as 0.02) in computing the averages. Therefore, the reported averages are maximal. If all such samples are eliminated it would change the general averages by less than 10 percent but would reduce the total number of samples by about one-quarter.

These measurements have been made in an attempt to determine the average concentration of strontium-90 in the whole skeleton of a man. In the worldwide sampling, however, only a single bone is obtained, generally either vertebrae, rib, or femur shaft. Although stable-strontium/stable-calcium ratios are essentially constant among the bones of the skeleton, experiments conducted by Schulert and Laszlo (*6*), in which single intravenous doses of calcium-45 and strontium-85 were given to ten patients, followed by analyses at death (1 to 125 days after administration), show large but consistent differences in the amount of strontium-85 per gram of calcium. The stable element result reflects long-term continuous ingestion on a near-constant diet of stable-strontium/stable-calcium. The tracer experiment shows the maximum differences to be expected in the adult skeleton for a dose of radioisotope over a short time interval. Since the bones of adults are not in equilibrium with the strontium-90 in the diet and since there is a long turnover time for mature bone, the strontium-90 concentrations in the various bones of the skeleton should also be different. Therefore it is necessary to define the normalization factors that are required to convert the analytical data on single bones to average strontium-90 concentrations for the whole skeleton.

Table 1. Normalization factors from single bone to average skeleton (adults).

Sr^{90} single dose in man		Sr^{90} distribution in 10 New York cadavers* (*7*)
Previous estimate (*1*) 1 case	Present estimate (*6*) 10 cases*	
	Vertebrae	
4.2	4.8	3.4
	Rib	
2.0	2.1	1.5
	Long bone shaft	
0.7	0.6	0.8

* The standard deviation is about ± 25 percent of the value.

Table 1 gives these normalization factors for the strontium-85 single-dose experiments and for natural strontium-90 in ten cadavers from New York (*7*). The distribution of strontium-90 is more uniform than that in the extreme case of the single strontium-85 dose, but it shows

significant differences with respect to the various bones of the skeleton. The strontium-90 factors, rather than those for the strontium-85 experiments, have been used in correcting all individual measurements to the whole-skeleton value for all samples from individuals 20 years of age and older.

In individuals of from 0 to 19 years of age, the strontium-90 distribution is more nearly uniform, since most of the strontium-90 has been introduced into the skeleton by bone growth rather than by exchange and remodeling, as is the case with adults; hence, no correction has been made. Clearly, however, this is only an approximation, since children's bones are still far from being in equilibrium with their diet. Empirical evidence that this treatment of the data more nearly reflects the true situation is found in the consistency of the data, in similar latitudes, for adults and children, represented by different bones. Samples are now being obtained to verify this point directly.

For convenience, the time intervals chosen to show the progressive increase in concentration of strontium-90 begin on 1 July of each year. The impact of the major nuclear tests of the spring of 1954 did not begin to show up in human bone until after 1 July of that year. The present discussion covers the period from 1 July 1953 to 30 June 1957.

The analytical errors on the individual measurements range from 2 to 50 percent, depending on the activity, but most samples have an error of the order of 5 to 10 percent. In general, the analytical error is quite small compared with the degree of uncertainty about the relation of an individual bone to the whole skeleton. Variations in the strontium-90 concentration within a given bone and among similar bones of the same individual show a standard deviation of about ±25 percent. Since this effect should average out in a large number of samples, the uncertainty in the national or global averages is probably controlled by variation in local fallout and diet rather than by variability in sampling procedure.

Regional Averages and Age Dependence

The strontium-90 content of the average human bone for each continent, as a function of age, for 1955–56 and 1956–57 is given in Table 2. It is evident that there is no significant difference in the age groups of 20 years and older. The differences from one 10-year age group of adults to another at the same station do not exceed the mean by more than about 10 percent for those cases where a large number of samples is available. Therefore, the adult samples may be pooled for statistical purposes. The ratio of the strontium-90 level in young children (0 to 4 years) to that in adults rises from about 6 to 1 in 1955–56 to 10 to 1 in 1956–57, reflecting the more rapid approach to equilibrium of the children due to the growth of new bone. It is noted that in 1956–57 the highest values are found in North America, while values for South America, Africa, and Australia are about half of this, with those for Europe in between. For all samples, the value for the average female is about 15 percent higher than that for the average male. Since the data include a thousand individuals nearly equally divided with respect to sex, this result is considered significant.

The "world averages" were computed by weighting the average for each age group by the fraction of the world population in that age group. The Asian samples come only from Japan, Taiwan, and India. But if Taiwan can be presumed to represent Southeast China, a large fraction of the people in Asia are included. The levels in Russia are very probably higher, due to the proximity to test sites. Since the differences in the average bone level from one station to another seldom exceed a factor of two, the calculated "world average" is relatively insensitive to the distribution of

Table 2. Average strontium-90 content in man, 1 July 1955 to 30 June 1956 and 1 July 1956 to 30 June 1957. All values are given in micromicrocuries of strontium per gram of calcium, normalized to the whole skeleton. The figures in parentheses give the number of samples in the category.

Location	Age at death (yr)								
	0–4	5–9	10–19	20–29	30–39	40–49	50–59	60–80	20–80 (average)
					1955–56				
North America	0.56(10)	0.44(4)	0.20(13)	0.09(34)	0.07(38)	0.06(14)	0.04(17)	0.07(34)	0.070(137)
South America	0.33(12)	0.19(16)	0.14(26)	0.07(48)	0.06(58)	0.08(45)	0.07(18)	0.11(15)	0.073(184)
Europe	0.42(30)	0.27(19)	0.23(46)	0.06(70)	0.10(59)	0.06(8)	0.16(1)	0.09(3)	0.078(140)
Asia		0.10(2)	0.20(9)	0.06(35)	0.07(32)	0.11(11)	0.13(13)	0.20(4)	0.085(95)
Entire world	0.43(52)	0.25(41)	0.20(94)	0.068(187)	0.076(187)	0.079(78)	0.077(49)	0.091(56)	0.076(556)
	World average 1955–56 = 0.15								
					1956–57				
North America	0.67(30)	0.69(17)	0.38(15)	0.07(14)	0.06(9)	0.08(16)	0.05(5)	0.07(18)	0.070(62)
South America	0.16(3)	0.20(1)	0.19(5)	0.03(5)	0.02(2)	0.03(2)	0.06(3)	0.01(1)	0.034(13)
Europe	0.65(2)	0.34(4)	0.34(9)	0.06(20)	0.07(4)	0.04(6)	0.06(1)	0.08(2)	0.059(33)
Africa			0.06(2)	0.03(2)	0.03(3)	0.04(4)			0.035(9)
Asia	0.93(1)	0.12(2)	0.32(2)	0.06(8)	0.04(6)	0.12(8)	0.06(5)	0.05(5)	0.070(32)
Australia	0.75(3)	0.60(2)			0.03(3)	0.03(4)	0.03(3)		0.030(10)
Entire world	0.64(39)	0.57(26)	0.30(33)	0.059(49)	0.047(27)	0.070(40)	0.052(17)	0.065(26)	0.060(159)
	World average 1956–57 = 0.20								

the sample location. Finally, the stations in the United States, Europe, Japan, China, and India may represent 70 to 80 percent of the world's population.

Table 3 shows that the strontium-90 level in human beings is more uniform than the total fallout. One reason for this, as suggested to us by Lyle Alexander, of the U.S. Department of Agriculture, is the widespread distribution of milk products from the United States and northern European countries.

The world-wide average for the urban world population in 1955–56 is seen to be about 0.15 micromicrocuries of strontium-90 per gram of calcium, which represents a level of about 0.076 in adults and 0.43 in young children. In 1956–57 these figures are 0.20, 0.060 and 0.64, respectively, showing an increase in the young children of 50 percent and in the world average, of about 30 percent.

Individual Stations and Time Effect

Table 4 lists the average values for children aged 0 to 4 and 5 to 9 and for adults, as a function of time, in the stations from which analyses have been completed. Significant comparison of certain localities can be made only if the samples are numerous, because of the individual biological variation. Values for Houston, Texas, appear to be significantly higher than those for other North American cities for 1954–56. Values for adults in Chile, South Africa, and Australia are lower than those for adults in Northern Hemisphere stations by a factor of two. It is noted that the adults show a small apparent decrease in strontium-90 content during the past year. This is primarily due to the variation in stations represented in the two years (for example, the bones of individuals in Houston, which had higher-than-average values in 1954–56, are not represented in 1956–57). The results do show that the rise in children is much greater than that in adults. This is a striking reflection of the fact that, whereas adults are merely exchanging a small fraction of their skeleton each year, the children are building new bone in equilibrium with their diet, in addition to exchange.

Table 3. Comparison of variations, with latitude, in strontium-90 content of human bone and in total fallout.

Latitude	Human bone (Av. for 1955–57) ($\mu\mu$c Sr^{90}/g Ca)		Soil, (June 1956)* (mc/mi^2)
	Age at death		
	0–4	Adults	
60–40°N	0.55	0.07	~8
40–20°N	0.68	0.085	9.4
20–0°N	0.32	0.075	3.9
0–20°S		0.05	2.0
20–40°S	0.46	0.06	2.6

* From remarks prepared by Merril Eisenbud, manager of the New York Operations Office, U.S. Atomic Energy Commission, for hearings of the Joint Committee on Atomic Energy, 27 May 1957.

Comparative strontium-90 data for England have been obtained by Bryant *et al.* (*8*) from a dozen localities. For 1955–56 their data show an average of 0.77 micromicrocuries of strontium-90 per gram of calcium in 18 samples, 0.22 micromicrocuries of strontium-90 per gram of calcium in 3 samples, and 0.08 micromicrocuries of strontium-90 per gram of calcium in 19 samples for the 0 to 4, 5 to 9, and adult age groups, respectively. The averages agree with those reported in Table 4, within the standard deviation of each, and they are as high as those of any comparable area in North America. The higher average rainfall in the British Isles apparently compensates for the greater distance from the Nevada test site. In addition, westward-moving Russian tropospheric debris would presumably contribute more fallout to Great Britain than to the United States.

Table 5 compares the change in levels of strontium-90, with time, for cumulative New York rainout, New York and Wisconsin milk, bones of young children (age 0 to 4) and of adults in northeast-

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Table 4. Average strontium-90 concentration in man for three periods (1 July 1954 to 30 June 1955; 1 July 1955 to 30 June 1956; 1 July 1956 to 30 June 1957) for each station. All values are given in micromicrocuries of strontium-90 per gram of calcium, normalized to the whole skeleton. "Adult" represents samples of individuals who died at 20 years or older. The figures in parentheses indicate the number of samples in the category.

Location	Bone	1954–55			1955–56			1956–57		
		Age at death (yr)			Age at death (yr)			Age at death (yr)		
		0–4	5–9	Adult	0–4	5–9	Adult	0–4	5–9	Adult
Boston	Vertebrae	0.78(2)			0.61(3)	0.22(1)		0.69(25)	0.71(15)	0.06(19)*
New York	Mixed	0.17(1)		0.05(44)			0.05(38)			
Denver	Vertebrae	0.29(3)			0.48(4)	0.1(1)	0.08(44)	0.67(3)	0.59(1)	0.06(6)
Houston	Mixed	0.32(2)		0.15(23)	0.82(3)		0.14(11)			
Vancouver	Tibia	0.32(2)	0.07(2)	0.08(16)			0.05(29)	0.42(2)	0.72(2)	0.08(21)
Puerto Rico	Vertebrae			0.08(15)	0.27(2)		0.07(16)		0.48(1)	
Venezuela	Rib		< 0.17(1)		0.33(12)	0.34(8)	0.09(51)			
Colombia	Rib → Vertebrae			0.09(2)			0.04(7)			0.02(2)
Argentina	Vertebrae									0.05(2)
Chile	Rib		0.19(3)	0.10(13)		0.04(8)	0.07(86)	0.16(3)	0.20(1)	0.03(9)
Brazil	Rib		0.07(1)	0.10(4)			0.05(41)			
Australia	Rib							0.75(3)	0.60(2)	0.03(10)
Taiwan	Mixed	0.2(1)		0.03(2)		0.14(1)	0.08(18)	0.93(1)	0.09(1)	0.04(4)
India	Rib		0.32(2)	0.10(6)		0.06(1)	0.07(58)			0.08(11)
Japan	Rib						0.14(19)		0.14(1)	0.08(15)
Iran	Femur									0.03(2)
Denmark	Vertebrae	0.18(1)	0.20(1)	0.03(2)		0.22(1)	0.05(12)		0.36(1)	0.04(7)
England	Vertebrae				0.74(5)	0.18(6)	0.04(14)	0.31(1)		0.06(11)
Germany	Femur	0.31(3)	0.10(2)	0.11(12)	0.30(20)	0.18(6)	0.08(79)			0.05(11)
Italy	Rib			0.09(4)	0.37(1)		0.13(14)			
France	Rib						0.05(5)			
Switzerland	Vertebrae	0.53(3)		0.09(5)	0.60(4)	0.47(6)	0.09(17)	0.98(1)	0.44(2)	0.09(10)
Union of So. Africa	Rib									0.04(11)

* Femur

ern United States, and bones of young children from North American stations. These sets of bones were taken because they represent the largest number from one area or continent over the period of interest. The values for milk and for the cumulative fallout go up proportionally, whereas those for bones lag behind, due to the delay in reaching equilibrium with the diet. Thus, although the fallout and milk samples increased three-and-one-half- to sixfold from the end of 1953 to the end of 1956, the bone levels increased two- to threefold.

Distribution in Single Population Group

In order to predict the fraction of the human population that will have any given concentration of strontium-90, it is necessary to examine the statistical distribution of strontium-90 in certain well-defined groups. Consider first only people living in cities or those who obtain their food from central distribution centers. The greatest spread will be obtained by grouping together adults of all ages from all localities. Figure 1 is a histogram showing the distribution of all adult samples in 1955–56. There are a number of factors which tend to make the distribution for adult groups, even at one site, much larger than will actually be the case at equilibrium. These include biological variation from one bone to another in a single individual and the different rates at which individuals equilibrate their bones with their diet. Thus, a histogram of adult samples would be considerably skewed to the right. Such a distribution will probably become narrower as equilibration proceeds. The group in which the equilibrium situation is likely to be most closely approximated is that of young children (Fig. 1), who have strontium-90 more uniformly distributed in their bones. Thus, although the distribution curve for all adults in 1955–56 shows a log normal form with a standard deviation of about 100 percent and with one case out of 557 having a value seven times the mean, the young children from Boston and Switzerland more closely approximate a Gaussian-type distribution, with a standard deviation of about 40 percent. Since even these children are not at equilibrium, it is expected that the distribution will become still narrower. For urban populations the distribution at equilibrium may approach a normal Gaussian distribution, but it is expected that the world-wide distribution will always be skewed somewhat to-

Table 5. Increase in strontium-90 levels in various materials. The figures in parentheses represent the number of samples in the category.

Sample	Units	1953–54	1954–55	1955–56	1956–57
Cumulative rainout (New York)*	mc/mi²	5	9	18	28
Milk (New York)†	μμc Sr^{90}/g Ca	0.8	1.25	2.4	3.5
Milk (Wisconsin)†	μμc Sr^{90}/g Ca	1.2	2.0	3.0	4.3
Bones of children (0 to 4 yr) (New York and Boston)	μμc Sr^{90}/g Ca	0.28(5)‡	0.57(3)	0.61(3)	0.69(25)
Bones of children (0 to 4 yr) (North America)	μμc Sr^{90}/g Ca	0.28(5)‡	0.38(10)	0.56(10)	0.67(30)
Bones of adults (New York and Boston)	μμc Sr^{90}/g Ca	0.02(3)‡	0.05(44)	0.05(38)	0.06(19)

* Data from New York Operations Office of U.S. Atomic Energy Commission and Lamont Geological Observatory.
† Data from New York Operations Office interpolated from reports (*10*).
‡ Data of Libby (*12*).

ward higher values since, locally, it is possible to increase the amount of strontium-90 per gram of calcium in the soil, and therefore in foods, by factors of 100 to 1000 times the urban mean but to decrease it by factors of only 2 to 10. Present experience in attempting to locate such high values suggests that they will represent a very small fraction of the world population.

From the limited number of samples near equilibrium at any given location it is difficult to estimate the ultimate situation with precision. Nevertheless, it does appear that the bulk of the world's population can be described roughly by a Gaussian distribution with a standard deviation of about 40 percent. Thus, at equilibrium, 99 percent of the people will probably be within about twice, or half of, the mean. Data are simply not available to estimate the distribution in nonurban segments of the population, but it is probably less uniform.

Bone Level and Diet

If the amount of strontium-90 per gram of calcium can be defined in the diet of a population group, and if the discrimination factors between diet and

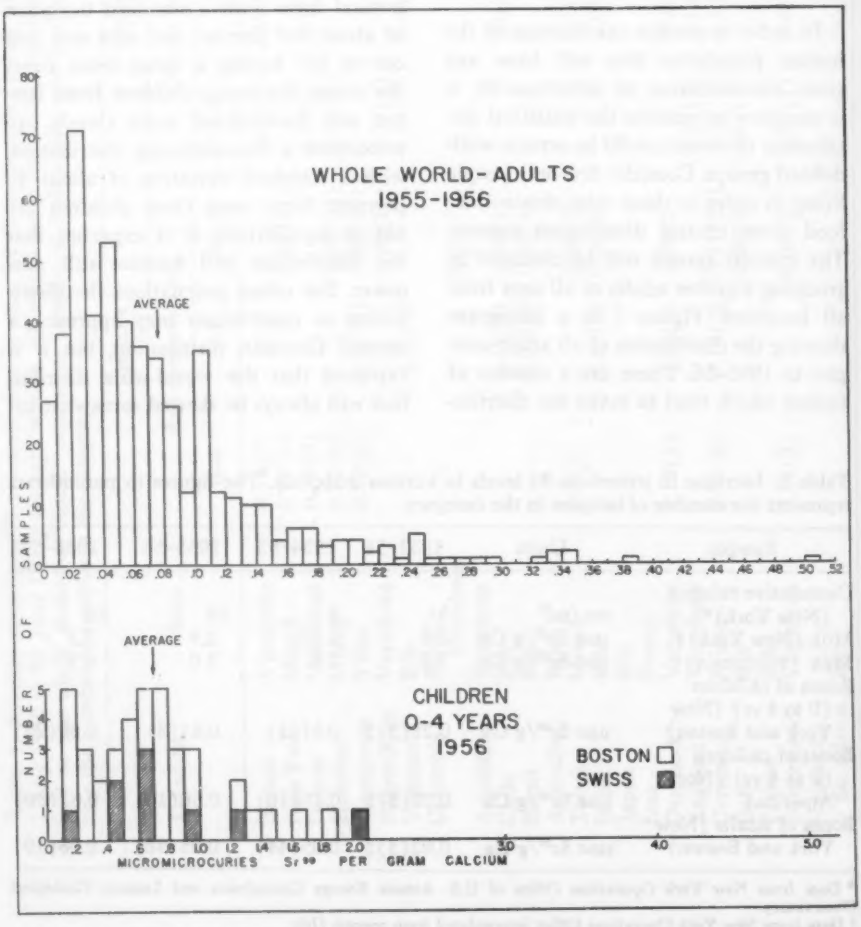
WHOLE WORLD - ADULTS
1955-1956
AVERAGE
SAMPLES
0 .02 .04 .06 .08 .10 .12 .14 .16 .18 .20 .22 .24 .26 .28 .30 .32 .34 .36 .38 .40 .42 .44 .46 .48 .50 .52
AVERAGE
NUMBER OF
CHILDREN
0-4 YEARS
1956
BOSTON
SWISS
0 .2 .4 .6 .8 1.0 1.2 1.4 1.6 1.8 2.0 3.0 4.0 5.0
MICROMICROCURIES Sr90 PER GRAM CALCIUM

Fig. 1. Histogram of strontium-90 concentration in the bones of adults and children.

bone are known for strontium-to-calcium, the equilibrium level of strontium-90 in the whole skeleton can be predicted. In determining the contribution of strontium-90 to the bone from the diet of any individual it is necessary to know the calcium content, the strontium-90-per-gram-of-calcium ratio for each ingredient, and the strontium-calcium fractionation factors along the food chain. Table 6 gives the relative contribution of calcium by various foods in most national diets (*9*). It is evident that, for a large portion of the world, milk products are the dominant source of calcium. Fish carry very low specific activity of strontum-90, due to the dilution in the large reservoir of ocean calcium. Meat and eggs contribute a negligible amount. Table 7 gives data on the concentration of strontium-90 in milk from various parts of the world, obtained by the New York Operations Office of the U.S. Atomic Energy Commission (*10*) and by the British workers (*11*), for 1956. Table 8 gives new experimental measurements on the concentration of strontium-90 in frozen foods in the United States. Presumably, canned vegetables will show a similar average, but the making of measurements on this important food source is only beginning. The data on cereals are also meager as yet, but the indication is that the figures are higher than those for milk of the same region by a factor of about 6 (*12, 13*). The extent of calcium-strontium discrimination in the transfer from diet to human bone is still debated, although the divergence of opinion has decreased. The previously suggested value of eight (*1*) now appears high. Comar (*14*) has estimated that the factor is about two. However, the work of Spencer, Laszlo, and Brothers (*15*) and of Schulert *et al.* (*6*) suggests a value of about four, and this value has been used in the subsequent calculations.

When one knows the average diet and the strontium-90 content of foods, the relative importance of the various components of the diet can be estimated. In 1956–57 in the northeastern United States, for example, the level in milk was about 3.5 micromicrocuries of strontium-90 per gram of calcium, the vegetable intake was about 7.3 (Table 8), and the level for cereals was estimated at about 20. Therefore, the equilibrium concentration of strontium-90 in the bone for this dietary composition, when the discrimination factor of four and the data of Table 6 are used, is computed to be 1.1 micromicrocurie of strontium-90 per gram of calcium which is dominated by the milk level.

From the diet data and the discrimination factor, the strontium-90/calcium ratio which will occur in the newly deposited bone of children can be calculated. Langham and Anderson (*16*) first suggested this approach and used it to compute the fraction of equilibrium for skeletons of any given age group. The data for bone indicate that for adults the total integrated exchange and remodeling amount to about 7 percent of the equilibrium value if the discrimination factor of four is assumed. In predicting the strontium-90 level in children it is assumed that, in addition to growth, they have the same percentage of exchange and remodeling as adults. Other assumptions and data on which the calculation is based are as follows: (i) The New York milk levels are taken from Table 5 and are assumed to be representative of the northeastern United States. (ii) The relative contribution to the calcium content of the diet from milk and vegetation for this period is given in Table 6. (iii) The yearly accretion of skeletal calcium in males is given by Mitchell *et al.* (*17*). (iv) The discrimination between strontium and calcium in transfer from diet to bone is four. (v) The fetal protective factor is two (*18*).

The calculation yields the solid curve shown in Fig. 2. The dotted curve is computed in an identical manner, but a discrimination factor from food to bone of two instead of four is assumed. Also plotted are the experimental data from the United States and Europe,

which closely approximate the theoretical curve for a discrimination of four.

The concentration of strontium-90 in adults suggests that only 1 to 2 percent of bone is exchanged or remodeled per year.

On the basis of these relationships, the equilibrium levels in human bones are computed for the end of 1956 for all continents. The results are given in Table 9. It is clear that the equilibrium level for the Southern Hemisphere will be about one-half that for the Northern Hemisphere.

Future Levels

The concentration of strontium-90 in human bone at any future time for any specified kind and amount of testing can now be predicted within certain limits. The greatest area of uncertainty is that which pertains to the stratospheric reservoir. Although there are a number of ways of attacking this problem, we will start with the actual levels in bone. The results are summarized in Table 10, along with other comparative data.

Table 6. Sources of calcium in national diets (in percentage of contribution to total calcium in the diet).

Country	Cereals	Potatoes and starchy roots	Pulses and nuts	Meats	Eggs	Fish	Milk and dairy products
Austria	6	6	1	1	1		85
Belgium–Luxembourg	8	11	1.5	1.5	2	1	75
Denmark	6	9	2	2	1	1	79
Finland	6	7		1	1	1	84
France	8	10	2	2	2	1	75
Germany (West)	7	14	1	1	2	1	74
Greece	17	5	12	1	1	1	63
Ireland	8	12	1	2	2		75
Italy	19	6	8	1	3	1	62
Netherlands	6	7	1	1	1	1	83
Norway	5	5	1	1	1	1	86
Portugal	26	26	9	1	2	6	30
Sweden	4	5	1	1	1	2	87
Switzerland	5	4	2	1	1		87
United Kingdom	6	7	2	2	2	1	81
Yugoslavia	20	6	5	1	1		67
Canada	4	4	2	2	3		85
United States	4	3	2	2	4	1	85
Argentina	9	5	1	3	2		79
Chile	15	7	5	2	2	2	67
Cuba	12	11	10	2	1	1	64
Peru	17	29	8	2	2	1	41
Uruguay	8	5	1	3	2		82
Venezuela	9	5	8	1	1	1	75
India	24	2	2				51
Pakistan	22	1	1				72
Israel	15	3	4	1	3	2	73
Japan	46	17	8		3	8	18
Philippines	39	15	18	2	3	5	18
Egypt	30	2	9	1	1	1	57
Rhodesia	36	2	14	5	1	1	41
Turkey	38	7	14	2	1	1	37
Union So. Africa	21	3	2	2	1	1	71
Australia	7	4	2	3	2		82
New Zealand	5	2	1	2	2	1	88

There may be as much as a half-year lag between fallout and its effect on the strontium-90 level in vegetation, but this interval is short when compared with the other factors involved; so it will be assumed that diet level and integrated total fallout level are essentially in equilibrium. The predicted equilibrium value for the average population (1.1 micromicrocuries of strontium-90 per gram of calcium in North America), calculated on the basis of fallout through the end of 1956 and on the correlation of the actual bone data with the predicted population levels as a function of age shown in Fig. 2, appears to be fairly well defined. (Actually, if no more strontium-90 were deposited on the ground after 1956, this hypothetical equilibrium level would not be attained, since the concentration of strontium-90 in the reservoir would be reduced continuously by radioactive decay.)

Prediction of future levels involves knowledge of the stratospheric reservoir. Libby (*19*) has estimated the mean residence time of the stratospheric debris to be about ten years, with an uncertainty of about 25 percent. A maximum value for the stratospheric debris which will fall out with no further testing could be obtained by multiplying the 1956–57 annual fallout by ten (that is, about 110 millicuries per square mile in northeastern United States). This, however, requires the assumption that all 1956–57 fallout was stratospheric. The limited data on the ratio of strontium-89 to strontium-90 (*20*) suggest that the 1956–57 fallout was largely stratospheric in origin. A minimum estimate can be made by accepting Libby's (*19*) estimate of a current stratospheric burden of a fission equivalent of 24 megatons of TNT and by assuming that this will be deposited uniformly over the entire world. This would add 12 millicuries of stratospheric debris per square mile to the surface of the earth. Machta (*21*) has given valid reasons to support the theory of nonuniform deposition of stratospheric debris. His estimates, made on this basis, would lead to a prediction of 60 to 70 millicuries of stratospheric fallout per square mile in the northeastern United States if there should be no further testing.

It is now possible to predict the level of strontium-90 in children's bones in the northeastern United States for 1977 (when most of the stratospheric debris will have fallen out) if it is assumed (i) that no additional debris is introduced into the stratosphere after mid-1957; (ii) that the fallout to mid-1957 is 26 millicuries per square mile, calculated on the basis of Lamont analyses of 18 soil samples taken in the New York City area; and (iii) that the minimum, probable, and maximum estimates for the stratospheric fallout are 12, 65, and 110 millicuries per square mile, respectively. The equilibrium levels of strontium-90 per gram of calcium will be 1.2, 2.9, and 4.3 micromicrocuries of strontium-90 per gram of calcium, respectively.

These data can be calculated for any other geographical location if the level in the soil at mid-1957 is known. At all times the average for North America would be the highest for any land mass, with the possible exception of the U.S.S.R., and it would in general be higher by a factor of two than that for countries in the Southern Hemisphere (Table 9).

Table 7. Strontium-90 in milk at various locations, 1956. [Data from New York Operations Office and British workers (*10, 11*).]

Location	Amount ($\mu\mu$c Sr^{90}/g Ca)
Perry, N.Y.	3.3
State College, Miss.	5.3
Columbus, Wis.	3.4
Mandan, N.D.	8.8
Portland, Ore.	5.5
Japan	2.8
United Kingdom	4.5
Somerset, England	4.2
Other areas in England	6.9

If testing continues at such a rate that the annual deposit of strontium-90 is 10 millicuries per square mile (as it was for the northeastern United States in 1956–57), the equilibrium level on the ground would be 400 millicuries per square mile, since the mean life of strontium-90 is 40 years. The corresponding equilibrium bone level of the population would reach 21 micromicrocuries of strontium-90 per gram of calcium by the year A.D. 2100.

Table 9. Equilibrium strontium-90 concentration for end-of-1956 diet (discrimination factor 4).

Continent	Concentration ($\mu\mu$c Sr^{90}/g Ca)
North America	1.1
Europe	0.9
Asia	1.1
South America	0.5
Africa	0.5
Australia	0.5

Conclusions

1) For any given station, the strontium-90 content of adult bone is independent of age.

2) The regional differences in the strontium-90 levels in human bone are much smaller than the differences in total fallout. The maximum difference appears to be a factor of two between United States–Europe and the Southern Hemisphere.

3) The average concentration of strontium-90 in the skeleton for most of the world population at the end of 1956 was about 0.20 micromicrocuries of strontium-90 per gram of calcium. The average for North American or European children was about 0.7 micromicrocuries of strontium-90 per gram of calcium, whereas that for adults was lower by a factor of approximately ten.

4) Single bones can be used for estimating the total skeleton concentration of strontium-90 with a precision of about ± 25 percent. The strontium-90 is reproducibly but inhomogeneously distributed in the adult skeleton, but it appears to be more nearly uniform up to the age of about 19.

5) Increase in the strontium-90 content of the diet follows that of the total

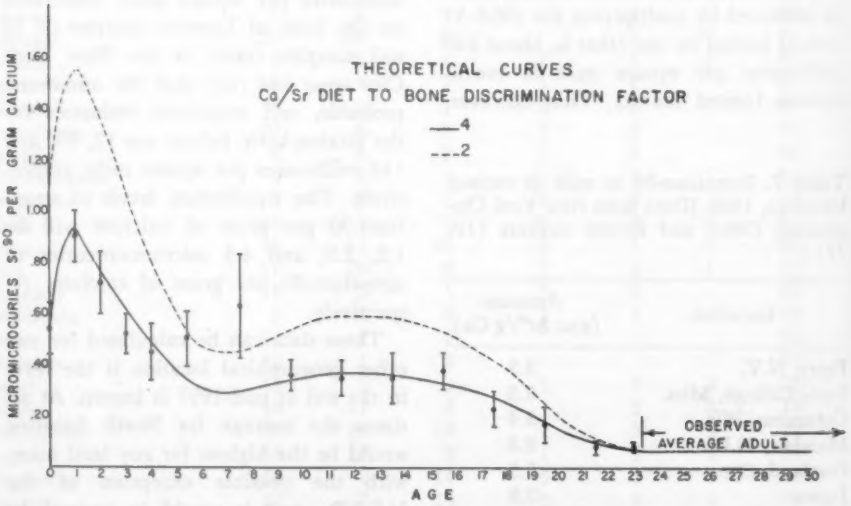

Fig. 2. Comparison of the experimental data on the concentration of strontium-90 in man as a function of age with the theoretical curves obtained from the dietary level of the northeastern United States from 1952 to 1957 and discrimination factors between diet and skeleton of four and two.

integrated fallout, but the strontium-90 level in bones lags behind, due to delay in reaching equilibrium.

6) The present distribution of strontium-90 in adult populations is considerably wider than will be the case at equilibrium. It is estimated that, at equilibrium, the urban populations of the world, including those dependent on central food distribution systems (this will represent 80 percent of the people) will be described roughly by a Gaussian distribution with a standard deviation of about ±40 percent of the mean.

7) The way in which the high values will be distributed is not yet known, but maximum values are expected in areas where the soil has a low calcium content and where local consumption of milk or vegetable foods is low.

8) The predicted levels of strontium-90 concentration for various age groups in present populations, based on known present diet levels and bone growth, agree well with observed values if a discrimination factor between diet and bone of about four is assumed.

9) If there were no further atomic tests after mid-1957, children in the northeastern United States would reach a peak level of 2.9 micromicrocuries of strontium-90 per gram of calcium. The uncertainty in this prediction probably does not exceed a factor of two.

10) If testing continues at a rate such that a certain region receives ten millicuries per square mile per year, (as the northeastern United States did in 1956–57), the equilibrium bone level for the population will be 21 micromicrocuries of strontium-90 per gram of calcium.

Table 8. Concentration of strontium-90 per gram of calcium in common frozen vegetables (1956) (Lamont Geological Observatory data).

Date of harvest	Vegetable	State	Concentration ($\mu\mu$c Sr^{90}/g Ca)
Sept. 1955	Lima beans	Idaho	6.3 ± 0.3
Sept. 1955	Lima beans	California	10.0 ± 0.4
Early 1956	Lima beans	Delaware	2.9 ± 0.2
Early 1956	Cauliflower	?	2.7 ± 0.3
1955–56	Corn	?	3.6 ± 0.2
1955–56	Peas	?	1.3 ± 0.3
Early 1956	Spinach	Tennessee	6.1 ± 0.1
Feb. 1956	Turnip greens	Tennessee	7.8 ± 0.1
June 1956	Asparagus	Maryland	1.7 ± 0.2
June 1956	Peas	Minnesota	5.8 ± 0.2
July 1956	Peas	Washington	7.8 ± 0.3
Aug. 1956	Peas	Maine	21.3 ± 0.6
Aug. 1956	Cut green beans	New York	20.2 ± 0.5
Sept. 1956	Corn	Minnesota	1.6 ± 0.3
Sept. 1956	Squash	Oregon	3.1 ± 0.2
Sept. 1956	Brussels sprouts	California	4.3 ± 0.2
Sept. 1956	Cut green beans	Pennsylvania	4.6 ± 0.3
Sept. 1956	Cut green beans	Pennsylvania	8.0 ± 0.4
Sept. 1956	Lima beans	Delaware	8.4 ± 0.2
Fall 1956	Cauliflower	New York	8.1 ± 0.3
Fall 1956	Spinach	California	11.5 ± 0.3
Oct. 1956	Asparagus	Maryland	1.7 ± 0.2
Oct. 1956	Broccoli	Maryland	4.7 ± 0.1
Oct. 1956	Broccoli	Maryland	6.7 ± 0.3
Oct. 1956	Broccoli	Maryland	8.5 ± 0.3
Oct. 1956	Brussels sprouts	California	12.0 ± 0.3
Oct. 1956	Cauliflower	New York	9.1 ± 1.3
Mar. 1957	Spinach	California	13.9 ± 0.2
		Average	7.3

Table 10. Present and predicted future levels of strontium-90.

Situation or condition	Level ($\mu\mu c$ Sr^{90}/g Ca)
Industrial MPC.*	1000
Large-population MPC.	100
Average background radiation equivalent [Libby (*22*)]	50
End of 1956	
World average	0.20
North American children (0 to 4 yr), average	0.67
North American adults, average	0.07
Predicted equilibrium value, North America	1.1
Predicted equilibrium value, Southern Hemisphere	0.5
Future	
Predicted average for children in northeastern United States, 1977 (no further testing—essentially at equilibrium)	
Minimum	1.2
Probable	2.9
Maximum	4.3
Predicted average for population at equilibrium for an area of continuous fallout of 10 mc/mi^2 yr (A.D. 2100)	21

* Maximum permissible concentration recommended by the International Committee on Radiation Protection.

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2. This research is being supported by the Division of Biology and Medicine of the U.S. Atomic Energy Commission. The encouragement, support, and criticism of W. F. Libby, of C. L. Dunham, Forrest Western, H. D. Bruner, and W. D. Claus of the Division of Biology and Medicine, and of M. Eisenbud, J. H. Harley, and I. Whitney of the New York Operations Office, are greatly appreciated. Daniel Laszlo, chief, Division of Neoplastic Diseases, Montefiore Hospital, has provided valuable counsel and assistance in biomedical aspects of the program. Other scientific contributions to the planning and interpretation of this work have been made by H. L. Volchok, of Isotopes, Incorporated; C. L. Comar, of Oak Ridge National Laboratory; Wright Langham, of Los Alamos Scientific Laboratory; and K. K. Turekian, of Yale University. Elizabeth Hodges, Edwin Peets, and Rieta Slakter were key personnel in the carrying out of the technical phases of the investigation. They were assisted by D. Walton, D. Tuck, J. Rippey, G. Markle, R. Alley, and W. Blake. The manuscript was read critically by F. Bronner, H. D. Bruner, C. L. Dunham, W. Kisieleski, M. Eisenbud, P. K. Kuroda, W. H. Langham, D. Laszlo, W. F. Libby, L. W. Machta, L. D. Marinelli, W. F. Neuman, W. P. Norris, F. Western, and H. Q. Woodard.
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SCIENCE

Strontium-90 in Man III

The annual increase of this isotope and its pattern of world-wide distribution in man are defined.

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The investigation of the mechanism of the distribution of strontium-90 from nuclear detonations and its uptake in man has continued. This article (*1*) presents new data on strontium-90 concentration in human bone, largely from samples obtained in 1957–58. The measurements extend the earlier work (*2*, *3*) in time and location and add details which give greater understanding of the principles of the movement of strontium-90 in the biosphere. The results of the past year also clearly define the annual increment in strontium-90 in the skeleton for the years 1953 to 1957 in a single location (New York City).

The ultimate goal of this study is to predict the future distribution of strontium-90 for the entire world population. Enough data have been obtained on the strontium-90 levels in human bone, in food, in soil, and in rainfall as a function of geographical location so that a first approximation to the world distribution curve can be made. The curve for the present distribution of strontium-90 in adults has been calculated and this is used to predict the distribution for young children in 1966, when the maximum level of strontium-90 in the diet from tests made to date will have been reached.

Experimental Procedures

Samples of human bone are received at regular intervals from about 35 stations in a world-wide network. During 1957–58 an attempt was made to divide the samples somewhat equally according to whether the individuals had been older or younger than 20 years at death. Since it appeared from earlier work (*3*) and from data given in Table 1 that age is not an important determinant of the amount of strontium-90 deposited in adults, equal portions of adult bone from a given locality were integrated to reduce the number of analyses required. Exceptions to this practice were made where new stations were involved, in order that data on local variation might be obtained. Individual samples containing less than 3.0 grams of calcium in the case of an adult or 1.0 gram of calcium in the case of a child were not analyzed during 1957–58, since larger statistical counting errors result from the inclusion of such samples, as a result of the small quantity of strontium-90 that is present.

Most of the bone analyses were made by two commercial laboratories (*4*). Some analyses and intercalibration tests were performed at this laboratory, by methods described elsewhere (*5*).

The samples were all single bones (some were integrated samples) with the exception of 160 whole skeletons obtained from cadavers in New York City.

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Measurements have shown that the distribution of natural strontium is uniform in the adult skeleton to within at least 10 percent (*6*). Detailed studies in adults in which strontium-85 and calcium-45 tracers were used (*7*), and studies of fallout strontium-90 in both children and adults (*8*) show that the strontium-90 from the diet is nearly uniform in the bones of the skeletons of young children, while in the bones of adults it occurs in different concentrations that are systematic and reproducible. Schulert *et al.* (*8*) have determined these relations and have shown that the average concentration of strontium-90 in a particular adult skeleton can be estimated from the analysis of a single bone of that skeleton. The results reported below are all given in micromicrocuries of strontium-90 per gram of calcium. In the case of children, the level of strontium-90 in a single bone is taken as the skeletal average. The concentration of strontium-90 in the whole skeleton of an adult (of age 20 or more) is computed, by Schulert *et al.* (*8*), from the analysis of a single bone by using the actual strontium-90 distribution found in skeletons of people who died in 1957:

Vertebrae/whole skeleton = 1.8
Rib/whole skeleton = 1.0
Femur/whole skeleton = 0.5

The standard errors for these ratios are 12, 14, and 25 percent, respectively.

The uncertainty that results from computing the concentration of strontium-90 in the whole skeleton from the analysis of a single bone is generally less than the counting error attributable to the low activity levels of the single-bone samples. This is particularly true for the femur and for small samples of any adult bone (*8*).

Regional Averages and Time Effect

The concentrations of strontium-90 that were found in human bone from various localities in 1957–58 are compared in Table 2. All adult samples were combined for this purpose, since, as was noted above, no effect attributable to the age of the individual has been observed. Note that the samples for the United States have about the same concentration as the average for other parts of the world in the same latitude band. Note also that rice-diet areas (for example, Thailand, Japan, and the Philippines) have a concentration somewhat higher than the average. A world average of 0.19 μμc of strontium-90 per gram of calcium is obtained by weighting each latitude band according to its adult population but treating the Western and rice-diet areas separately. The standard deviation is given for localities contributing more than 20 samples, to give some idea of the spread encountered in samples from a given station. Larger variations generally indicate that the number of samples was small, and the mean values therefore fluctuate considerably from year to year for a given station (*3*). Also included in Table 2 is the latest estimate of the cumulative fallout per unit of rainfall for each lati-

Table 1. Levels of strontium-90 in adult skeletons in relation to age. Samples were from the area of Western culture, 20° to 60° north latitude. Number of samples is given in parentheses.

Period	Level of Sr^{90} (μμc/g of Ca)				
	Age (yr)				
	20–29	30–39	40–49	50–59	Over 60
1955–1956	0.12 (93)	0.13 (80)	0.09 (22)	0.16 (4)	0.12 (9)
1956–1957	0.13 (56)	0.15 (36)	0.12 (42)	0.15 (48)	0.12 (62)
1957–1958	0.15 (3)	0.36 (12)	0.22 (14)	0.17 (9)	0.17 (77)

tude band to January 1958, based on all available determinations of strontium-90

Table 2. Regional averages for strontium-90 in adult skeletons in 1957–1958.

No. of samples	Location	Sr^{90} μμc/g of Ca)	Standard deviation	Cumulative fallout [mc of Sr^{90}/mi^2/unit of rainfall (Jan. 1958)]
	40° to 60°N			
13	Vancouver	0.30		
73	Boston	0.18	0.10	20
1	Denmark	0.17		
6	England	0.25		
18	Germany	0.14		
	20° to 40°N			
12	Houston	0.18		17
2	Taiwan	0.18		
22	Israel	0.18	0.13	
4	Japan	0.33		
4	Hawaii	0.15		
	0° to 20°N			
25	Puerto Rico	0.12	0.08	3
4	Colombia	0.07		
46	Guatemala	0.10	0.07	
4	Philippines	0.27		
21	Thailand	0.36	0.29	
	0° to 20°S			
13	Belgian Congo	0.21		4
	Ecuador	0.13		
	20° to 40°S			
16	Argentina	0.14		6
29	Chile	0.07	0.05	
4	Union of South Africa	0.18		
5	Cape Town	0.07		
36	Australia	0.20	0.11	

in soil and rainfall. There is less apparent difference in the levels in bone than in the recorded fallout, between the Northern and Southern hemispheres (Tables 2 and 3). This fact may be due in part to the widespread flow of wheat and powdered milk from the Northern to the Southern Hemisphere. Other equalizing factors may be the higher rainfall of the local food-producing areas of the tropical regions and the higher ratio of vegetable calcium to milk calcium in the diet in the Southern Hemisphere.

The gradual build-up of strontium-90 in human bone that has occurred over the past four years is shown in a general way by the regional averages (Table 4). The anomalies reflect changes in the number and location of sampling stations during the three-year period. The best indication of the time effect is given by the whole-body samples from New York (Fig. 1); a large number of samples were available from this station, and the size of the samples made it possible to make more accurate analyses than could be made with smaller samples. It may be noted that the concentrations of strontium-90 in the skeletons of these cadavers was about 30 percent lower, on the average, than concentrations in other skeletons from the same area; this probably reflects the poorer nutritional state of these persons.

Strontium-90 in Children and Variation with Age

A general summary of the 1957–58 findings for samples of human bone of individuals who died at less than 20 years of age is given in Table 3. This is the largest number of samples for a single year yet obtained, and most of them were of sufficient size to give reasonably accurate analyses. Note the near-equality of the concentrations in North America and Europe in each category where an appreciable number of samples exists. The average difference in concentration for samples from the Northern and Southern hemispheres is in agreement with that obtained for adults.

The concentration of strontium-90 varies with age for young people at the present time, since there has not been sufficient time for their skeletons to equilibrate with strontium-90 in the diet. Strontium-90 is deposited directly in the formation of new bone and through exchange and other turnover processes in preexisting bone. It is instructive to compute the concentration of strontium-90 as a function of age for a population with a given strontium-90 content in the diet and to compare this theo-

retical curve with the experimental data. Since the United States, Canada, and Europe appear to have strontium-90 levels in soil, food, and human skeletons that are closely similar (*9, 10*), bone samples from these areas can be grouped for this purpose, thus providing a larger number of samples to test the theory.

The calculation requires knowledge of: (i) the strontium-90 concentration in the average diet for the past five years; (ii) the discrimination factor for strontium relative to calcium from diet to bone; (iii) the number of grams of bone deposited per year at each age as a result of skeletal growth; and (iv) the magnitude of the average bone turnover per year.

The strontium-90 concentration in the average diet of individuals in these areas has been estimated from the data of the Atomic Energy Commission's Health and Safety Laboratory (*11*), the Lamont Geochemical Laboratory (*9, 12*) and the British group (*10, 13*). The resultant averages in micromicrocuries of strontium-90 per gram of calcium for North America and western Europe for 1 July of each year from 1953 through 1957 are 0.4, 1.6, 3.6, 5.1, and 6.5, respectively.

The data for the average amount of bone deposited per year are taken from Mitchell *et al.* (*14*) for males in the United States.

Schulert *et al.* (*7*) obtained a discrimination factor of 4 for calcium against strontium from diet to bone, on the basis of double-tracer (calcium-45, strontium-85) intravenous single doses

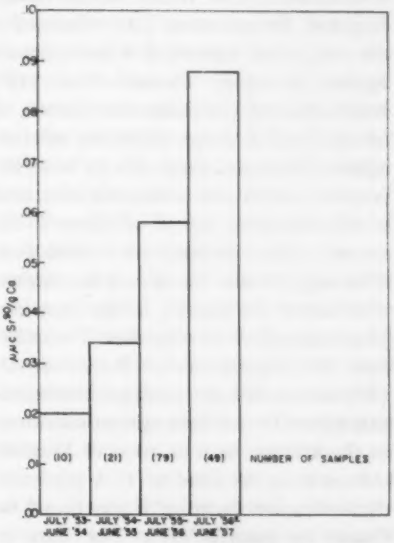

Fig. 1. Concentration of strontium-90 in whole skeletons of New York City adults.

Table 3. Level of strontium-90 in children's bones, 1957–1958. Number of samples is given in parentheses.

Age	Level of Sr^{90} (μμc/g of Ca)				
	North America	Europe*	South America	Asia	Africa, Australia
Fetus	0.64 (9)	0.60 (27)		1.3 (15)	0.25 (2)
0– 6 mo	1.18 (8)	1.15 (28)		0.21 (4)	0.45 (1)
7–12 mo	1.84 (1)	1.47 (5)	0.58 (38)	1.15 (4)	
1– 2 yr	1.53 (7)	1.39 (9)	0.54 (5)	0.11 (1)	0.27 (1)
2– 3 yr	1.49 (9)	1.71 (4)	1.28 (1)		0.60 (1)
3– 4 yr	1.23 (7)	0.87 (3)			0.43 (5)
4– 5 yr	0.74 (5)	0.73 (3)			0.48 (3)
5– 6 yr	0.70 (12)	0.66 (6)	0.28 (10)		0.55 (2)
6– 8 yr	0.60 (12)	0.61 (5)	0.39 (4)	0.28 (2)	0.53 (5)
8–10 yr	0.88 (11)	0.39 (4)	0.28 (8)	0.09 (1)	1.50 (2)
11–15 yr	0.51 (21)	0.55 (5)	0.22 (5)	0.50 (3)	0.49 (8)
16–19 yr	0.39 (11)	0.24 (3)	0.20 (15)	0.45 (2)	0.22 (4)

* Data from Bryant *et al.* (*28*) on European children for 1957–1958 were included to give a better comparison for Europe and North America.

in human patients and of previous work by Spencer *et al.* (*15*) on discrimination across the gastrointestinal wall in man. In an experimental study involving the stable strontium-to-calcium ratios in the normal diet and the skeletons of people in communities of Wales and northwest England, Bryant *et al.* (*10*) obtained a discrimination factor of 4 for calcium against strontium. Comar *et al.* (*16*) have obtained discrimination factors of about 2 and 4, respectively, for calcium against strontium from diet to bone for rats fed a milk and a nonmilk diet, and a discrimination factor of about 2 for several human patients on a milk diet. This suggests that the actual discrimination factor for human beings may depend somewhat on the diet. Nevertheless, the experiment of Bryant *et al.* (*10*) shows that the average discrimination factor for calcium against strontium in the average man in areas of Western culture must be close to 4. A placental discrimination factor of 2 was found by Comar in animals (*17*). The data in Fig. 2 for North America and Europe indicate that this factor is about the same in human beings.

If a discrimination factor of 4 for calcium against strontium from diet to bone is assumed, the average turnover rate for the normal adult can be calculated from the known concentration of strontium-90 in the diet over the past five years and the present average concentration of strontium-90 in adult bone. The result is 3.8 percent per year. If the behavior of strontium is assumed to be similar to that of calcium, a maximum of 1 percent may be accounted for by intracrystalline exchange, whereas perhaps another 1 percent is due to remodeling. The rest is accounted for by passage to the more spongy bone, where the rate of turnover is much higher. Once this latter reservoir becomes saturated, the apparent average turnover rate will drop to the intracrystalline exchange and remodeling rate. From the experimental data the gross average turnover for adults is calculated to be 3.8 percent per year during this period. It is assumed for purposes of calculation that this same rate applies to children, although their rate of turnover is probably higher. Since the proportion of calcium deposited in the process of growth relative to that deposited through turnover is large in the case of young children, no great uncertainty is introduced through basing the calculation for children on this rate.

Table 4. Levels of strontium-90 in adult bone from 1955 through 1958. Number of samples is given in parentheses.

Location	Sr^{90}(μμc/g of Ca)		
	1955–1956	1956–1957	1957–1958
40°–60°N	0.12 (177)	0.12 (176)	0.19 (111)
20°–40°N	0.14 (88)	0.13 (114)	0.19 (44)
0°–20°N	0.11 (130)	0.20 (63)	0.16 (101)
0°–20°S	0.07 (41)	0.08 (22)	0.25 (16)
20°–40°S	0.11 (88)	0.12 (78)	0.14 (90)

The theoretical curve computed in this way is given in Fig. 2 and is compared with the experimenal data for children's bones in North America and in western Europe obtained in this study and by Booker *et al.* (*13*). The standard deviation in the analyses for any given age group is about 30 percent of the mean for those sets in which there are at least eight samples. This variation is probably due primarily to individual differences in diet. It is seen that the points for the various age groups follow the curve reasonably well, the greatest divergence being found in early adolescence. The ratio of the maximum, which occurs at about age 1 year, to the adult average is about 8; this ratio will be reduced if the concentration of strontium-90 in the diet becomes more constant with time.

On the basis of the values given above for the bones of individuals in North America between the ages of 0 and 20, it is now possible to weight the level of strontium-90 in bone by the fraction of

the population in each age group and thus to compute the present world average concentration of strontium-90 in human bone. This calculated world average as of 1 January 1958 is 0.52 μμc of strontium-90 per gram of calcium.

Future Human Burden of Strontium-90

The accumulation of new data relative to the concentration of strontium-90 in human bone and diet and to the deposition of strontium-90 on the earth's surface makes it possible to predict future levels in the human population with greater accuracy. This calculation is based upon atmospheric contamination from nuclear tests to the end of 1958 and on the assumption that the diet level is proportional to total fallout. The calculation tacitly implies that strontium-90 will be distributed in the future approximately as it has been in the past, although the Soviet tests of October 1958 may alter the distribution to a minor extent in the direction of increasing the amount deposited in the North Temperate Zone in the future. The quantities of greatest interest are the anticipated concentrations of strontium-90 in the diet and the related anticipated levels of strontium-90 in newly formed bone.

The concentration of strontium-90 in the average Western diet was 6.5 μμc per gram of calcium on 1 July 1957, when the level in milk was 6.1 μμc per gram of calcium. A comprehensive survey of U.S. milk in the United States in July of 1958 gave an average of 8.2 μμc of strontium-90 per gram of calcium (*18*), which is equivalent to a level in the diet of 8.7 μμc of strontium-90 per gram of calcium. To extrapolate to 1 January 1959, it follows that the Western diet included 9.7 μμc of strontium-90 per gram of calcium on that date. Also, on 1 January 1959 the cumulative deposition of strontium-90 on the surface of the earth was about 2.2 megacuries (*19*). In the light of data on the isotopic composition of stratospheric debris that have been derived from new rain and from balloon sample analyses (*20*), it seems likely that the mean residence time of strontium-90 in the stratospheric reservoir is three years rather than the five (*21*) or ten (*22*) that have been estimated previously. From this figure and from the deposition rate it is possible to estimate that the stratospheric reservoir contained about 1.8 megacuries on 1 January 1959, exclusive of debris from the Soviet tests of October 1958. If the latter contributed as much as a major test series—for example, the United States' Redwing series of 1956 (*22*)—the total stratospheric reservoir would be about 2.6 megacuries. On the basis of these three assumptions—2.6 megacuries to be deposited, a mean residence time of about three years, and a discrimination factor of 4—when due allowance has been made for radioactive decay, the strontium-90 concentration in the diet, and hence in newly depositing bone, can be readily computed. The result (Fig. 3) shows that a peak in the level of strontium-90 in new bone will occur about 1966. The average concentration in the skeletons of young children on a Western diet in the Northern Hemisphere at that time will approach but not reach this peak level.

This estimate of the future levels that will be reached if there is no further introduction of fission debris into the atmosphere is probably maximal. One of the fundamental assumptions in the calculation is that the strontium-90 levels in the diet will be proportional to the *cumulative* fallout (*23*), and others have emphasized that the level in the diet may be related also to the *rate* of fallout, and that in some cases the rate factor may predominate. At present the data are not adequate for quantitative assessment of the relative importance of the rate factor. If it should be determined, for example, that as much as half of the up-

take of strontium-90 by plants is due to the rate of fallout, the predicted curve might have to be lowered by as much as a factor of 2. The rate of fallout, however, did not change appreciably from 1954 to 1957, whereas the skeletal concentration of strontium-90 in an urban population increased markedly (Fig. 1). It is concluded, therefore, that cumulative fallout is the dominant factor for the average diet in areas of Western culture, but the data cannot rule out the possibility of a contribution from the rate factor which would reduce the predicted levels (Fig. 3). Other factors that would also reduce the levels in diets in the future would include loss of strontium-90 by vertical migration in the soil, or chemical fixation. Neither of these factors is considered significant (*19*). The plowing of pasture land, however, will probably cause some reduction in the levels of strontium-90 in food over those that have been predicted.

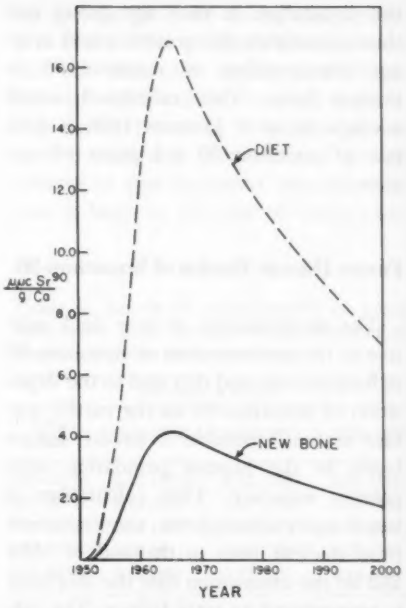

Fig. 3. Uptake of strontium-90 in food and new bone in the area of Western culture, 20° to 60°N.

World-wide Distribution

The other question of importance is that of the ultimate distribution of concentrations of strontium-90 in the world population. This is complicated to such an extent by the fact that there is great variation with age in children—a result

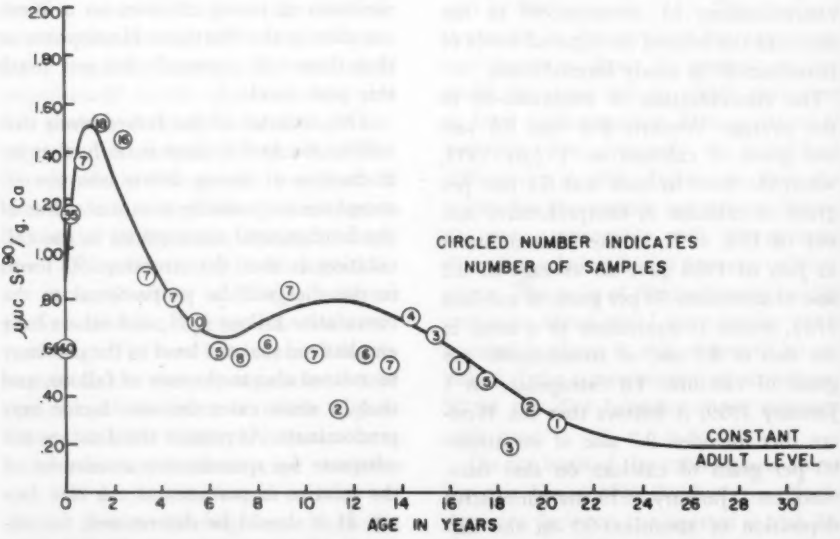

Fig. 2. Concentration of strontium-90 in the skeleton, in January 1958, in North America and western Europe.

of the rapidly changing levels of strontium-90 in the diet (Figs. 2 and 3)—that a histogram of all bone samples would not be significant. The fact that the strontium-90 concentration in adult bone is independent of age and that the average values over large geographical areas are rather similar provides a suitable characteristic for a frequency plot. As the rate of change of levels of strontium-90 in the diet decreases, it is to be expected that the shape of the distribution curve for the whole population will approach that obtained from adult samples at the present time. The ultimate variation for the world population occasioned by the differences in the concentration of strontium-90 in individual diets is already reflected in the skeletons of adults.

Four histograms are presented in Fig. 4. These cover all the more recent (1957–58) samples. They represent, from top to bottom, (A) values for whole-body adult samples (80 skeletons) from New York City; (B) computed skeletal values from the measurement of individual adult bone samples from the northeastern United States (100 samples); (C) computed skeletal values from individual adult bone samples from rice-diet areas (Thailand, Taiwan, the Philippines, and Japan) (126 samples); and (D) computed skeletal values from individual adult bone samples from the whole world (838 samples).

Although the number of samples represented in histogram A (whole-body adult samples from New York City) was limited, the radiochemical analyses of the whole-body ash are more accurate than the skeletal values computed from analyses of single bones.

The histogram (B) of the skeletal burden computed from 100 analyses of individual bones from Boston and New York shows a larger spread than (A). This range is more apparent than real since the analyses of the single bones show an average counting error of about 30 percent, whereas the counting error on the whole-body ash averages about 10 percent for 1957–58 samples. Furthermore, the biological variation within a single bone and variation in the particular-bone–whole-skeleton ratio extend the range of the data. These factors may readily account for the difference in the values shown in histograms A and B; hence it may be concluded that the whole-body samples from New York City give the more accurate representation of the distribution of concentrations of strontium-90 in the population of the northeast United States and probably in most of the population living on a Western diet in the latitude band of 20° to 60°N.

Histograms B (northeastern United States) and C (rice-diet areas) may be compared directly, since they are both derived from single-bone analyses. It is clear that in the rice-diet areas the variation in strontium-90 concentration in the population is larger by a factor of about 2 than in the northeastern United States. This is probably a result of the more restricted flow of food in the Orient, the higher average contribution of vegetable calcium to the diet, and the greater variability of the vegetable-calcium–milk-calcium ratio in the diet in rice-diet areas. Also, the concentration of strontium-90 tends to vary to a much greater extent in vegetables than in milk (*12*).

Histogram D represents analyses of individual adult bones for the entire world for the period 1 July 1956 to 30 June 1958. Although the sampling is not proportional to the total population in each area, this curve provides the best empirical basis available on which to base an estimate of the world distribution of strontium-90 in man. The true distribution is no doubt narrower, as may be deduced from the comparison of histograms A and B above. Nevertheless, the curve given in histogram D is adopted in the discussion that follows, though it is acknowledged to be somewhat too broad. This distribution is clearly not normal, nor does it correspond closely to a lognormal pattern. A

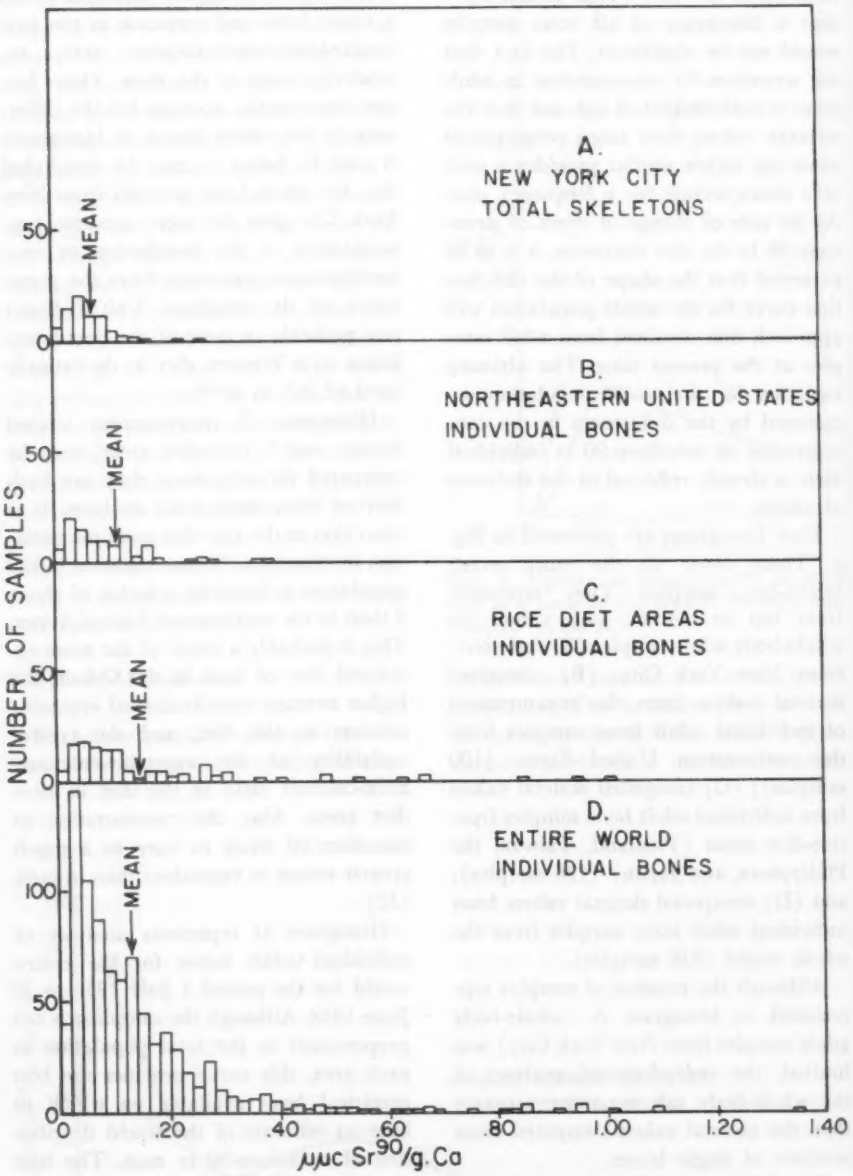

Fig. 4. Histograms showing comparative distributions of strontium-90 in bone.

large fraction of the world's population gets most of its calcium from milk products, while the rest (largely Oriental) gets calcium from a variety of foods, cereals being the most important. That this distribution curve adequately represents the world adult population can be tested in a number of ways. It is useful to examine the range of strontium-90 concentration in milk in areas of Western culture, where milk products are the dominant source of calcium. Milk from a large variety of sources has now been analyzed (*11, 24*). These samples include a set of about fifty collected by this laboratory from untilled farms or from poor land in many parts of the United States. The highest concentration found in these samples was only about five times the mean for the United States. In the high-acidity–low-calcium areas in Kentucky, the highest concentrations in the milk samples were only twice the average for the United States and represented isolated farms. A restricted area in western North Dakota produces a milk that averages three to five times the mean for the United States. The people affected would be those who actually live on the farms that produce this milk, for if the milk is sold into the urban markets and combined with milk from other areas, the level of strontium-90 is quickly reduced. The population that might conceivably be affected is less than 0.1 percent of the population of the United States. Histogram D suggests that concentrations in foodstuffs at twice the mean level do not contribute greatly to the distribution curve.

Numerous food samples have been obtained from various parts of the world and analyzed for strontium-90. Levels in the foods which contribute significantly to the calcium in the diet do not vary by more than a factor of 2 or 3 from the mean in almost all cases. Again, the mixing of foodstuffs prevents individuals from maintaining a diet which is consistently high in strontium-90.

A special field study (*9*) was undertaken to sample the foods of the truly isolated Indian tribes of the uppermost part of the Amazon basin. Here the tropical conditions cause intensive leaching of the calcium from the soil. These people eat only what they can grow within a half mile or so of their village, and they have a very restricted diet. Most of their calcium comes from the yuca, which they prepare in various ways. Studies of a considerable number of sites showed the yuca to contain four to five times more strontium-90 per gram of calcium than components of the average world diet, despite the fact that the actual fallout in this area is considerably less than in latitudes 20° to 60°N. The number of people involved in this primitive situation, where there is virtually no inflow of food and where the mean annual temperature and rainfall are high, does not appear to exceed a few tens of thousands and constitutes less than 0.001 percent of the world population. In the world histogram (D) it is seen that the contribution to the distribution curve from these special groups whose diets contain concentrations of strontium-90 of five times the world average is quite small. It is concluded, therefore, that the empirical distribution curve (D) is reasonably representative of the entire adult world population in 1958 and probably of the whole world population at some future time if the concentration of strontium-90 in the diet becomes fairly constant with time.

A further critical problem is that of the proper method of extrapolating the distribution curve to the higher values. The curve cannot proceed indefinitely to the right for several reasons. Since the fallout is somewhat proportional to integrated rainfall for a given latitude band, and since the highest local rainfall does not exceed the world average by much more than a factor of 5, there is a limit set on the initial variation. Actually, the major agricultural areas have rainfall ranging only from about 20 to 60 inches per year, except in irrigated areas. With time, local variations be-

come averaged out. The available calcium content of the soil can vary widely but, again, the variation is not large over most agricultural areas. The mixing of food completely obscures any local highs from the two factors discussed above so that, except for a few primitive areas, the high and low concentrations of strontium-90 in plants will not produce an equivalent range in man. Even the most primitive people tend to get their calcium from more than one type of food, and, further, they vary the source of their foods during their lifetime. Again, all these factors tend to equalize the diet. From all of the data at hand, it appears doubtful that any person in the world will have a sustained diet which exceeds the world average for strontium-90 concentration by as much as a factor of 20. This, then, sets the upper limit to the distribution curve.

Finally, it is of interest to consider what the distribution of strontium-90 among young children will be in 1966 when, it is estimated on the basis of tests to date, the maximum intake of strontium-90 in the diet will occur. The average concentration of strontium-90 in the skeletons of young children will then be about 4 μμc of strontium-90 per gram of calcium. The curve for the present distribution of strontium-90 in adults (Fig. 4, D) probably also represents the distribution for children of a specific age, since it comprehends the variation in world diet. On this basis it is predicted that at the time of maximum intake in the diet (1966), 10 percent of the young children will be found to have a skeletal concentration of strontium-90 of twice the mean, and 1 percent, of about five times the mean, and that none will have a concentration exceeding 80 μμc of strontium-90 per gram of calcium.

What hazards these levels present to the human race are still not certain. If suggestions, by Brues (*25*) and Finkel (*26*), that there is a threshold for the induction of leukemia or bone tumor are correct, no person will be injured by the strontium-90 that will be deposited by world-wide fallout from nuclear detonations that occurred up to the end of 1958. If the incidence of bone cancer and leukemia is linear down to zero radiation, as Lewis (*27*) and others have suggested, the number of cases can be readily computed for any future year (Figs. 3 and 4; Table 2).

Conclusions

1) In January of 1958 the average burden of strontium-90 in the skeletons of adults throughout the world was 0.19 μμc of strontium-90 per gram of calcium. This burden is independent of age above the age of 20. The concentrations of strontium-90 in bone differ less than the total fallout, as between the Northern and Southern hemispheres. This is attributable largely to the movement of food, particularly of powdered milk and wheat from the Northern Hemisphere into the Southern Hemisphere.

2) A theoretical curve for concentration of strontium-90 versus age has been constructed on the basis of findings on levels of strontium-90 in the diet for the past five years, data on bone growth reported by Mitchell *et al.*, and a discrimination factor of 4 for calcium against strontium from diet to bone. This curve corresponds closely to that for actual data from bone samples from the zone of Western culture of 20° to 60° N.

3) The average concentration of strontium-90 in the skeleton for all people in the world as of January 1958 was about 0.52 μμc of strontium-90 per gram of calcium.

4) On the basis of data on the strontium-90 in diet and bone, the world deposition, and the level of strontium-90 in the stratosphere at the end of 1958, curves have been drawn to show the anticipated concentrations of strontium-90 in the diet of individuals in the area of Western culture of 20° to 60°N and in newly deposited bone to the year A.D.

2000. It is estimated on the basis of these data that the maximum concentration in young children will occur about 1966.

5) A histogram representing 838 samples indicates the present distribution of strontium-90 in the adult population of the world. It is assumed that this reflects the actual spread of all age groups as a result of dietary variations throughout the world. The indicated distribution may be somewhat wider than the actual distribution, due to analytical errors and biological variation in the ratio of single bone to whole skeleton. It is predicted that in 1966 the average young child in the world will have a skeletal concentration of strontium-90 of about 4 μμc of strontium-90 per gram of calcium; that 10 percent may have a concentration of 8 μμc; that 1 percent may have a level of 20 μμc; and that none will have a level exceeding 80 μμc of strontium-90 per gram of calcium.

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Comparative Metabolism of Strontium and Calcium in Man*†

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The metabolic fate of intravenously administered Sr^{85} and Ca^{45} was studied in man. It was found that the isotopes were somewhat equally divided between bone and soft tissue shortly after administration, but that after four months, more than 99 per cent of the fraction of each isotope remaining in the body resided in bone. The initial specific activity (isotope concentration per gramme calcium) was far greater in soft tissue than bone with the values approaching uniformity in about four months. The retention among the bones in the interval studied was greatest in vertebrae and least in long bone shaft and skull. The bone exhibits no marked preference in the uptake of the isotopes initially. However, as Sr^{85} is preferentially excreted by the kidneys, the relative ratio of Ca^{45} to Sr^{85} remaining in bone gradually increases. The net retention of Ca^{45} is about 60 per cent whereas that for Sr^{85} is about 25 per cent.

LE MÉTABOLISME COMPARATIF ENTRE LE STRONTIUM ET CALCIUM DANS L'HOMME

Le résultat métabolique de Sr^{85} et Ca^{45} administré intraveineusement fut étudié dans l'homme. Il fut découvert que les isotopes étaient divisés prèsque également entre l'os et le tissulaire peu après l'administration, mais après quatre mois, plus de 99 pour cent de la fraction de chaque isotope restant dans le corps se trouvait dans l'os. Au dèbut, la radioactivité spécifique (la concentration d'isotope par gramme de calcium) était plus grande dans le tissulaire que dans l'os, les valeurs approchant de l'uniformité apres environ quatre mois. La retention entre les os dans l'interval étudié, était la plus grande dans les vertèbres et était la moindre dans les longs os et le crâne. L'os ne montre aucune préférence initialement entre les isotopes. Néanmoins comme le strontium est excreté par les reins de préférence, le rapport rélatif entre le Ca^{45} et le Sr^{85} restant dans l'os augmente graduellement. La retention nette du Ca^{45} est près de 60 pour cent et près de 25 pour cent pour le strontium.

СРАВНИТЕЛЬНЫЙ ОБМЕН СТРОНЦИЯ И КАЛЬЦИЯ В ЧЕЛОВЕКЕ

Изучалась судьба введенных человеку внутривенно стронция-85 и кальция-45. Показано, что вскоре после введения оба изотопа распределяются более или менее равномерно между костями и мягкими тканями. Однако, через 4 месяца после введения

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‡ Deceased 7 June, 1958.

в костях обнаруживается более 99% от общего количества изотопа, содержащегося во всем теле. Начальная удельная активность (концентрация изотопа на грамм кальция) в мягких тканях значительно выше, чем в костях, однако по истечении 4-х месяцев удельные активности практически выравниваются. При сравнении содержания изотопа в различных костях отмечено самое высокое поглощение в позвоночнике и самое низкое—в длинных трубчатых костях и в черепе. В начальный период для костей не наблюдается заметной избирательности поглощения вводимых изотопов; однако, с течением времени отношение содержания в костях кальция-45 к стронцию-85 постепенно увеличивается, так как через почки преимущественно выделяется стронций-85. При этом поглощение в костях кальция-45 достигает приблизительно 60% и стронция-85—около 25%.

METABOLISCHER VERGLEICH VON STRONTIUM UND CALCIUM IM MENSCHLICHEN KÖRPER

Der metabolische Ablauf von intravenös verabreichten Sr^{85} und Ca^{45} wurde erprobt im menschlichen Körper.

Es wurde festgestellt, dass die Isotopen kurz nach Verabreichung gleichmässig verteilt waren zwischen Knochen uns Geweben, jedoch nach vier Monaten, mehr als 99 prozent jedes einzelnen Isotopen die im Körper verblieben, waren in den Knochen enthalten. Die ursprüngliche Tätigkeit (Isotopen Konzentration per Gramm Calcium) war bedeutend grösser in weichen Geweben als in den Knochen; nach vier Monaten waren die Auswertungen beinahe einheitlich. Die Behaftung zwischen den Knochen während der Versuchszeit war am grössten in der Wirbelsäule und am geringsten in Röhren- und Schädelknochen. Ursprünglich zeigt der Knochen keine lange markante Bevorzugung in der Aufnahme von Isotopen. Jedoch, da Sr^{85} durch die Nieren bevorzugt ausgeschieden wird, vergrössert sich das relative Verhältnis von Ca^{45} zu Sr^{85} allmählich. Der Netto-Rückstand von Ca^{45} ist etwa 60 prozent wogegen der des Sr^{85} etwa 25 prozent beträgt.

INTRODUCTION

Strontium represents a trace element in the human body, having an average concentration of 4·5 parts per 10,000 parts of calcium in bone.[1]

Widespread interest in strontium today is due to the presence of the long-lived radioisotope Sr^{90} among the products of nuclear fission.[2] Strontium-90 is generally regarded as the principal health hazard among the fission products since it is selectively taken up by the human skeleton where it remains deposited for many years.[3] In order to be able to evaluate the radiotoxicity of Sr^{90} in man, the knowledge of the metabolism of the element in humans is of paramount importance. Extensive work has been done to determine the absorption and excretion of administered strontium and calcium isotopes.[4] The present study was designed to show the relative retention of strontium compared to calcium in man and to determine the distribution of administered isotopes within the body including both bone and soft tissue.

EXPERIMENTAL PROCEDURES

1. *Clinical design and sampling*

Tracer quantities of Sr^{85} [$T_{\frac{1}{2}}$ 65 days] and Ca^{45} [$T_{\frac{1}{2}}$ 163 days] in chloride form were administered in a single dose simultaneously to ten terminal cancer patients by the intravenous route [Table 1]. The average dose was 1·5 μc/kg for Sr^{85} and 0·4 μc/kg for Ca^{45}. The intravenous route of administration was selected since it excludes the parameter of gastro-intestinal absorption. Since autopsy analyses were employed, the patients were, of necessity, of limited life expectancy with cancer involvement, and cannot be considered as normal healthy adults. Survival times of patients studied varied from 3 hr to 124

TABLE 1. List of patients studied

Patient	Age	Sex	Wt. (kg)	Time interval isotope admin.–death (days)	Clinical diagnosis	Skeletal involvement
H. H.	62	F	45	1/8	Carcinoma of pancreas	None
S. K.	50	M	54	2	Hodgkins disease	All vertebrae
S. J.	60	F	54	3	Carcinoma of colon	Moderate
C. C.	59	F	73	3	Carcinoma of breast	Diffuse
J. Z.	72	M	73	6	Carcinoma of lung	Diffuse
G. B.	70	F	54	18	Carcinoma of breast	None
S. G.	49	M	50	30	Multiple myeloma	Moderate
T. M.	70	M	43	39	Carcinoma of lung	Single area of skull
A. W.	63	M	61	65	Carcinoma of kidney	Thoracic vertebrae
M. S.	70	F	56	124	Carcinoma of lung	None
J. T.	67	M	76	251	Multiple myeloma	Moderate
I. J.	55	F	53	960	Multiple myeloma	Diffuse

days. In addition, samples were obtained from one patient who received Sr^{85} only, 251 days prior to death and from one patient who received Ca^{45} only, 960 days before death. At autopsy, samples of many soft tissues and of bone were taken for analysis. As many as twenty-five soft tissue samples were obtained in the early studies while later sampling and analysis of soft tissues were restricted to four major organs, namely, muscle, liver, kidney, and lung. Except for muscle, these organs were obtained *in toto* and only small sections were removed for histologic analysis. In regard to bone, samples of vertebrae and ribs were obtained from all patients and in seven of the ten cases either femur shaft, iliac crest, and skull were obtained. Particular attention was given to the variation among bones of the skeleton as well as the distribution within single bones.

2. *Radiochemical assay*

Strontium-85 is readily assayed, since it emits a γ-ray of moderate energy. The sample is merely dried, ashed, and counted in a well-crystal γ-ray spectrometer. The isotope counted with an efficiency of approximately 10 per cent with an associated background of about 10 counts per min. (Slight variations in both of these values were constantly checked by appropriate blanks and standards.)

Two chemical procedures were used in the Ca^{45} assay, one adapted to bone and the other for soft tissue.

Ashed bone is dissolved in HCl after which the pH is adjusted to 3·0–3·5 with ammonium hydroxide. A mixture of oxalic acid and ammonium oxalate is then added to precipitate the calcium oxalate. (Care is taken to avoid a pH higher than 3·5 since this precipitates some calcium as the phosphate.) The calcium oxalate is filtered and converted to the oxide by heating in a muffle furnace. The calcium oxide is weighed, dissolved in HCl, and diluted to 100 ml in a volumetric flask. A convenient aliquot is taken, the pH adjusted, and the calcium again precipitated as the oxalate. After digestion the precipitate is filtered on 29 mm diameter No. 42 Whatman filter paper which is then placed on a brass planchet, covered with a thin piece of pliofilm and placed in a low-level end window β-counter.

For the determination of Ca^{45} in soft tissue, the sample is ashed in platinum and then fused with sodium carbonate at 1200°C. The fusion mixture is leached with water, filtered to remove sodium phosphate and soluble carbonates, and the residue dissolved in hot HCl. After cooling, ammonium hydroxide is added to pH 7, precipitating ferric hydroxide which also occludes remaining phosphate ion. The precipitate

is filtered with the calcium remaining in the filtrate. To remove traces of calcium which may have been carried with the ferric hydroxide, the precipitate is redissolved in HCl and again precipitated, the filtrate being combined with the previous one. Excess ammonia is expelled by boiling. Oxalic acid and ammonium hydroxide are added to precipitate the calcium oxalate. After digestion and cooling, the oxalate is filtered and counted in the same manner as the bone samples.

The β-counter in which the Ca^{45} samples are counted is equipped with an anti-coincidence ring so that background is about 2 counts per min. The counting efficiency for Ca^{45} varies with particular counters from 8 to 12 per cent so that 10^{-6} μc can be adequately detected in a sample. The Sr^{85} which follows the calcium through the chemistry, registers on the β-counter with an efficiency of only 0·3 per cent. As the Sr^{85} concentration is known from the γ-assay, this small correction for the Sr^{85} is made in the β-count.

PRECISION ANALYSIS AND TREATMENT OF DATA

In order to properly evaluate the experimental findings, it is necessary to know the relative precision of each particular result. The problem may be considered in three categories:

(1) the precision of the radiochemical determinations,

(2) the biological precision of different portions of the same tissue, and

(3) the precision between the tissues of two individuals treated in an identical manner.

There is also the matter of statistical error of counting but the counting time was almost always sufficient to reduce this to below 1 per cent, making it negligible compared to the other variations.

The chemical precision is of concern primarily in the Ca^{45} determination, since essentially no chemistry is involved in the Sr^{85} assay. Duplicate and in some cases triplicate samples were analysed. The average of the mean deviations for the Ca^{45} determinations in these small groups is 4·8 per cent and 7·5 per cent in bone and soft tissue respectively.

In comparing the biological variation in two or more parts of the same tissue, an effort was made to obtain representative samples, e.g. a cross section of bone or the half of a kidney cut along its major axis. (In subsequent discussion, systematic variations within bone are noted.) The average of the mean deviations for the Ca^{45} determinations in these groups is 22·0 per cent for soft tissue and 16·4 per cent for bone.

The corresponding values for Sr^{85} are 14·5 per cent for soft tissue and 17·1 per cent for bone.

There is insufficient direct information to assess the biological variation from one individual to another. However, the relative distribution of isotopes in the skeletons of different individuals gives some indication. Thus the standard deviation in the vertebra/rib concentration ratio for Sr^{85} is 30 per cent showing, as expected, that variation among individuals is somewhat greater than variation in representative samples in a single tissue. The concentration ratio of one bone to another is changing in time, as subsequently discussed, but the change is slight over a four month interval.

These facts account for a large amount of the "scatter" in the data and underline the need for caution in its interpretation. Nevertheless, certain generalizations and a number of significant facts can be drawn from the experimental results.

In order to give a gross overall picture of the behavior of the isotopes in the human body in time, the total body load is calculated for each patient. A standard procedure, which admittedly involves considerable approximation, is employed as follows:

Soft tissues

(1) The weights of lung, liver, and kidney were obtained at autopsy. Muscle weight is estimated from the total body weight using the body composition data of MITCHELL *et al.*[5]

(2) The total content of each tissue is then calculated by multiplying the estimated tissue weight by the tissue concentration.

(3) Accordingly, these four tissues constitute 40·3 per cent of the total body weight, whereas the total soft tissue comprises 85·2 per cent. On the assumption that the tissues analysed are representative of all the soft tissue, the total isotopic content of these 4 tissues is multiplied by 85·2/40·3 to give the total estimated isotopic content for soft tissue.

Bones

The bones are divided into three groups, which according to the data of TROTTER,[6] comprise the following percentages of the total skeleton:

I	"Long" bone: limb, hip, skull (and mandible)	70·6%
II	Rib	5·7%
III	Vertebra and sternum	8·9%
	Total	85·2%

(1) The weight of each bone analysed is estimated from the body weight and the data of TROTTER[6] together with the data of MITCHELL *et al.*[5] for the percentage of total skeletal mass.

(2) Bones within a group which are not represented are assumed to have the same value as the average of the others in order to estimate the total isotopic content for each group.

(3) When an entire group is not represented, its value is estimated by using the femur/rib ratio (for "long" bone) and the vertebra/rib ratio (for the vertebra/sternum group) as obtained from the average on all patients.

(4) The total estimated isotopic content of the three groups is multiplied by 100/85·2 to get the estimated total skeletal content.

RESULTS AND DISCUSSION

The distribution of Sr^{85} and Ca^{45} in the body, the percentage of the isotopes accounted for, and the ratios of each isotope contained in bone and soft tissues are listed in Table 2. Also shown are the ratios of Ca^{45}/Sr^{85} in bone and soft tissues. In spite of the problems of representative sampling the approximations entering in the calculations, and the fact that the extent and degree of the cancer involvement varied markedly, a fairly consistent pattern is obtained. The limitations of this analysis are demonstrated most sharply by the calculated Ca^{45} retention in patient C. C. at three days (127 per cent). There is a partial explanation for this since the patient (a female weighing 72·6 kg) had a higher percentage of adipose tissue than normal. Thus the standard assumption that the skeleton represents 14·8 per cent of the body undoubtedly gives an estimated bone weight in this case which is in excess of the true value. Since the concentration of the isotopes per gramme of bone is much greater than of soft tissue, the standard procedure results in a high value for total body retention. It is noted that both isotopes are somewhat equally divided between bone and soft tissue shortly after their administration and that the proportion gradually shifts so that less than one-half per cent of the body load of each isotope is retained in the soft tissues by 124 days. This is primarily due to the more rapid removal of the isotopes from the soft tissues. The data also suggest some translocation from soft tissue to bone, particularly of Ca^{45} as previously noted by others in animals over short intervals.[7] This translocation is shown graphically in Fig. 1. Whereas the skeletal Sr^{85} retention begins to decline within the first few days and

TABLE 2. Distribution of Sr^{85} and Ca^{45} in soft tissues and bone; tissue concentrations expressed in per cent of administered dose

	Patient days*	H. H. 1/8	S. K. 2	S. J. 3	C. C. 3	J. Z. 6	G. B. 18	S. G. 30	T. M. 39	A. W. 65	M. S. 124
Sr^{85}	Soft tissue	55	31	44	35	26	22	1·2	1·4	1·5	0·15
	Bone	33	47	36	45	42	41	20	26	30	26
	Total	88	78	80	80	68	63	21	27	32	26
	Sr^{85} ratio bone/soft tissue	0·60	1·5	0·82	1·3	1·6	1·8	17	19	20	173
Ca^{45}	Soft tissue	33	28	28	53	32	14	3·0	5·5	1·9	0·27
	Bone	30	42	55	74	59	36	35	71	79	60
	Total	33	70	83	127	91	50	38	76	81	60
	Ca^{45} ratio bone/soft tissue	0·91	1·5	2·0	1·4	1·8	2·6	12	14	42	222
Ca^{45}/ Sr^{85}	Soft tissue	0·60	0·90	0·64	1·5	1·2	0·64	2·5	3·9	1·3	1·8
	Bone	0·91	0·89	1·5	1·6	1·4	0·88	1·8	2·7	2·6	2·3

* Days elapsed between the injection of the isotopes and *exitus* of patients.

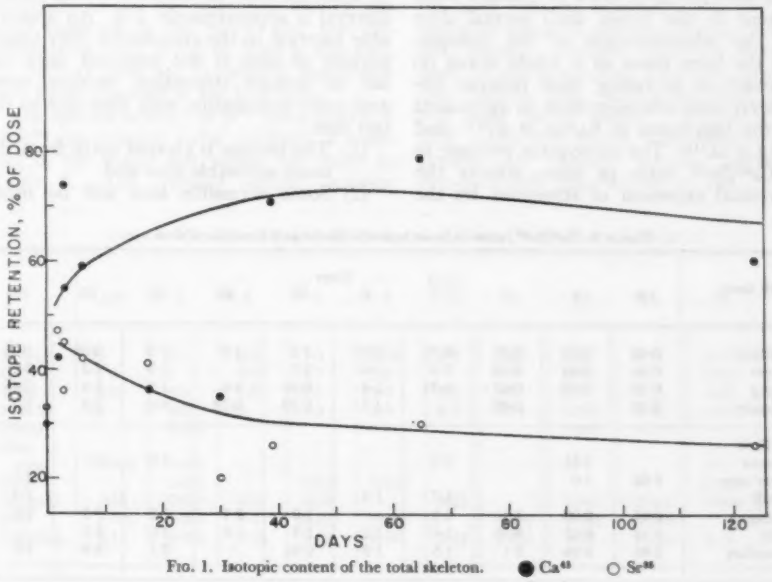

FIG. 1. Isotopic content of the total skeleton. ● Ca^{45} ○ Sr^{85}

approaches a plateau at about 25 per cent, the Ca^{45} appears to rise for the first month and levels off at about 65 per cent. Three experimental points are badly off the Ca^{45} curve, but these are the three least valid values of the ten: the high 3 day point [C. C.] is subject to question as previously mentioned, and the low points at 18 and 30 days represent the only skeletons for which just two bones were obtained for analysis. The retention values for the two isotopes are consistent with those obtained in balance studies over shorter intervals by other investigators.[4, 8, 9, 10]

The ratios of Ca^{45}/Sr^{85} of the individual tissues are listed in Table 3. Although these ratios are somewhat more variable for soft tissues than for bone, both show an increase with time; the values for bone being generally 2 or above after 30 days. This is a reflection of the fact that Sr^{85} is preferentially eliminated from the body and thus has a shorter average biological residence time than calcium.

The enrichment in favor of calcium is not apparent in the bones until several days after the administration of the isotopes. Thus the bone tissue as a whole shows no preference in accepting both isotopes immediately after administration in agreement with the conclusion of BAUER *et al.*[11] and COMAR *et al.*[12] The subsequent increase in the Ca^{45}/Sr^{85} ratio in time reflects the preferential excretion of strontium by the kidneys. It has been stated that the overall calcium enrichment from diet to bone is governed primarily by the calcium/strontium discrimination factor across the intestine and by that of urinary excretion.[13, 14] In the course of time, the renal discrimination factor is reflected by the retention of the isotopes in the body, i.e. primarily in the skeleton. Whereas the intestinal discrimination is essentially a one step process (disregarding the endogenous fecal strontium) in that the isotopes which reach the intestinal tract are either taken across the intestinal wall or eliminated from the body, the kidney discrimination is a cyclic process in which ions that have been retained in the body during one circulatory cycle and via the renal tubules are presented to the kidney again during the next circulatory cycle. This repetitive renal process leads to continued preferential excretion of radio-strontium and therefore to increased relative retention of radio-calcium. The factor of net effective enrichment from blood to bone for Ca^{45} in the two to four month interval is approximately 2–3. An appreciable increase in the enrichment over longer periods of time is not expected since the *loci* of isotope deposition become more and more inaccessible with time due to the fact that:

(1) The isotope is cleared early from the more accessible sites and
(2) Some accessible sites will be made

TABLE 3. Ca^{45}/Sr^{85} ratios in bone and soft tissues as a function of time

Soft tissue	Days									
	1/8	2	3	3	6	18	30	39	65	124
Kidney	0·46	0·86	0·81	0·74	0·91	1·2	1·3	1·5	0·58	0·87
Liver	0·34	0·84	0·63	1·3	1·6	1·7		1·9	1·2	3·0
Lung	0·28	0·68	0·67	0·74	1·4	0·50	4·6	1·2	1·1	0·44
Muscle	0·72		0·88		1·7	0·70	0·28	8·0	1·4	1·7
Bone										
Femur		0·81		1·9				1·8	2·1	
Iliac crest	0·86	1·0								2·7
Skull				1·7	1·5					2·3
Sternum	0·94	0·90	1·1	1·4		1·0	2·4	2·2	3·5	2·0
Rib	0·74	0·82	0·76	1·4	1·3	1·3	1·0	3·6	2·2	
Vertebra	0·86	0·94	2·1	1·5	1·3	0·94		2·7	3·8	1·9

unavailable by the continued deposition of new bone mineral.

Although extensive data on the excretion of Sr^{85} and Ca^{45} in man have been obtained,[4, 15] these excretions have not been followed for prolonged periods of time. One would expect the elimination of administered Sr^{85} and Ca^{45} to follow a power function as in the case of radium.[16] While the present data on bone retention of the isotopes are inconclusive, they suggest that only slight changes are occurring after the second month, compatible with such an elimination pattern.

Since the Ca^{45}/Sr^{85} retention ratio reaches two in two months, and since these isotopes present in bone remain there for years, it is concluded that this enrichment will also occur under continuous intake of Sr^{90} due to fallout. A comparison of average Sr^{90} content of diet and human bone in northeastern United States by ECKELMANN *et al.*[17] indicates that the overall calcium/strontium discrimination from diet to bone is four. Since the discrimination against strontium across the intestinal barrier has been reported to be two[4] and the discrimination from blood to bone is also two, the present data are in general agreement with those reported in the literature.

The specific activity of Sr^{85} (concentration of Sr^{85} per gramme stable calcium) of bones and of the major soft tissues are listed in Table 4. The average specific activity of vertebrae and rib are respectively 5·4 and 2·3 times that of femur. This distribution must clearly change in time as evidenced by the fact that common strontium is uniformly distributed throughout the skeleton.[1] However, this approach to uniformity proceeds very slowly since Sr^{90}, which has been in our environment for several years, is reported to be more greatly concentrated in vertebrae and rib than in long-bone shaft by factors of about four and two respectively.[17] It is apparent that while the isotopes are largely concentrated in the skeletal tissue, the specific activity of the soft tissue greatly exceeds that of bone throughout the initial two month period following injection. It is noted in the 124 day and 251 day samples, however, that the specific activities are of the same order of magnitude. This indicates that about four months are required for the soft tissue to be cleared of the strontium initially deposited therein. Subsequently the isotopic content of the soft tissue probably reflects that which is resorbed from the bone into the blood stream, (the blood probably being in near equilibrium with the soft tissue). Further evidence that this must be the case comes from the data from one patient who was given Ca^{45} alone, 960 days before death. Samples of the four major soft tissues were obtained and showed an average specific activity of one-twentieth that shown by the 124 day

TABLE 4. Specific activity of Sr^{85} in bone and soft tissue (% administered dose/g Ca)

	Days										
	1/8	2	3	3	6	18	30	39	65	124	251
Kidney	21	8·1	8·8	7·6	0·29	2·3	0·69	0·47	0·57	0·17	0·092
Liver	8·0	12	7·4	1·7	5·8			0·54	0·25	0·070	0·043
Lung	8·0	9·5	1·8	5·1	4·8	3·2	1·6	1·4	0·36	0·32	0·019
Muscle	11	3·3	12	4·0	2·9	4·1	0·24	0·25	0·42	0·053	0·056
Femur		0·036						0·024	0·016		
Iliac crest	0·048	0·070								0·11	0·073
Skull		0·031		0·017	0·012					0·027	
Sternum	0·18	0·067	0·21	0·10		0·25		0·067	0·062	0·099	0·14
Rib	0·071	0·035	0·077	0·035	0·052		0·052	0·078	0·056		0·044
Vertebra	0·17	0·095	0·16	0·13	0·16	0·14	0·070	0·16	0·116	0·14	0·11

case. Therefore it may be concluded that at intervals greater than four months, urinary excretion reflects bone resorption. The excretion may also give an index of the isotopic bone concentration, although the relationship is complex. For example, animal experiments have shown that the specific activity of soft tissue may ultimately become much less than that of bone.[18, 19] Thus the relation is largely an empirical one.

It is evident that in the practical experimental evaluation of agents for their effect on removing strontium from the skeleton, the most meaningful results would be obtained at long periods after strontium administration when the specific activity of the soft tissue approximates that of bone, or at least does not greatly exceed it. The induced enhancement of urinary radio-strontium excretion achieved in the early phase[20] will largely represent clearing of soft tissues, whereas a sustained effect produced at later periods will represent removal of the isotope from bone. Studies carried out in our laboratories on the relationship of urinary radio-strontium excretion and bone concentration determined at time intervals of varying length following the administration of the tracer are in progress at the present.

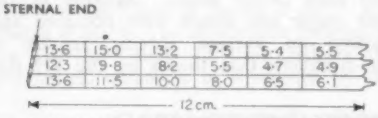

FIG. 2. Variations of Sr^{85} concentration within a rib section.

Fig. 2 shows that the isotope concentration varies greatly within a single bone. For example, Sr^{85} concentration decreases by a factor of greater than two in a section of rib as one proceeds from the sternal end for 12 cm toward the axillary portion of the rib. This finding indicates the biological variation one may expect to encounter in rib samples taken at random.

Table 5 lists the data of radio-strontium concentration in successive one mm layers of the femur which were taken from the external surface and were followed towards the marrow cavity. It is noted that the samples taken after three hours and two days show high activity in the outermost layer followed by a marked decline in the second and third mm. The value then gradually rises with the layer adjacent to the marrow having the highest concentration of all. However, the samples obtained 65 days after the administration of the isotopes show a progressive unidirectional decrease of activity from the outermost layer which has the highest concentration to the inner layer which has the lowest. This may suggest a greater resorption of the isotope through the marrow cavity than from the outer surface of the bone.

TABLE 5. Sr^{85} concentration in various layers of the femur

Time	Depth of bone mm	Concentration in % of dose/kg bone
3 hr	1	4·11
	2	1·64
	3	1·72
	4	1·84
	5	2·25
	Marrow	11·16
2 days	1	6·00
	2	3·96
	3	3·95
	4	4·48
	5	4·57
	Marrow	7·41
65 days	1	2·91
	2	2·01
	3	1·57
	4	1·35
	5	1·21

SUMMARY AND CONCLUSIONS

(1) Tracer quantities of Sr^{85} and Ca^{45} were administered simultaneously to ten cancer patients by the intravenous route and samples of bone and four major soft tissue organs were obtained at autopsy for radio-assay.

(2) The isotopes are approximately equally distributed between bone and soft

tissue for the first few days after administration; after four months about 99·5 per cent of the isotopes which are retained in the body are found in bone.

(3) The net retention of the isotopes appears to level off at about 60 per cent for Ca^{45}, and 25 per cent for Sr^{85}.

(4) There is a gradual increase in the Ca^{45}/Sr^{85} ratio in bone, which reaches a value of two or more in the two to four month interval.

(5) The specific activity of the isotopes is much greater in soft tissue than in bone throughout the first two months after administration, but the values approach the same order of magnitude at four months.

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Strontium-90 Distribution in the Human Skeleton

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INTRODUCTION

Strontium-90 originating from nuclear detonations has now been disseminated throughout the globe so that measurable quantities are present in practically all surface soil, food and human bone. The biological chain of strontium-90 transfer is under extensive investigation so as to assess the present and potential hazard (1, 2, 3). The ultimate quantity of interest is, of course, the amount of strontium-90 which reaches the human skeleton. The distribution within the skeleton is also important for the proper evaluation of the physiological hazard. Thus, if the areas of the skeleton which contain a higher than average strontium-90 concentration are those which are more susceptible to radiation damage, the strontium-90 in the skeleton will be more hazardous than if it were distributed uniformly.

A further practical consideration which has made the determination of the strontium-90 distribution within the skeleton imperative has been the need for properly computing the skeletal load of this radioisotope in man from analyses of single bones. For the past five years the Lamont Laboratory has been assaying bones received from a world-wide network (3). Individual bones from autopsy are supplied, e.g. vertebra, rib, femur, and tibia. If valid comparison is to be made between bones of different types, the skeletal distribution must be known.

The results of Schulert and Laszlo (4) on skeletal distribution in humans who had received a single tracer dose of strontium-85 show that the bones vary widely in the concentration of this isotope shortly after administration. Their average results at time intervals of one day to four months show vertebra and rib concentrations as 6.0 and 1.9 times femur shaft, respectively. This clearly will not be the situation for young children who are depositing new bone or with adults who have had a constant strontium-90 diet for a lifetime. This is evidenced by the fact that stable strontium is uniformly distributed throughout the skeleton (5). The present study was undertaken to determine the actual strontium-90 distribution in the human skeleton at the present non-equilibrium state. It must of course lie between the distribution pattern of the single dose and uniformity.

PROCEDURE

Samples of the principal bones from adult cadavers were analyzed. Effort was made to obtain a representative sample of each bone to typify as closely as possible samples obtained in the world-wide survey program. Therefore, the bulk of the investigation centered on rib, vertebra, and long-bone shaft. In a concluding series equal amounts of identical bones from two groups of cadavers were pooled. This permitted much more accurate analysis.

Whereas the sampling of adult skeletons is adequate, that in children is much more limited. One complete skeleton of a thirteen-year-old male was analyzed. In eight instances, two to four bones each from young children were obtained for analysis. A number of fetal and neonatal skeletons were also measured.

The samples were prepared and assayed for strontium-90 using procedures reported elsewhere (6, 7). Briefly, the method consists of chemically isolating the alkaline-earths, allowing the yttrium-90 daughter of strontium-90 to accumulate, milking off and counting the yttrium-90. Due to the relatively low activity of long-bone shafts, the sample required for precise assay is often too large to use this procedure conveniently. In many of these samples as well as total body samples, therefore, a preliminary separation of strontium from calcium by an adaptation of the method of Burgus (8) was performed.

RESULTS

The concentrations of strontium-90 in three bone types and the total body of 27 individuals and three integrated sets of five individuals each who died in 1956 and 1957 are given in Table 1. In most cases a number of long bones were analyzed in a search for significant differences but within the experimental error these were not observed. The averages indicate the relative activity in vertebrae, rib and long bone. If only one or two bones of a given type were analyzed the error is the standard deviation of the counting statistics. In cases in which three or more bones were analyzed the average value is given together with the standard error on the mean. In some instances the counting error is quite large due to the low concentration of strontium-90. The uncertainty indicated by these standard errors is dominated by the limitation in the precision of the radiochemical analysis rather than by biological variation.

The strontium-90 concentration in the children's bones is given in Table 2. Table 3 summarizes the relative strontium-90 concentrations in the important bones of adults and children and newborns.

Table 4 compares the present values with those used previously (3) based on preliminary data on the strontium-90 distribution in cadavers and short-term strontium-85 investigations (4). The results from this more comprehensive comparison show that the actual strontium-90 variation in adult bone is less than that obtained from the short-term experiments but is still far from uniformity. The uncertainty on these factors (Table 3) can be reduced in the future as samples of higher activity level become available.

DISCUSSION

A. General Pattern

The data show that: (1) the distribution in adults is markedly nonuniform, (2) the distribution in the fetuses and newborns appears to be uniform, and (3) the overall concentration is much higher in children than in adults. These results reflect the fact that the growing bones of young children are metabolically active throughout, whereas the bones of the adult skeleton have lower and more variable activity. From the dietary concentration of strontium-90, it is possible to compute that the equilibrium bone level in this period should be between 1 and 1.5 S.U. (micromicrocuries strontium-90 per gram calcium) (3). This is to be compared with the measured levels in the bones of the 42 individuals reported here i.e., adult skeletal average 0.06, adult vertebrae 0.11, and young children 0.8. Thus children are somewhat close to being

in equilibrium with their diet while even the most active adult bones are far from equilibrium. The values in young children suggest near uniformity with the long bone being slightly lower than the skeletal average. The concentration in the fetal skeleton is significantly less than that in young children in agreement with the fetal protective factor observed in experimental animals by Comar (9).

B. Change in Skeletal Distribution in Time

The nonuniformity within adult skeletons would be only a temporal effect on a constant strontium-90 diet, since common strontium is uniformly distributed in the adult skeleton (5). The present data on strontium-90 are as yet insufficient to measure this trend toward equilibrium. This is due to the fact that (a) total skeletons from the early years are limited in number, and (b) values of strontium-90 concentrations, particularly on long-bone shaft, are too low to measure with adequate precision until 1956. This shift toward equilibrium, however, is definitely seen if the strontium-90 values reported here are compared with those obtained in the short-term strontium-85 experiments of Schulert, Laszlo, et al. (4), as the data in Table 4 clearly indicate.

C. Intra-bone Variations

Considerable intra-bone variations in strontium-90 concentrations are also apparent. Insufficient strontium-90 is present to make radioautographic investigations of discrete areas. However, data from bone segments analyzed indicate, as expected, that the epiphyses, joint areas, and bone extremities generally have higher concentrations than the midsections of the shafts.

In the two most recent composite analyses shown in Figure 1, the joint areas average 57% higher than limb shafts; also rib ends average about 25% higher than the midsections although the data are somewhat scattered. In both of these cases the mid-60% of the bone is taken as the shaft while the rest is included with the joint.

Figure 2 shows the strontium-90 concentration throughout 4 - 5 cm. intervals of tibia and femur from a 13-year-old. There appears to be a progressive change from end to midpoint with extreme values differing by about a factor of four. Bone extremities are generally not analyzed under the Lamont world-wide strontium-90 monitoring program. In previous calculations, insufficient consideration was given to these higher areas of the skeleton in extrapolating from single bone to total body average concentration. Thus it is noted from the summary in Table 4 that while the relative values of the three bone groups are reasonably consistent with the previous data, the value for body average is higher in relation to the other bones than was previously supposed.

D. Radiological Implications

The distribution data on the adult skeleton now make it possible to determine total skeletal load no matter what bone of the body is analyzed. The question arises as to whether the average skeletal concentration or the highest bone concentration is the most important quantity. The investigations of Brues (10)and Finkel (11) suggest a threshold does exist for somatic damage. If a threshold exists for the deleterious effects of strontium-90 then the

highest concentration in a local area of the skeleton is of primary importance, since this is the area which will first exceed the threshold. However, if there is no threshold and the probability of damage throughout the skeleton is strictly proportional to the radiation received, then the total radiation delivered to the skeleton (or the average Sr^{90}/gram calcium) is the important consideration.

Even with a nonthreshold concept, distribution is a consideration in evaluating hazard since certain skeletal areas are more susceptible to radiation than others. For example, since some leukemia is caused by irradiation of the bone marrow, and since about 40% and 25% of the skeletal marrow is located in the vertebrae and ribs respectively, it is calculated from the adult distribution data of Table 3 that the marrow is getting twice the radiation as the average received by the whole skeleton and thus this damage is twice as great as the average values indicate even with the linear, nonthreshold concept. Nevertheless, the nonuniform distribution in the adult skeleton is of little consequence in establishing tolerable fallout contamination since the strontium-90 concentration, as noted, is highest in children which have essentially uniform distribution.

It is generally agreed that the elimination of strontium-90 from the body cannot be defined in terms of a simple biological half life, since the element (in common with the other alkaline earths) is metabolically treated in such diverse manner in different areas of the skeleton (12) The disappearance of strontium from the skeleton may be considered as taking place from a series of loci, each with its own mean residence time. The distribution

data obtained in this investigation show that the residence time for strontium-90 in vertebrae is shorter than that for rib, which in turn is shorter than long-bone shaft (since the metabolically more active bone which accepts the isotope most readily would also lose it most rapidly). However, there are, of course, subcategories of relatively active and inactive areas within each bone type.

SUMMARY AND CONCLUSIONS

1. Various bones of the skeleton and an aliquot of the total skeletal ash were analyzed for strontium-90 in the case of many adult cadavers from the New York City area. A number of fetuses were analyzed in the same manner as well as individual bones (usually 2-4 per skeleton) from children in the 0-14 age group.

2. The distribution of strontium-90 is nonuniform in adults with the concentration in vertebrae, rib, and long-bone shaft being respectively, 1.8, 1.1 and 0.5 times the body average.

3. Young children exhibit essentially uniform distribution.

4. The data permit the determination of total skeletal strontium-90 burden in a population.

Table 1

Strontium-90 in Adult Skeletons 1956-57, New York City
($\mu\mu c\ Sr^{90}/g\ Ca$)

Vertebra	Rib	Long Bone Average *	Total Skeleton
.067 ± .008	.05 ± .007	.02 ± .003	.064 ± .009
.18 ± .02	.13 ± .01 .10 ± .01	.03 ± .007	.163 ± .010
.14 ± .02	.10 ± .02 .04 ± .02	.03 ± .003	.052 ± .004
.05 ± .02	.02 ± .02	.04 ± .015	.038 ± .006
.16 ± .03	.04 ± .03	.04 ± .005	.047 ± .006
.07 ± .02	.03 ± .01	.02 ± .003	.021 ± .004
.03 ± .01	.02 ± .02	.03 ± .003	.014 ± .005
.12 ± .01	.08 ± .01	.05 ± .01	.028 ± .003
.10 ± .01	.03 ± .02	.02 ± .005	.024 ± .005
.17 ± .03	.12 ± .03	.06 ± .01	.042 ± .005
.07 ± .01	.14 ± .02	.01 ± .01	.039 ± .005
.13 ± .02	.05 ± .02	.08 ± .05	.06 ± .02
.09 ± .01	≤.03 .03 ± .02	.04 ± .007	.096 ± .007
.06 ± .02	.03 ± .03 ≤.02	.02 ± .003	.107 ± .008
≤ .02	.03 ± .02 .03 ± .03	.02 ± .005	.072 ± .006
.03 ± .02	.02 ± .01	.02 ± .005	.126 ± .008

* Errors on vertebra, rib and total skeleton are counting errors. In long bone (in which three or more analyses were done) standard error of the mean is given.

Table 1 (cont'd.)

Vertebra	Rib	Long Bone Average*	Total Skeleton
.02 ± .03	.06 ± .01 .04 ± .02	.05 ± .01	.077 ± .012
.12 ± .01	.17 ± .02 .10 ± .01	.02 ± .007	.071 ± .006
.08 ± .02	.06 ± .02 .03 ± .02	.02 ± .003	.079 ± .006
.14 ± .02	.04 ± .02 .10 ± .02	.03 ± .003	.052 ± .004
.09 ± .01	≤ .02 .07 ± .01	.03 ± .007	.052 ± .007
.14 ± .01	.09 ± .01 .10 ± .01	.03 ± .015	.088 ± .007
≤ .04	.045 ± .020 .101 ± .014	.02 ± .003	.062 ± .005
.112 ± .018	≤ .02	.03 ± .01	.016 ± .005
.068 ± .011	≤ .02 ≤ .04	.02 ± .005	.090 ± .006
.12 ± .02	.04 ± .02 .02 ± .03	.02 ± .003	.051 ± .005
.18 ± .01 +	.048 ± .005 .080 ± .007	.012 ± .003	≤ .07
.139 ± .020 +	.114 ± .005	.025 ± .005	≤ .06 **
.095 ± .004 +	.095 ± .006	.033 ± .011	≤ .06 **
Ave. 0.11	.07	.03	.06

* Errors on vertebra, rib and total skeleton are counting errors. In long bone (in which three or more analyses were done) standard error of the mean is given.

** Normalized to early '57 average total skeleton

\+ These are composites of five samples for all bones.

Table 2

Strontium-90 Concentrations in Various Bones of the Skeletons of Children
($\mu\mu c\ Sr^{90}/g\ Ca$)

	Vertebra	Rib	Long Bone
Newborn	.49 ± .07	.54 ± .16	.33 ± .04
	.33 ± .10	.07 ± .13	.17 ± .08
	.35 ± .08	.68 ± .15	.34 ± .04
	.50 ± .09	.59 ± .17	.81 ± .07
	.68 ± .06	.97 ± .08	.80 ± .04
	.47 ± .06	.57 ± .08	.78 ± .04
	.28 ± .15	.85 ± .21	.29 ± .08
	.69 ± .05	.44 ± .05	.77 ± .06
	1.02 ± .07	.71 ± .08	.81 ± .06
	.34 ± .05	.61 ± .05	.34 ± .04
Average	.52	.60	.54
2 mo - 6 yrs	.93 ± .40	.17 ± .25	.89 ± .12
	.65 ± .15		.41 ± .08
	.55 ± .09	.52 ± .08	.65 ± .11
	.75 ± .36	.98 ± .24	.65 ± .11
	1.22 ± .23	1.55 ± .13	.68 ± .08
	.49 ± .30	.22 ± .13	≤ .12
	.75 ± .36	.98 ± .24	
		.41 ± .04	.24 ± .08
	1.22 ± .31	1.48 ± .14	.78 ± .09
Average	.94	1.04	.58
13 yr old	.21 ± .03	.22 ± .02	.13 ± .02
	.16 ± .03		.16 ± .02
	.12 ± .05		≤ .05
			.15 ± .03
			≤ .09
			.19 ± .03
			.12 ± .009
			.46 ± .02
			.08 ± .04
			.30 ± .05
			.07 ± .08
			.11 ± .05
Average	.16	.22	.13

Table 3

Relative Distribution of Strontium-90 in the Skeleton at Different Ages

	Vertebra	Rib	Long Bone	Body Average
Adult (ave. 32)	1.8 ± .1	1.1 ± .1	.5 ± .1	1.0 ± .1
13 yr old	.7 ± .1	1.0 ± .1	.6 ± .1	.8
2 mo - 6 yrs (ave. 14)	.9 ± .1	1.0 ± .1	.6 ± .1	
Newborn (ave. 10)	.9 ± .1	1.0 ± .1	.9 ± .2	

The indicated uncertainties are the standard errors on the mean values after the bones of each individual or integrated set of individuals are normalized to whole body ash = 1.0 for the adults and rib = 1.0 for all others.

Table 4

Ratio of Sr^{90} Concentration in Particular Bone to Skeletal Average

	Present	Previous Estimate (3)	Sr^{85} (4)
Vertebra/Whole Skeleton	1.8	3.4	4.8
Rib/Whole Skeleton	1.1	1.5	1.5
Long Bone/Whole Skeleton	.5	.8	.8

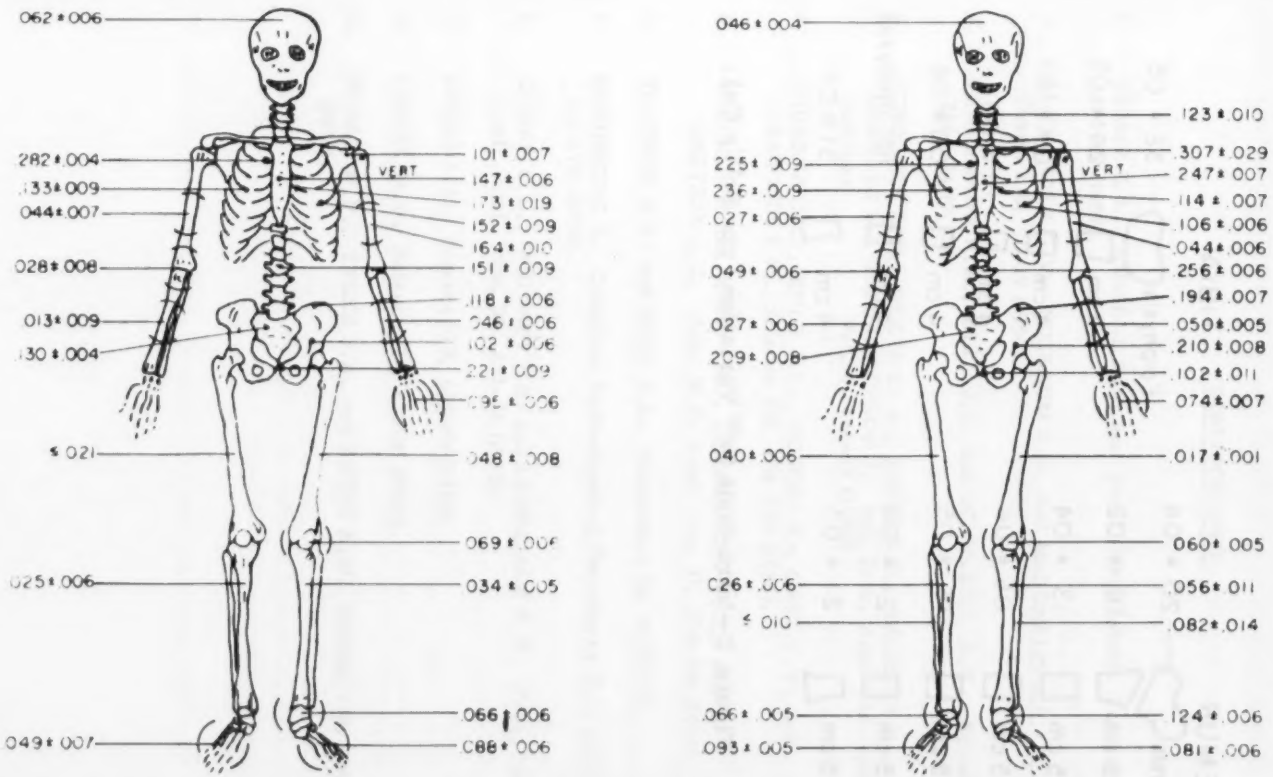

Figure 1—Sr^{90} Variation Within Skeleton ($\mu\mu c\ Sr^{90}/g\ Ca$). Each skeleton represents a composite of five which were analyzed together.

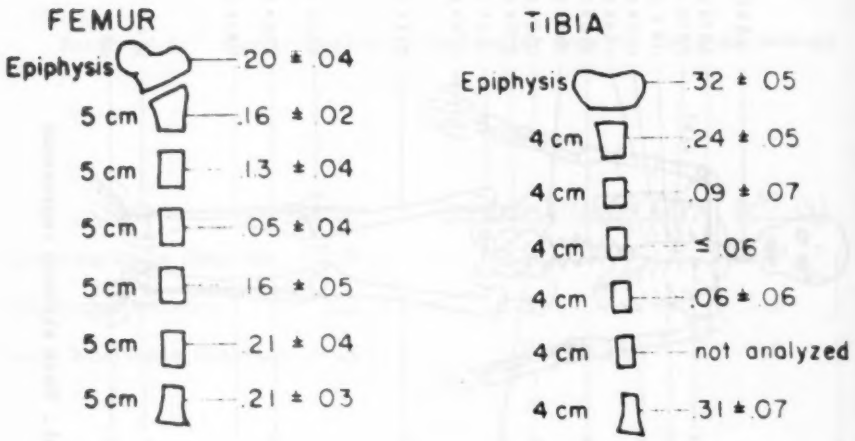

Figure 2—Intra-bone Sr^{90} Variation ($\mu\mu c$ Sr^{90}/g Ca).

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Comparative Hazards of Radioisotopes in Nuclear Fallout

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It is evident that there is much uncertainty and confusion among the public at large concerning the problem of nuclear fallout. This problem, affecting as it does the entire populace, should be adequately understood by the scientific community so that we in turn can allay to some degree the muddled public view of the problem.

The question is difficult for a number of reasons, a principal one being that damage produced by low level radiation may remain insidiously latent for years, making the procurement of quantitative data on hazard to man an enormous task. Some problems such as whether thresholds exist below which radiation is harmless are likely never to be answered unequivocably for all types of damage. In spite of these large areas of uncertainty regarding our knowledge of the biological effects of radiation, rational and satisfying evaluations of the fallout problem are possible. Reasonable comparisons can be made to the background radiation which has always been in man's environment so that estimates relative to long term human experience may be made. This report will not deal with the absolute biological effects of radiation, but rather the relative hazards the biochemical and radiochemical nature of some of the important radioisotopes present in nuclear fallout.

The most prominent long term hazards which have been considered

with regard to fallout are strontium-90, cesium-137 and carbon-14. The strontium-90 being primarily confined to bone, it is chiefly a somatic hazard whereas cesium-137 and carbon-14 are distributed throughout the body and are thus both somatic and genetic hazards. In Table 1 the annual radiation dose and the infinity radiation dose for the isotopes in our immediate environment up to mid '58 (stratosphere excluded) are given together with a subjective quality rating on each value.

Table 1

	Annual Dose (mr)	Infinity Dose (mr)
Sr^{90}	5 (Good)	200 (Fair)
Cs^{137}	3 (Good)	$\leq$ 120 (Poor)
C^{14}	0.1 (Very good)	25 (Very good)

It is noted that the current radiation dose which is computed from actual isotopic measurements, is known to reasonably good precision but difficulties arise predicting what these isotopes will do in the future. The future carbon picture can be precisely predicted since that present will continue in the biospheric reservoir just as the upper atmosphere produced carbon-14 has always done. Prognosis in the case of strontium-90 is somewhat uncertain, and our present knowledge is very poor with regard to the

future of cesium-137. A prime uncertainty lies in our insufficient understanding of the route of entry of the cesium and strontium into the plant foodstuffs from the environment, principally to what degree the uptake is foliar and to what degree it proceeds via the soil. With regard to the soil component, uncertainty exists as to how this might change in time due to migration of the isotopes in the soil and possible incorporation in unavailable forms. With regard to the gamma-emitting cesium-137 roughly half of the present radiation is external, and there are also manifold difficulties in extrapolating the external component in time. In any case, this contribution would be very variable in particular individuals depending on one's locality and mode of living.

It is readily seen that if the primary route of entry is foliar and that once the isotopes reach the soil they are largely out of the plant growth cycle, the future incorporation in man will be somewhat reduced. If the isotopes in the soil, however, remain available throughout their period of radioactive decay, the larger doses (which are those given in Table 1) would be more valid.

All are aware of the many short term isotopes concomitant with fission, perhaps principal consideration having been given to iodine-131 because of its concentration in the thyroid and strontium-89, which of course chemically follows the strontium-90. These are frequently classed

as presenting no long term hazard to the individual and correctly so. However, a problem which needs further exploration is that of the concentration of some of these short-lived isotopes in fetal life during periods of testing. For example, on an activity basis the production of strontium-89 in bomb fission is about 160 times that of strontium-90, and consequently a Sr^{89}/Sr^{90} ratio of 20 in milk during test periods is not uncommon. Since the growing fetal skeleton reflects the mother's diet rather than the mother's skeleton, the fetus may get considerably more radiation from strontium-89 than from strontium-90 under such conditions. Thus this problem deserves more intensive investigation, particularly in view of the fact that fetal life is more susceptible to radiation damage than post-embryonic life.

One further isotope which has been previously mentioned by others as a potential hazard, but not generally given serious consideration is plutonium-239. The incorporation of this isotope from soil into plants is insignificant. Furthermore, it is not an ingestion hazard since if it is taken orally, it is almost completely unabsorbed across the gastrointestinal wall. However, material in the atmosphere may enter the body through inhalation. If we take the published data for strontium-90 in the air and both strontium-90 and plutonium-239 in the soil, and assume that the ratio of the two isotopes in the air is the same as in the soil beneath it, it is computed that an individual might accumulate 0.1% of the Maximum Permissible Concentration in a few years. This compares to current strontium-90 levels in children of 1 - 2%

of the Maximum Permissible Concentration and thus the two problems may be only about an order of magnitude apart in importance. Thus, while the plutonium problem is of lesser importance at the moment, when one considers the long biological retention of plutonium, it is seen that its relative importance will increase in time for the present population. Furthermore, its 24,300 year physical half-life means that the problem will be increasingly important from generation to generation as long as it is being added to the environment.

It is concluded that nuclear fallout is not a critical hazard to mankind at the present time. It is further concluded that intensive research on many facets of the problem is necessary, including some heretofore relatively neglected areas since the problem will inevitably increase in importance as we proceed into the atomic age.

DATA ON THE STRONTIUM-90 CONTENT OF MILK AND OTHER FOODS DURING 1958

by

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This report summarizes the data available to the Health and Safety Laboratory on the Strontium-90 content of milk and other foods during 1958. As such, it is intended to supplement HASL-42, "Environmental Contamination from Weapons Tests", which summarized information through the end of 1957.

This report includes analyses performed at the Health and Safety Laboratory and under analytical contracts. In addition, certain other pertinent data from other laboratories are included for comparison. A considerable portion of the information will also be made part of other reports submitted to the Joint Committee for their consideration.

Some of the results given in this report have become available after the issuance of the last quarterly supplement to HASL-42 and will not be otherwise published until the next supplement. The policy on data publication is to include all analyses complete at the time of preparation of the quarterly supplements, or a report such as the present one.

Milk Analyses

The two earliest series of milk analyses were begun in 1954 and included powdered milk from Perry, N. Y., and liquid milk purchased in New York City. Two stations were added in 1955: Columbus, Wisc., and Mandan, N. D. The analyses of the monthly samples are shown in Fig. 1 for Perry, N. Y., Fig. 2 for New York City, Fig. 3 for Columbus, and Fig. 4 for Mandan. As a comparison, the U. S. Public Health Service analyses of 1958 samples are shown in Fig. 5.

A comparison of the yearly average levels at the four sampling stations in 1957 and 1958 is given below:

	Micromicrocuries Sr^{90} per gram calcium	
	1957	1958
Perry	3.8	5.9
New York City	4.5	7.7
Columbus	4.2	6.8
Mandan	16.4	20.5

In past years there has been a tendency for a rise in Strontium-90 content during the summer followed by a decrease about the first of the year. This is not apparent in the current samples and follows the corresponding high rate of fallout at our pot sampling stations. The major contribution to the Strontium-90 in milk comes from fallout deposited on vegetation and not from the soil. It is thus more dependent on fallout rate than total deposition.

The high concentration of Strontium-90 in the dried buttermilk from Mandan has not been satisfactorily explained in terms of soil content, rate of fallout, or vegetation content. Several experiments in the past have shown that no significant difference exists in the Strontium-90 to calcium ratio in the separate portions of milk, such as whole milk, skimmed milk, or buttermilk. While this particular material is used for animal feed, it still may be a contributor to the overall Strontium-90 level in animal bone and later to milk for the human diet.

A few miscellaneous milk samples have been reported since the issuance of HASL-42. These are listed in Table I. Considerable caution is necessary in considering data on dried milk obtained abroad. This may actually have originated in the United States or Scandinavia and have been re-packaged.

Wheat and Wheat Products

Samples of vegetation have been analyzed under the two uptake studies sponsored by the Commission, the Chicago Milkshed program and the HASL pasture site program. It must be pointed out that investigations during 1958 showed that the analytical methods in use by some laboratories were not removing all of the strontium from the ashed samples and the data from earlier times should be looked on with suspicion. These data were reported in HASL-42, however, and the 1957 data are included here in Table 2. These latter analyses are believed to be correct.

Certain of the samples analyzed were fodder, but these are more indicative of the levels to be expected in grains than are the vegetable samples.

After a discussion in March 1958 of fallout levels in Minnesota, Dr. Maurice Visscher of the University of Minnesota submitted a series of 1956, 1957 and 1958 wheat samples to HASL. The detailed results of the analyses are shown in Table 3, and they are summarized in Table 4. The latter table includes values for $\mu\mu c$ Sr^{90} per gram of calcium and per kilogram of wheat.

The mean Sr^{90} in $\mu\mu c$ per kilogram of wheat is 45 for 1956, 66 for 1957, and 44 for 1958. This rise and fall is another strong indication that the Strontium-90 content of wheat depends on the fallout rate rather than total

deposition. This general area received heavy fallout during the 1957 Nevada Tests. The 1958 crop was probably harvested before all of the heavy fallout in late 1958 could affect it.

While vegetation levels would not drop to zero in the event of cessation of tests, they would fall to a fraction of present values. The actual level would then be dependent on soil content of Strontium-90, soil composition and plant type.

A series of flours and breads purchased in New York City have been analyzed and are reported in Table 5. The general scatter of the data indicates the extreme variability of wheat products. In addition, fortification of flour and bread with calcium will tend to distort values of $\mu\mu c\ Sr^{90}$ per gram of calcium.

Comparison of the white and whole wheat bread results makes it appear that the levels are the reverse of the expected values. No evidence was found of sample reversal, but fresh samples will be tested.

A survey of wheat, bran, short and three grades of flour from single millings of wheat from the 17 major producing states is in progress. Data will not be available for at least two months, however. Such a survey will be more readily interpreted than the scattered data presently recorded.

Estimate of Dietary Components for Strontium-90 Intake

The levels of Strontium-90 in milk intended for human consumption has averaged 5 to 10 micromicrocuries per liter over the past year. Using the average U. S. consumption of 320 quarts per year (Table 6), there would be an intake of about 1500 to 3000 micromicrocuries of Strontium-90 associated with about 300 grams of calcium.

The annual consumption of wheat products is approximately 90 kilograms. Wheat flour contains about 200 mg of calcium per kilo (Table 7), so that the total intake would be 18 grams of calcium. The Minnesota wheat data indicate a 1958 mean of about 150 $\mu\mu$c/gm Ca. From the German data[1], we would expect flour from this wheat to be 50-100 $\mu\mu$c/gm Ca as a maximum. This would yield a Strontium-90 intake of about 1000 to 2000 micromicrocuries associated with the 18 grams of calcium.

The annual consumption of fruits and vegetables other than potatoes is about 125 kilograms. The Lamont data[2] show a mean of 7.6 $\mu\mu$c/gm Ca for leafy vegetables. Thus, the 125 kilograms would yield an intake of 700 micromicrocuries of Strontium-90 associated with about 100 grams of calcium. Similar calculations may be performed for potatoes.

The total estimate of U. S. yearly intake would be,

	μμc Sr^{90}	g. Ca
Milk	1500 - 3000	300
Wheat	1000 - 2000	18
Fruits and Vegetables	700	100
Potatoes	220	15

This would indicate an annual intake of about 3500 to 6000 micromicrocuries of Strontium-90 associated with about 400 grams of calcium.

British data[3] for 1957 indicate a calcium intake of 250 grams from milk, 120 grams from cereals, and 75 grams from other foods. For the levels in food during the summer of 1957, the Strontium-90 intake would have been 1300 micromicrocuries from milk, 250 from cereals, and 850 from other foods. These are comparable expect for the higher U. S. value for wheat.

Their data would indicate an annual intake of about 2400 micromicrocuries of Strontium-90 associated with about 450 grams of calcium.

The uptake of Strontium-90 has been studied and there are indications that the Sr/Ca observed ratios in going from diet to bone are lower for non-milk diets than for milk diets. In other words, Strontium-90 is absorbed

less readily from vegetable foods. However, the levels of Strontium-90 in various foods indicate that milk cannot be considered the sole important source of contamination. Considering all sources, the equilibrium content of the human skeleton in the U.S. could approach twice the level that would result from milk alone.

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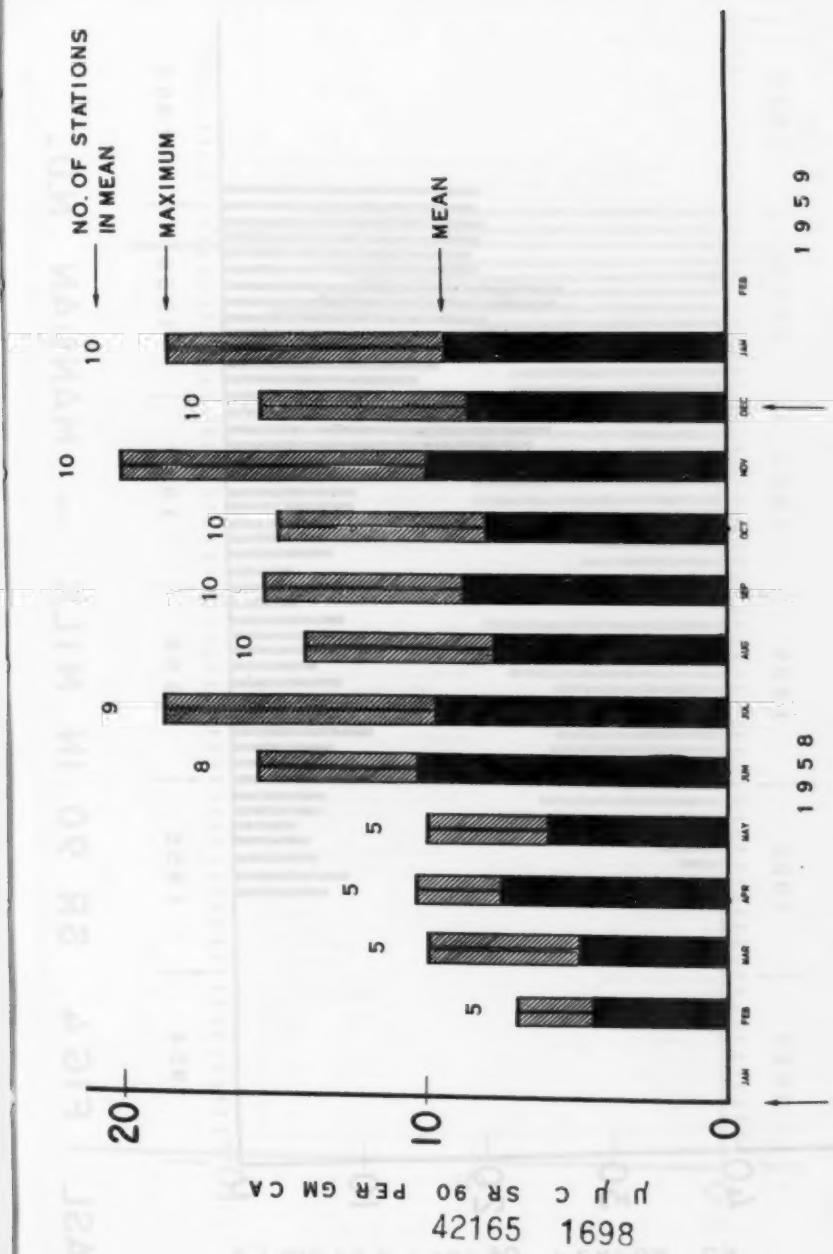
NO. OF STATIONS IN MEAN
MAXIMUM
MEAN
μμc SR 90 PER GM CA
20
10
0
JAN FEB MAR APR MAY JUN JUL AUG SEP OCT NOV DEC JAN FEB
1958
1959
5 5 5 5 8 9 10 10 10 10 10 10

HASL FIG 5 USPHS MILK ANALYSES

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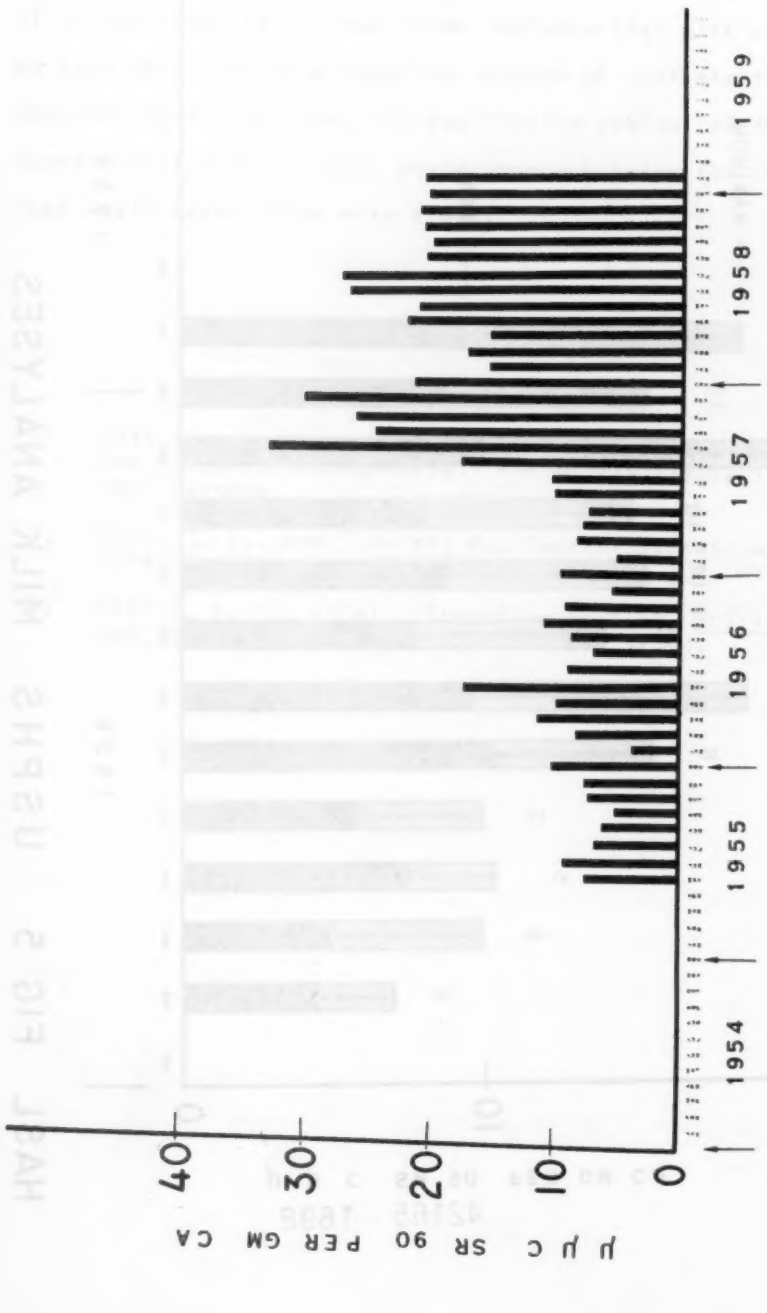
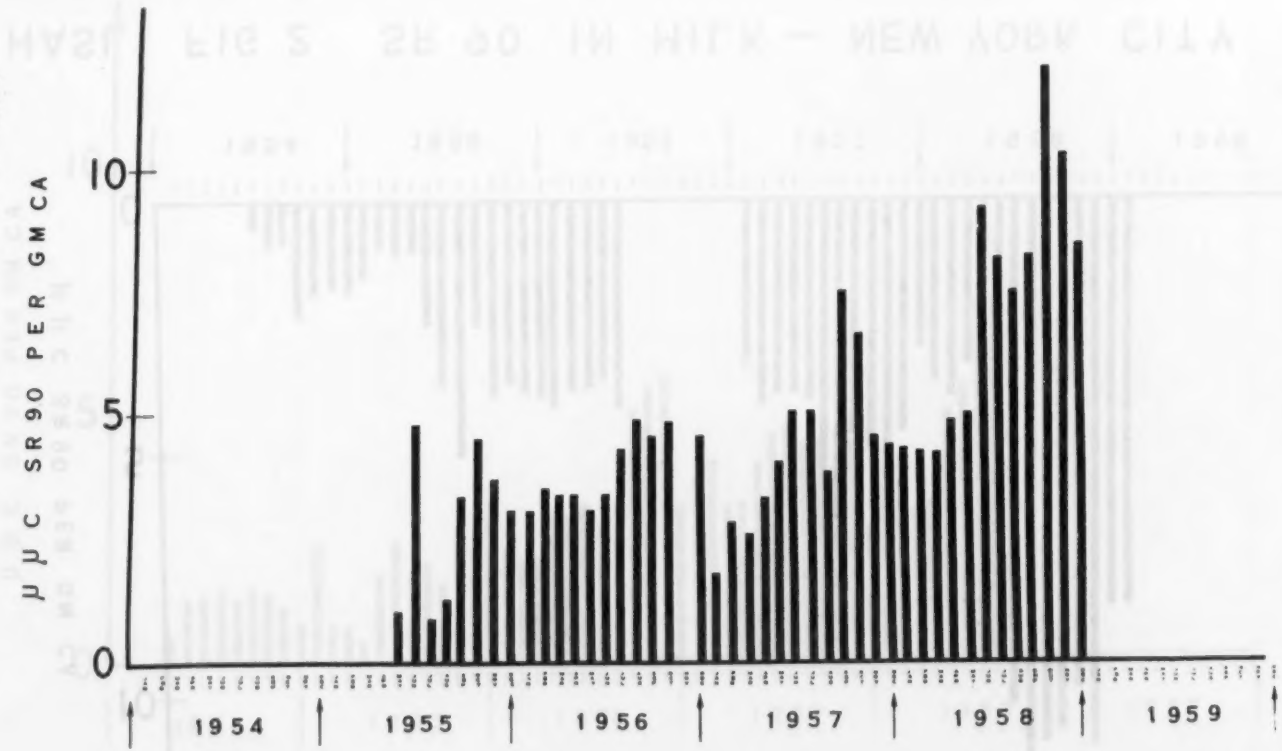


HASL FIG 4 SR 90 IN MILK — MANDAN N.D.



HASL FIG 3 SR 90 IN MILK — COLUMBUS WISC.

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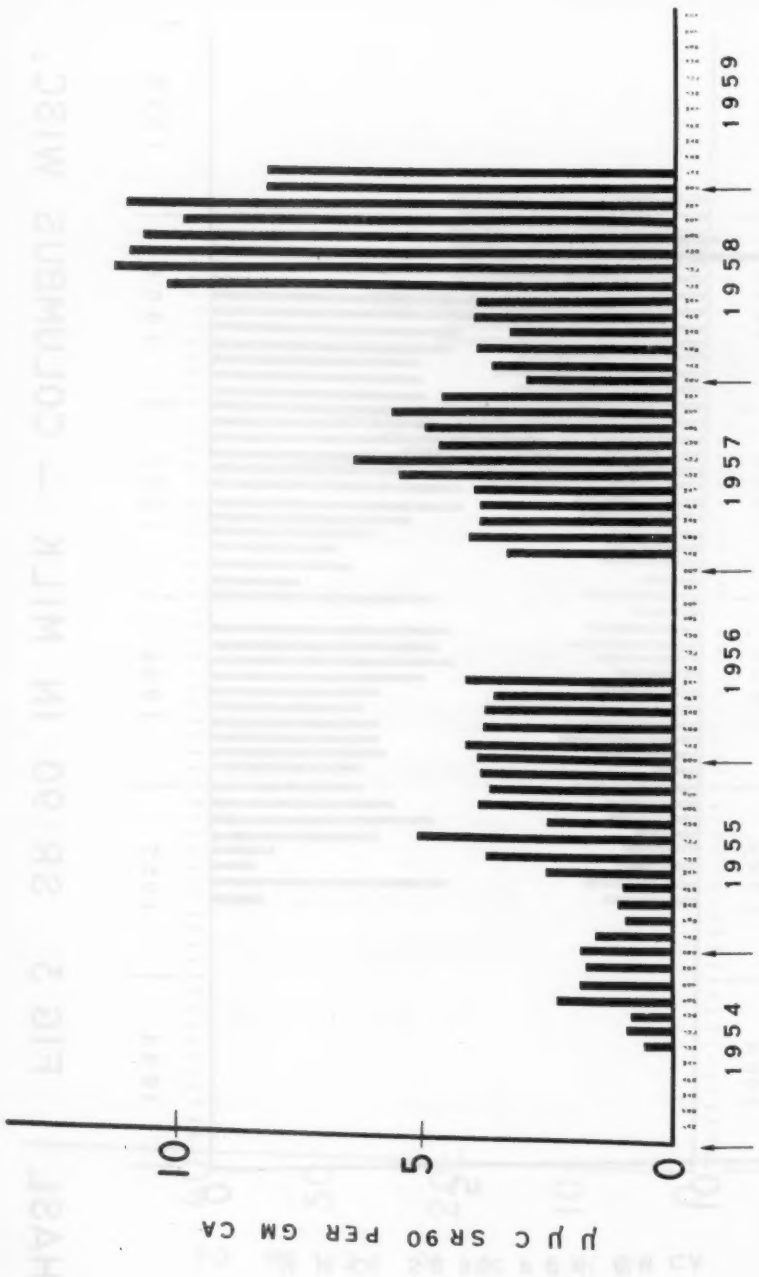

HASL FIG 2 SR 90 IN MILK — NEW YORK CITY

HASL FIG I SR 90 IN MILK — PERRY N.Y.

Table I

Strontium-90 Analyses of Milk Samples from Miscellaneous Locations

HASL #	Type	Sampling Date	Location	$\mu\mu c$ Sr^{90}/g Ca	% Ca In Ash
6361	Powdered	May '57	Japan	0.34 ± 0.15	14.9
7650	"	July '57	"	2.64 ± 0.44	14.3
8590	"	2-9-58	"	2.30 ± 0.32	15.7
8866	"	5-21-58	"	3.34 ± 0.33	15.3
9248	"	7-2-58	"	4.18 ± 0.33	13.6
7541	Liquid	10-15-57	Peru	0.09 ± 0.45	15.7
7540	"	10-20-57	"	0.32 ± 0.43	15.8
7539	"	11- 3-57	"	0.57 ± 0.60	16.0
7538	"	11-11-57	"	0.75 ± 0.40	15.2
7275	Liquid	Oct '57	Chile	4.29 ± 1.53 d/m/liter 3.50 ± 1.53 d/m/liter	
7309	Powdered	11-29-57	"	2.95 ± 0.08	14.4
7891	Liquid	Spring'58	"	0.32 ± 0.55	15.6
7892	"	"	"	2.47 ± 0.68	15.3
8402	Liquid	4-23-58	Spain	7.98 ± 0.77	14.6
8401	"	5- 5-58	"	3.98 ± 0.71	14.0
8403	"	5-12-58	"	3.06 ± 0.61	15.4
8405	"	5-13-58	"	2.16 ± 0.62	14.6
8400	"	5-29-58	"	6.88 ± 0.59	16.0
8406	"	6- 4-59	"	1.54 ± 0.45	15.8
8404	"	--	"	4.80 ± 0.96	14.6
7239*	Powdered	Nov '57	Argentina	1.66 ± 0.08	16.3
7240*	"	"	"	1.56 ± 0.07	16.5
7305*	"	"	"	1.36 ± 0.07	18.2
7057	Powdered	Sept'57	Italy	4.36 ± 0.34	17.1
7058	Condensed	"	"	5.67 ± 0.41	11.0
7577	Received as ash	Late '57	India	2.76 ± 0.08 2.68 ± 0.09	17.0 --
7256	Dry	Nov '57	Union of S.Africa	8.93 ± 0.15	15.1

* Two separate analyses by two contractor laboratories.

Table 2

1957 HASL PASTURE SURVEY

Vegetation Site	HASL#	Beltsville#	Type	Date Planted	Date Cut	% Ash of green or dry wt.	% Ca in Ash	$\mu\mu c$ Sr^{90}/g Ca	Ave. $\mu\mu c$ Sr^{90}/g Ca
Tifton and Alapaha, Ga.	6914	57986	bermuda		June '57	4.09 (green)	6.53	96.4 ± 3.0	96.4 ± 3.0
	6915	57987	bermuda		Aug. '57	2.84 (green)	6.56	181 ± 3.9	181 ± 3.9
	6916	57988	turnip greens	Oct. '57	Nov. '57	7.75 (dry)	8.05	20.0 ± 1.4	20.0 ± 1.4
	6917	57989	bl. eyed pea hulls	Aug. '57	Oct. '57	2.28 (dry)	7.57	72.7 ± 5.7	72.7 ± 5.7
	6918	57990	bl. eyed pea peas	Aug. '57	Oct. '57	6.06 (dry)	1.88	23.0 ± 5.5	23.0 ± 5.5
New Brunswick, N.J.	7221	57821	fescue, birdsfoot		Aug. '57	3.79 (green)	9.20	131 ± 3 128 ± 3	130 ± 2
	7222	57822	fescue, alfalfa, birdsfoot		8-16-57	7.37 (green)	12.30	69.0 ± 1.5	69.0 ± 1.5
Raleigh, N. C. (Expt. Farm)	8056	58257	clover, grass		July '57	7.59 (dry)	4.34	220 ± 5.2	220 ± 5.2
Ithaca, N. Y. (Cornell Univ.)	7216	57815	alfalfa - 1st cut		6-15-57	7.61 (green)	10.00 10.25	28.1 ± 1.1 29.4 ± 1.2	28.8 ± 0.9
	7217	57816	alfalfa - 2nd cut		8-15-57	7.23 (green)	16.20 16.50	34.0 ± 0.9 34.3 ± 0.9	34.0 ± 0.7
Ithaca, N. Y. (Shapley Farm)	6910	57704	cabbage	5-23-57	7-24-57	0.50 (fresh)	3.87	22.0 ± 2.4 24.4 ± 2.0	23.2 ± 1.7
	6911	57705	beans	6-6-57	7-29-57	0.73 (fresh)	11.3	5.51 ± 0.44 6.47 ± 1.06	5.99 ± 0.68
Logan, Utah (Expt. Farm)	7208	57816	clover, grass		10-11-57	14.19 (green)	8.15 8.20	160 ± 3 156 ± 3	158 ± 1
Sweden, N. Y. (Griffin Farm)	6908	57278	alfalfa		7-1-56	9.09 (dry)	10.3	23.2 ± 1.5 19.1 ± 2.0	21.2 ± 2.9
	8581	58140	alfalfa		late June '57	8.81 (dry)	14.20	45.2 ± 1.5	45.2 ± 1.5
Brawley, Calif (Expt. Station)	6201	57220	pea pods		2-26-57	0.88 (wet)	5.9	<0.3	<0.3
	6906	57218	broccoli		2-26-57	1.40 (wet)	9.5	1.1 ± 0.9 1.2 ± 0.9	1.2 ± 0.1
	6907	57219	alfalfa		2-26-57	1.63 (wet)	8.9	8.3 ± 0.6 5.9 ± 0.6	7.1 ± 1.7
	6912	57727	cantaloupe rinds and flesh		†	10.55 (dry)‡	9.4‡	<0.33 <0.32	<0.33
	6913	57728	cantaloupe seeds		†	16.80 (dry)	12.2‡	<0.24 <0.25	<0.24

Table 3

Wheat Samples Sent to HASL by Dr. Maurice B. Visscher
of the University of Minnesota Medical School

Results of Strontium 90 and Calcium Analyses

Ref.#	Location	Varieties (Spring Wheat)	Year	% Ash of Wt. Rec'd.	d/m Sr^{90}/g ash	% Ca in ash	mμc Sr^{90}/g Ca
1	Rosemount	Henry, Selkirk, Mida, Rushmore, Lee, Thatcher	1956	1.95	8.83 ± 0.35	2.44	163 ± 7
2*	Southwest	Lee, Mida, Thatcher, Henry, Selkirk, Rushmore	1956	2.07	7.06 ± 0.34 8.20 ± 0.47	2.02 2.04	157 ± 7 181 ± 10
3	Grand Rapids	Lee, Mida, Thatcher, Henry, Selkirk, Rushmore	1956	2.21	3.66 ± 0.28	1.87	88.2 ± 7.8
4	Morris	Lee, Mida, Rushmore, Thatcher, Selkirk, Henry	1956	2.00	3.09 ± 0.27	1.70	81.8 ± 7.1
5	Duluth	Lee, Henry, Mida, Rushmore, Selkirk, Conley	1956	2.27	3.74 ± 0.28	2.04	82.4 ± 6.4
6*	Crookston	Henry, Rushmore, Lee, Mida, Thatcher	1956	1.55	2.33 ± 0.35 2.42 ± 0.29	1.47 1.42	71.4 ± 10.2 76.8 ± 9.2
7	Wadena	Mida, Lee, Rushmore, Thatcher, Selkirk, Henry	1956	2.07	4.91 ± 0.38	2.36	93.6 ± 7.2
8	N. Minnesota	Lee, Selkirk, Henry, Russel (Lee x Kenya Farmer)	1957	1.91	5.43 ± 0.29	1.98	124 ± 7
9	Rosemount	Selkirk, Lee, Thatcher	1957	2.26	6.36 ± 0.42	1.91	150 ± 10
10*	Morris	Thatcher	1957	2.21	8.11 ± 0.36 7.85 ± 0.49	1.88 1.73	194 ± 8 205 ± 13
11	Grand Rapids	Lee, Selkirk, Russel (Lee x Kenya Farmer)	1957	2.06	6.54 ± 0.42	1.98	107 ± 12
12*	Crookston	Thatcher, Lee, Selkirk, Conley	1957	3.64	6.78 ± 0.42 6.95 ± 0.33	0.50 0.52	610 ± 38 602 ± 29
13	Southwest	Thatcher, Selkirk, Lee, Russel (Lee x Kenya Farmer)	1957	2.12	5.38 ± 0.31	1.66	166 ± 8
14	Wadena	Thatcher, Selkirk, Conley (Lee x Kenya Farmer)	1957	2.15	4.80 ± 0.37	2.06	105 ± 8
15*	Duluth	Selkirk, Russel	1958	1.84	4.28 ± 0.31 4.09 ± 0.28	1.60 1.82	121 ± 9 101 ± 7
16	Grand Rapids	Varieties not known	1958	1.77	7.52 ± 0.35	1.84	184 ± 9
17	Rosemount	Selkirk, Conley	1958	1.35	5.88 ± 0.32	2.13	124 ± 7
18	Unknown	Russel RO 58	1958	1.70	6.92 ± 0.32	1.86	169 ± 8
19	Southwest	Conley, Selkirk	1958	1.84	4.48 ± 0.29	1.06	191 ± 12
20	Unknown	Conley, WF 58	1958	2.05	5.78 ± 0.34	1.65	158 ± 9
21	Wacoa	Selkirk	1958	1.84	4.89 ± 0.32	1.70	189 ± 8
22*	Crookston	Conley	1958	2.00	4.58 ± 0.29 4.84 ± 0.31	0.97 1.05	212 ± 13 214 ± 14
23	Int'l. Falls	Unknown	1958	1.98	4.59 ± 0.30 4.78 ± 0.31	1.73 1.76	120 ± 8 122 ± 8

* run as blind duplicates.

Error term is one standard deviation due to counting.

Table 4

Summary of Minnesota Wheat Results

Location	μμc Sr^{90}/kg Ca			μμc Sr^{90}/kg Wheat		
	1956	1957	1958	1956	1957	1958
Rosemount	163	150	124	76	65	36
Southwest	169	146	191	71	51	37
Grand Rapids	88	187	184	36	61	60
Morris	82	200	-	28	80	-
Duluth	82	-	111	38	-	35
Crookston	74	606	213	17	112	42
Waseca	94	105	129	46	47	40
N. Minnesota	-	124	-	-	47	-
International Falls	-	-	121	-	-	42
Unknown	-	-	169, 158	-	-	54, 54

Table 5

Strontium-90 and Calcium in Flour and Bread Purchased in New York City on February 19, 1959

	Item	Description	HASL#	% ash of original purchased material	d/m Sr^{90} / g ash	% Ca in ash	μμc Sr^{90} / g Ca	μμc Sr^{90} / kg original material
Flour								
white	1.	4, 5 lb. pkgs. - Sunnyfield Family - Enriched, Bleached A and P. N.Y., N.Y.	9517	0.434	6.01 ± 0.29	4.40	61.5 ± 3.0	11.7 ± 0.6
	2.	4, 5 lb. pkgs. - Pillsbury Best XXXX - Enriched, Bleached Pillsbury Co., Minneapolis, Minn.	9520	0.666	6.81 ± 0.26	10.70	28.7 ± 1.1	20.4 ± 0.8
	3.	4, 5 lb. pkgs. - Gold Medal-All Purpose Bleached General Mills, Inc. -Minneapolis, Minn.	9521	0.386	5.69 ± 0.27	3.99	64.2 ± 3.1	9.9 ± 0.5
	4.	2, 10 lb. pkgs. - Hackers - All Purpose Enriched, Unbleached	9518	0.476	9.89 ± 0.39	3.73	119 ± 5	21.2 ± 0.8
whole wheat		Standard Milling Co., Kansas City, Mo.						
	5.	5, 2 lb. boxes Elams Stone Ground 100% whole wheat flour Elam Mills, Inc. - Broadview, Ill.	9519	1.64	26.79 ± 0.56	2.02	598 ± 12	198 ± 4
Bread								
	6.	12, 1 lb. loaves Jane Parker white bread Jane Parker Bakers - A & P, N.Y., N.Y.	9523* 9631	3.33	8.38 ± 0.34 8.58 ± 0.32	5.24 5.21	72.0 ± 2.9 74.1 ± 2.7	126 ± 5 129 ± 5
	7.	12, 1 lb. loaves Jane Parker whole wheat Jane Parker Bakers - A & P, N.Y., N.Y. ("made with 100% whole wheat flour")	9522* 9632	2.2	0.97 ± 0.20 0.96 ± 0.20	8.75 8.89	5.0 ± 1.0 4.9 ± 1.0	9.6 ± 2.0 9.5 ± 2.0

* Run as blind duplicates.

Table 6

Dietary Values for Man

Amounts in grams (unless otherwise noted) per person per week

Item	Average	Maximum
Milk and milk products	6.2 quarts	7.8 quarts
Flour and flour equivalent	1850	2070
Fats and oils	580	900
Eggs	0.82 doz.	1.04 doz.
Sweets, sugar	830	1110
Fresh fruits	1070	5800*
Fresh vegetables		
Potatoes	1990	2220
Leafy vegetables	155*	920*
Other vegetables	730*	2800*
Canned fruits and vegetables	1500	1750
Frozen fruits and vegetables	60	150
Dried fruits and vegetables	100	190
Meat and Fish		
Beef	440	620
Pork	750	800
Other	220	270
Poultry	250	300
Fish	200	300

* Children only

"Basic Anatomical, Dietary, and Physiological Data for Radiological Calculations", AEC Research and Development Report HW-41638

* Run as blind duplicates.

Table 7

WATER AND CALCIUM CONTENT OF WHEAT AND WHEAT PRODUCTS

From: Composition of Foods--Raw, Processed, Prepared -- by Bernice K. Watt and Annabel L. Merrill U. S. Department of Agriculture Handbook No. 8, June 1950.

From: Table 1 -- Composition of Foods, 100 grams, edible portion

No.	Food and Description	% Water*	mg Ca
	Wheat, whole grain:		
717	Hard red spring	13.0	36
718	Hard red winter	12.5	46
719	Soft red winter	14.0	42
720	White	11.5	36
721	Durum	13.0	37
	Wheat flours:		
722	Whole (from hard wheats)	12	41
723	80% extraction (from hard wheats)	12	24
724	Straight, hard wheat	12	20
725	Straight, soft wheat	12	20
	Patent flours:		
728	All-purpose or family	12	16
730	Bread	12	16
732	Cake or pastry	12	17
	Wheat products:		
115	Bran (breakfast cereal)	2.6	94
116	Bran flakes (40% bran)	3.6	61
733	Flakes	3.8	46
735	Germ	11.0	84
736	Puffed	3.8	46
738	Rolled, dry	10.1	36
740	Shredded, plain	5.6	47
742	Whole meal, dry	8.2	46

* Based on the product as it reaches the mill prior to tempering.

UNITED STATES
ATOMIC ENERGY COMMISSION
WASHINGTON 25, D. C.

August 21, 1959.

Mr. James T. Ramey
Executive Director
Joint Committee on Atomic Energy
Congress of the United States

Dear Mr. Ramey:

During the May hearings on "Fallout from Nuclear Weapons Tests," data were presented giving strontium-90 levels in bread obtained by the Health and Safety Laboratory from New York City markets. A suggestion was made that during the laboratory work two samples labeled "white bread" and "whole wheat bread" might have been switched so that the labels were reversed.

Since then new samples have been obtained and analyzed. The results tend to confirm the suspicion that the original samples were indeed labeled in reverse. An absolute confirmation is, of course, impossible because nothing of the original samples is left upon which to do a re-analysis.

The attached note is to announce the error to the press and includes a complete tabulation of the old and new data and of some further new data on composite samples of bread.

Sincerely yours,

Charles W. Shilling, M. D.
Acting Director
Division of Biology and Medicine

NOTE TO EDITORS AND CORRESPONDENTS:

Last May when data on the strontium-90 content of bread samples were presented to the Congressional Joint Committee on Atomic Energy during its fallout hearings, representatives of the Commission's Health and Safety Laboratory indicated there was doubt about the reliability of two of the samples. They said then that a mislabeling error might have occurred and that new samples of breads of the same type would be analyzed to confirm or deny the suspicion.

The laboratory has completed analysis of the new samples and reported the data which indicate that at some point during the chemical analysis of the original bread samples, white bread ash had been labeled as whole wheat and the other way around. The attached sheet gives the original, erroneous results followed by the correcting data derived from the second set of samples.

In addition, the attached sheet reports the results of analyses made of composite samples of five different brands of white bread and five of whole wheat. These breads were purchased in New York City groceries, off the shelf, during the month of May, 1959.

Attachment

RESULTS OF BREAD ANALYSIS - NEW YORK CITY SAMPLES

	% Ash	% Ca In Ash	uuc Sr^{90} / g Ca	uuc Sr^{90} / kg Bread
Previously Reported - Sampled February 1959 (erroneously labeled)				
White Bread	3.33	5.24	72.0 ± 2.9	126 ±5
		5.21	74.1 ± 2.7	129 ±5
Whole Wheat Bread	2.2	8.75	5.0 ± 1.0	9.6 ±2.0
		8.89	4.9 ± 1.0	9.5 ±2.0
Repeat - Sampled May 1959 (correctly labeled)				
White Bread	2.55	7.70	5.9 ± 1.1	11.6 ± 2.2
Whole Wheat Bread	6.85	1.42	60.6 ± 6.5	58.9 ± 6.3
Composite Samples - May 1959				
White Bread	2.31	5.30	10.2 ± 1.6	12.5 ± 2.0
Whole Wheat Bread	2.57	3.34	42.8 ± 2.8	36.8 ± 2.5

(Thereupon at 12:35 p.m. a recess was taken until 2 p.m. the same day.)

AFTERNOON SESSION

Representative HOLIFIELD. The committee will be in order.

Our subject this afternoon will be fallout mechanisms, uptake from soils by plants, and foliar deposition.

Dr. Robert Reitemeier of the Atomic Energy Commission and Agricultural Research Service, Beltsville, Md., will be our first witness.

STATEMENT OF DR. ROBERT REITEMEIER,[1] AEC AND AGRICULTURAL RESEARCH SERVICE

Dr. REITEMEIER. Mr. Chairman and members of the committee, my statement will be substantially the same as my prepared statement, which I assume you have, except for the definition of some terms and perhaps enlarged discussion of some of the results.

The fission products contained in fallout particles enter the food chain of man primarily through plants and soils. Some particles are deposited initially on the plants, the remainder on the soil. It is proposed to discuss here mechanisms of intake of fission products by plants, reactions with soils, and land reclamation and decontamination measures.

A fraction of the particles deposited on food crops will be ingested by animals and humans if surface decontamination measures are not employed.

This refers to the decontamination of the surfaces of the food crops themselves.

Rain and wind will move some of the particles from plant to soil. The extent of this movement will depend on the shape of the plant and the characteristics of the plant surfaces. Actually, the meterological conditions occurring during the fallout affect the initial distribution of particles between plant and soil.

Fallout during a moderate or heavy rain, for example, will be expected to be deposited on the soil to a greater extent than is dry fallout. The particles remaining on the plants are subject to dissolution, that is, dissolving in water, to a degree governed by the solubility characteristics of the particles—such as size and chemical composition—and the weather conditions occurring after deposition.

Light rains, mist, fog, and dew increase the opportunity for dissolution. Some of the fission products made soluble by this action are absorbed into the plant, and a fraction of these may be translocated or more simply, moved, to other parts of the plant.

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[1] Since November 1956, soil scientist, Biology Branch, Division of Biology and Medicine, U.S. Atomic Energy Commission, Washington, D.C., and Soil and Water Conservation Research Division, Agricultural Research Service, U.S. Department of Agriculture, Beltsville, Md. Born at Logansport, Ind., on Jan. 24, 1912. Graduated from Logansport High School, 1929. Bachelor of Science (chemistry), Purdue University, 1933. Master of arts (chemistry), University of Missouri, 1934. Doctor of philosophy (agricultural chemistry and soils), University of Arizona, 1938. Shell Oil Co., Wilmington, Calif., technologist, 1938–39. U.S. Salinity Laboratory, Riverside, Calif., chemist, 1939–45. U.S. Department of Agriculture, Beltsville, Md., soil scientist, 1945–53 and 1954–56. Foreign Operations Administration, Ankara, Turkey, soils technician, 1953–54. Member of Soil Science Society of America, American Society of Agronomy, International Society of Soil Science, Washington Academy of Sciences, New York Academy of Sciences, Phi Lambda Upsilon, Phi Kappa Phi, and Sigma XI. Soils editor, Biological Abstracts, 1946–53. Associate editor, Atomic Energy and Agriculture, American Association for Advancement of Science, 1957. (Submitted by witness.)

Subsequent rains may leach some of the absorbed radioisotopes from the plant onto the soil. These, being already in a soluble form, can participate immediately in the reactions with the soil. Those isotopes in particles deposited on the soil must first be liberated by solution of the particles by water, acids, or exchange reactions.

The last would involve the replacement of atoms already held on soil particles by atoms from the fallout particles.

The mobilized fission products in the soil are then free, to varying degrees, to move through the soil, and to enter plant roots and be transported to other plant parts.

Some of our present knowledge has been obtained under actual fallout situations, the rest with completely soluble sources of radioisotopes. In the latter case, it is presumed that maximal biological availability of the isotope is manifested.

Many results of experiments and observations on soils and plants at the current fallout levels should be applicable to problems involving much higher levels, and, conversely results obtained with tracer applications—that is, at much higher levels—are applicable to present conditions.

The first section I would like to discuss in detail is foliar absorption.

Representative HOLIFIELD. By foliar absorption, you mean by the leaves and the external surfaces of the plant above the ground?

Dr. REITEMEIER. This would include the stems in a general sense. It would include both the leaves and the stems and other aboveground parts; yes, sir.

The extent of absorption of a radioisotope by aboveground parts of plants is usually determined by spraying or painting a particular part, or the entire aboveground portion, with a solution containing a known amount of the isotope, and after washing or skinning, measuring the residual amount of radioactivity by Geiger counting or radioautography.

Translocation of the isotope following absorption is detected in a similar manner, by treating only one plant part and later measuring the radioactivity in various other parts. Moderate absorption of isotopes of strontium, barium, ruthenium, and rubidium by the foliage of bean, beet, and tomato plants has been demonstrated (19).

The translocation of the absorbed strontium to other parts, however, was very slight and negligible in the direction of the roots.

Representative HOLIFIELD. By this you mean the outside deposit was greater than inside the plant through capillary action in absorbing it from the soil?

Dr. REITEMEIER. We are speaking now entirely of the absorption into the leaves and stems directly.

Representative HOLIFIELD. From the aboveground part of the plant?

Dr. REITEMEIER. Yes; in the case of strontium, we are saying that a moderate amount of the strontium which is deposited on the leaves is absorbed into the leaves but does not move very readily out of the leaf, especially downward, in the direction of the roots.

Representative HOLIFIELD. You will take up the other factor, the factor of absorption by the roots from the soil?

Dr. REITEMEIER. In another section; yes.

Absorption of strontium into tomato fruits from applications painted on their surfaces also occurred, as shown by radioautographs. The moderate leaching of root absorbed strontium 90 from leaves by water sprays also has been established.

This implies that there is a two-way movement depending on the moisture situation. That is, the rain may leach some of the strontium and some of the other isotopes from the leaf down onto the ground.

Following applications of solutions containing strontium 90, ruthenium 106, cesium 137 and cerium 144, to some leaves of sunflower and kidney bean plants, the movement of cesium to the other leaves and other aboveground parts was 100 times as great as that of the other isotopes.

Middleton found that radiostrontium sprayed on the foliage of wheat, potato, bean, cabbage, sugar beet, and swede plants—swedes being a fodder beet—moved to untreated parts to only a limited extent, while radiocesium moved to all parts of the plant.

The spraying of radiostrontium on immature heads of wheat effected a much higher strontium content of the grain than did spraying of plants before emergence of the heads.

The next mechanism is called stem-base absorption.

The relatively high strontium 90 content of the foliage of permanent grass pastures in the United Kingdom has not be explainable by soil uptake on foliar absorption. The concentration in the grass of some pastures appears to be 10 times as high as would be expected by way of the soil.

A further mechanism, called stem-base absorption, has been proposed to explain this situation. Established grass pastures in humid climates develop a root mat consisting of upper roots, the basal portion of the stems, and other organic matter. This mat lies just above the soil surface.

According to the proposed theory, it intercepts strontium 90 washed from the surfaces of the grass leaves by rain, and detains it long enough for a considerable fraction to be absorbed into the roots or stem and transported into the leaves. Soil reactions which would reduce the availability of strontium to roots, such as adsorption—which is the attachment of ions to particles in the soil—and fixation, which is defined as a binding in the soil in a nonavailable form, and dilution with the soil calcium are thereby avoided.

A consequence, in addition to the incerased efficiency of uptake by the grass, is that the strontium 90 content of the plant depends primarily on the current rate of deposition of fallout and not on the cumulative amount of strontium 90 in the soil.

I might add here that this statement is applicable, of course, to foliar absorption, also.

The stem-base mechanism would be expected to be less important in other areas. Pastures in the United States, for instance, are replanted relatively frequently and the vegetation usually is a legume or a grass and legume mixture.

Representative HOLIFIELD. The importance of this, from a practical standpoint, Doctor, is that we have X amount of deposit of strontium 90 on the soil. This does not necessarily mean that you would have that amount in the plant. The plant would only take up a percentage of that. Is that true?

Dr. REITEMEIER. In the case where there is a curent deposition of fallout, this says that a certain fraction will not come from the soil but will be absorbed directly by these roots before it touches the soil.

Representative HOLIFIELD. Carrying on through on that, let us asume it is a tomato plant. Part of the foliar uptake and part of the root uptake would be retained in the body of the plant itself and not deposited in the edible portion which people are concerned with. Is that true?

Dr. REITEMEIER. According to the statements I made in the previous section, there may well be some absorption into the tomato fruit of strontium deposited on the skin of the fruit. This will depend, probably, partly on the moisture condition.

Representative HOLIFIELD. I am glad to have that answer but it does not get quite to the point. The point I was trying to establish, if it can be established, was that if X amount is deposited on the outside of the plant and is absorbed by the capillary action of the plant from the roots, the residual deposit in the fruit itself, the peach or the apple or the tomato, would be only a percent of the total which has been deposited on the outside of the foliage and in the ground with the uptake from the root.

Dr. REITEMEIER. There will be only a fraction in each case—a fraction of the amount deposited on the plant—that will go into the fruit and a fraction of the amount that was deposited on the soil will go into the fruit. Only a small fraction in each case.

Representative HOLIFIELD. My next question would be, have you been able, through your experiments, to reduce to percentage that fraction which would go into the edible part of the plant?

Dr. REITEMEIER. In the case of tomato plants?

Representative HOLIFIELD. Let us take tomatoes and let us take a root plant like potatoes.

Dr. REITEMEIER. This might be of the order of 1 percent of the total amount deposited on the plant. If it were deposited at the time there was fruit I would suppose that the order of magnitude would be about 1 percent that might enter the fruit. This would probably be the maximum.

Representative HOLIFIELD. This would be different in the case of the potato because it would not be exposed to foliar uptake, but it would function in the root uptake area, I assume, from the soil only.

Dr. REITEMEIER. This is true of the potato tubers. There is a saving factor in this case of potato because potatoes normally have a very low calcium content. The strontium uptake by the potato tubers from the soil will depend primarily on the calcium content of those tubers, and this is low.

So we normally expect a relatively low contamination of potato tubers from soil uptake of strontium as compared to other crops.

Representative HOLIFIELD. Now, we are going to call on Senator Aiken, who is undoubtedly the most outstanding practical farmer in the Senate.

Senator AIKEN. I notice that you say, Doctor, that the application of fallout, strontium, cesium, or whatever it is, to the foliage would result in the absorption of some of it by the tomato which grows on the tomato vine. Did you undertake to find out if that same treat-

ment applied to the soil would come up through the stem into the tomato?

Dr. REITEMEIER. We probably do not have an experiment which would answer this particular question.

Senator AIKEN. Also, the fact that the potato which is the same family as the tomato does not absorb the strontium. Is that due to the resistance of the potato or the fact that it has a lower calcium content, although I do not know what the calcium content is, or to the fact that the strontium will not go down the stem?

Dr. REITEMEIER. To both. As I said, the movement downward from the leaves to the tuber will be small. Also, the intake by the tuber and the other roots from the soil will be low because this part of the potato plant has a low calcium content and the strontium does follow the calcium to a great extent.

Senator AIKEN. I notice, too, your reference to well-limed soils, that the plants growing on well-limed soils had a greater resistance or else there was a greater absorption of the strontium in the soil. You refer all the time to calcium.

Suppose that you used magnesium instead of calcium. What is the affinity between magnesium and these various fallout elements?

Dr. REITEMEIER. There appears to be some correlation between the soil magnesium levels and strontium contents of plants. The correlation usually is not as good as between strontium and calcium. Some experiments have shown some effect of magnesium on that relationship.

Senator AIKEN. It is the practice of good farmers to use calcium one time and then magnesium lime a few years later.

Dr. REITEMEIER. This is usually beneficial, yes.

Senator AIKEN. Another thing that interested me is the apparent affinity of nitrates for the fallout material. I believe you refer to it here somewhere.

Dr. REITEMEIER. In a later section I do make the general statement that nitrogen fertilizers, in general, have seemed to increase the uptake of fission products.

Senator AIKEN. Is that due to the nitrogen itself or what?

Dr. REITEMEIER. This may be a rather complex result. The usual result of adding nitrogen where it is deficient, of course, is to increase the overall size of the plant. This increased uptake may result partly from a better growth and larger size of the plant.

Senator AIKEN. The nitrogen would make broader channels for the strontium to get from the soil to get up into the plant.

Dr. REITEMEIER. You might put it that way.

Senator AIKEN. It would give a broader highway, really.

Dr. REITEMEIER. Yes.

Senator AIKEN. Was there any difference between sodium nitrate or ammonium nitrate applied to the soil and nitrate which was collected from the air by leguminous plants? If the affinity of strontium was for nitrogen itself, it seems to me it would hook onto that on the way down.

Dr. REITEMEIER. I assume that there is no such direct connection between strontium and nitrogen as your last statement would indicate. I think it is an indirect effect, primarily.

Senator AIKEN. The effect of the nitrogen would be that it makes the plant thriftier and, therefore, has broader access for these fallout elements to get from the soil up to the top of the plant.

Dr. REITEMEIER. This would be my opinion.

Senator AIKEN. Rather than any affinity between the nitrogen and the strontium?

Dr. REITEMEIER. In general, this would be my impression.

Senator AIKEN. Have you some cases where damage to human beings has been caused by absorption of strontium through milk or wheat or other commodities?

Dr. REITEMEIER. I personally do not know of any single case of any human being being damaged by strontium in the sense that you indicate here, which I think is in support of the testimony which has been given to you in the last 2 days.

Senator AIKEN. You know of no instances of actual damage being done, then?

Dr. REITEMEIER. I know of none.

Senator AIKEN. However, you do have instances of actual damage being done by lead poisoning?

Dr. REITEMEIER. Yes.

Senator AIKEN. Sodium selenate, which is very violent as a poison.

Dr. REITEMEIER. Yes.

Senator AIKEN. And other chemicals of that kind. So we might assume, then, that damage to health through drinking milk or eating tomatoes or beet greens and other things is far less from fallout at the present time than from other causes from eating the same foods.

Dr. REITEMEIER. About all I think I could say is to repeat that, as far as I know, there is no known case of any detriment to human beings from the amounts of strontium derived from food up to now.

Senator AIKEN. These broad tests are being made with milk and wheat because they are common products universally used in good supply all over the country of a more or less uniform makeup.

Dr. REITEMEIER. This is one of the important reasons.

Senator AIKEN. And because they do contain calcium.

Dr. REITEMEIER. This is especially true in the case of milk.

Senator AIKEN. That is the reason these commodities have been chosen rather than the proability that they will be found to be the greatest malefactors?

Dr. REITEMEIER. I believe the evidence you heard this morning indicates that quite a great variety of foods are being analyzed. I would not say it was restricted entirely to what we would call the major foods or food crops. Of course, a large part of the attention is being given to milk.

Senator AIKEN. We will say if a plant absorbs 100 units of strontium—I don't know what that would be, but it would be 100 units—that then the cow would absorb only a percentage of that, the milk would have only a percentage of what the cow absorbed and the human would only absorb the percentage that was found in the milk. Is that correct?

Dr. REITEMEIER. There would be two ways of looking at this. One frequent way, and one, I believe, that you are familiar with, is expressing this in strontium to calcium ratios. Of course, Dr. Comar later this afternoon is expecting to go into this much more than I

am competent to, and I feel that you might get more precise and authoritative answers from him on that particular question.

Representative HOLIFIELD. There is a very important question. It involves the element of discrimination both by the plant and by the animal and by the human being. We will go into that later on with the biologists. I hope that there will be some illustrations given to us tracing a certain amount.

I realize it can't be done exactly and accurately, but it can be somewhere in the ball park. I think it is very important that we have that.

Senator AIKEN. It seems if there should be progressive or proportionate rejections—I do not know what term would be used here—as it goes from soil to plant to cow to milk to human, possibly some witness may have made some tests on that.

Dr. REITEMEIER. Mr. Chairman, I do intend in a later section to discuss briefly this matter of discrimination in plants, and Dr. Comar will go beyond that into animals and humans.

Senator AIKEN. The reason I ask these questions is that so many people are getting so alarmed. I do not want them to get so alarmed that they all get emaciated.

Representative HOLIFIELD. You may proceed.

Dr. REITEMEIER. The next section is called soil-plant factors.

The customary problem in soil fertility is the increase of uptake of chemical elements by plants. The main goal of research on radioactive contaminants, however, is the reduction of uptake of elements. The absorption of a radioisotope from soil by plant roots requires the simultaneous occurrence of three conditions.

The isotope must be in the active root zone of the plant, its binding to soil particles must not be too tight, and the plant must have a mechanism for the absorption of the particular element.

Most fission products of greatest importance in the food chain, for example, strontium, cesium, barium, ruthenium, and rare earths, occur in soils and plants as cations, positively charged atoms. They become attached to soil particles, to varying degrees, by such chemical reactions as adsorption, cation exchange, that is, the replacement of an attached atom by another atom, and chemical preciptation.

Of this group of elements, strontium is consistently found to be absorbed the most. The others are absorbed only to a relatively slight extent.

Strontium behaves similarly to the chemically related nutrient element and important exchangeable cation, calcium. Where both the exchangeable strontium and calcium are uniformly distributed through the entire root zone, as in most pot experiments, the ratio of the two elements in the shoots is approximately the same as on the exchange complex of the soil, that is, discrimination between the two is slight.

The exchange complex is a mixture of colloidal clay and organic matter in the soil that holds cations on their surfaces.

However, in the field, the strontium 90 and calcium seldom are uniformly distributed in the root zone of any crop. Further, the root habits of crops vary with soil conditions. Where the strontium 90 is located near the soil surface, shallow rooted crops, as many grasses, will have a higher strontium to calcium ratio than deeper rooted crops.

Where it has been moved to a lower depth, as by plowing, the ratio in shallow rooted crops will be reduced.

Dilution of the strontium 90 by natural or applied available calcium often reduces the strontium 90 content of the crop. The term "available" here means generally in a form which can supply the needs of plants.

In this country differences among soils are more striking than effects of liming of a particular acid soil. The range of natural exchangeable calcium levels is wide, while even in very acid soils the exchangeable calcium level can usually be raised only severalfold by lime applications.

Additions of lime in excess of the cation exchange capacity are of no benefit. The cation exchange capacity is the total ability of a soil to hold exchangeable atoms. It is currently recommended that lime be applied only in amounts expected to provide better crops.

Fertilizer applications have diverse effects on the uptake of fission products from soils. Nitrogen fertilizers were found to increase the strontium 90, cesium 137, and cerium 144 content of oat plants, but phosphorus fertilizer decreased the cesium 137 uptake.

Potassium additions have been reported to reduce the uptake of radiostrontium by radish plants and wheat plants and the uptake of cesium 137 by wheat and pea plants. These effects of potassium are in accordance with the depressing effect of excessive potassium on the uptake of calcium and with the chemical similarity of potassium and cesium.

The uptake of cesium from soils usually is only one one-hundredth to one-tenth as great as that of strontium. This apparently results primarily from the fixation of most of the cesium in a nonexchangeable form, that is, not extractable by concentrated salt solutions.

Current experiments by Overstreet suggest that radiocesium in low concentrations may be irreversibly fixed by precipitation in the crystal lattice of the surfaces of micaceous or micalike minerals. This is a different process than those causing the fixation of large amounts of cesium, potassium, and ammonium.

The possibility of a mechanism for the nonexchangeable fixation of strontium 90 also has been under investigation. In order to be of appreciable benefit in the reduction of uptake, most of the strontium eventually would have to be fixed.

On the other hand, even a relatively slow rate of fixation might be important, because of the 28-year half-life of strontium 90. Certain experiments have shown no reduction of availability of strontium to plants with time.

Intensive extraction of California soils with concentrated calcium chloride and ammonium acetate solutions removed all but several percent of radiostrontium applications, but this does not preclude the possibility of long-time fixation effects.

Salt extractions of soils containing strontium 90 from fallout or applications of strontium 89 have shown a greater retention of radiostrontium by southeastern soils than by Ohio soils. Efforts to determine the existence of slow fixation reactions, and of more rapid chemical precipitation reactions by the addition of chemical compounds, are continuing.

Land reclamation: I should preface this by saying that the remedial measures indicated here are investigated as a part of our research program and are designed primarily for situations in which the level of contamination would be many, many fold greater than that at the present time.

If agricultural lands suddenly become extremely contaminated with fission products, especially strontium 90, drastic reclamation methods might have to be employed. Two possible approaches which have been investigated are the leaching of the strontium below the root zone, and the removal of contaminated crops and surface soil from the land.

Under leaching with rain, the downward movement of strontium is slow. During the first years of the strontium 90 monitoring program, the strontium 90 was located primarily in the 0-to-2-inch layer of uncultivated soil. Since then, increasing fractions of the total are found in the 2-to-6-inch layer.

In experimental soil columns, a dilute calcium concentration, as occurs in many irrigation waters, has been found to increase the rate of leaching of radiostrontium over that due to rain, but enormous volumes of such water would be required to move most of the strontium below the root zones of most soils.

Although large applications of acids and salts to soils in outdoor lysimeters, which are small soil plots usually bordered by concrete sides, moved radiostrontium to considerable depths; it was concluded that the cost for treating large areas would be excessive.

I should insert here also that when the strontium is removed from soils by leaching such as indicated here, that we can expect that most or all of the other exchangeable cations, such as calcium, will also be removed at the same time by the same method.

Representative HOLIFIELD. This would leach the soil of desirable elements as well as the undesirable?

Dr. REITEMEIER. Yes, and those might have to be replaced if you had to resort to such a practice.

Representative HOLIFIELD. I suppose probably the largest leaching experiment that we have ever known has been by the people of Israel near the Dead Sea where they have taken the soil there by baskets and handwashed it to get the salt content out.

The experimenal work that you are doing here would, of course, be very valuable in case the time should ever come when a nuclear war occurred and there was a heavy contamination of strontium 90 and cesium 137 and other long-lived isotopes on our bread-growing and vegetable-growing soils in the temperate zone.

Dr. REITEMEIER. That is the purpose behind that type of experimentation.

Representative HOLIFIELD. We hope we will never have to solve that problem, but I do think it is wise that the experimentation you are conducting is going forward.

Dr. REITEMEIER. The physical removal of contaminated crops and soil would be most effective immediately after the fallout, because with time, rain would redistribute particles and leach fission products into the soil. Field experiments in 1958 indicated that the harvesting of contaminated row crops, such as soybeans and sudan grass, can remove about three-fourths of the contamination. It should be added here that this particular harvesting consisted of two operations.

First, a mowing of the plants , which was followed by a flail forage chopper which removed the stubble and a small amount of soil. Thus the value of three-quarters reduction resulted from a combination of these two successive treatments.

Cutting and rolling of bluegrass sod and raking of heavy mulches removed over 90 percent. The most effective soil removal procedure tested was the scraping of level, smooth harrowed soil by a road grader, which removed virtually all the applied radioactivity. Disked or plowed land was less easily decontaminated.

These experiments are continuing.

Senator AIKEN. How would the scraping of the harrowed soil by the road grader remove the applied radioactivity? What became of it?

Dr. REITEMEIER. This is a matter of moving it from one place to another. The problem of ultimate disposal of the contaminated soil is still with us, even experimentally. I believe your question would mean that when the contaminated soil is scraped off into windrows, what do you do with the windrows. Is that correct?

Senator AIKEN. This would not be plowed soil. It would not be plowed before it was harrowed smooth and run over with a road grader.

Dr. REITEMEIER. One of my last statements was that disked or plowed land was less easy to decontaminate than the level, harrowed soil. There is a difference in effectiveness of the roughness of the land surface.

Senator AIKEN. How about land that has been rolled?

Dr. REITEMEIER. Actually, rolling was studied in these experiments.

Senator AIKEN. It was effective?

Dr. REITEMEIER. The results are, so far, inconclusive. Two types of rollers were used but on two different types of soil. We have not gotten the final answer yet as to the benefits of rolling in general.

Senator AIKEN. I ask that because on most dairy farms the land where the corn, clover, or grass is raised is replanted and rolled and harrowed every few years. That would indicate, if there was still contamination in the milk, it could come from pastureland which had not been plowed or rolled or harrowed. Would that be likely?

Dr. REITEMEIER. I am not quite clear on whether you are thinking of the situation that I would be discussing here with regard to decontamination of very heavily contaminated land or the present situation.

Senator AIKEN. Most dairies practice the rotation of crops, which means that the soil is replanted, rolled, and harrowed every few years. In the case of alfalfa it might go for a longer period of years, but particularly where they raise corn, sometimes every third year that soil is replanted.

Would that be helpful in warding off any contamination from fallout?

Dr. REITEMEIER. I believe the question is quite a different one than I was presenting here. There well may be an effect of that type even in the present situation. Actually, the problem which I was discussing would ask this question:

If you could prepare your land surface in a certain condition before a known heavy fallout, what type of a surface should you prepare?

The answer, so far, from the current experimentation is that you should put it in a seed bed type of condition. This anticipates a heavy deposition.

Senator AIKEN. Do you know how far down into the soil the fallout will go of its own volition?

Dr. REITEMEIER. Probably negligible.

Senator AIKEN. Very close to the top?

Dr. REITEMEIER. Just from gravity or settling or even with some wind action we would not anticipate a very deep penetration.

Senator AIKEN. It would virtually all be on the surface.

Dr. REITEMEIER. Yes. Unless it was accompanied by a very heavy rain which might cause some leaching at that time.

Senator AIKEN. One other question and then I will try not to be a nuisance any longer.

I note that you say that potash tends to reduce the uptake of radiostrontium by radish plants and I think you said by others, too. Do you know in what way potash retards the strontium in a plant? What is the reason for it? Is it a chemical reason?

Dr. REITEMEIER. As I indicated in that section, a similar result has been known for some time to occur in the case of potassium on calcium uptake. You probably know that some of the northeast pastures have a history of a potassium to calcium relationship problem in which even rather modest amounts of potassium have reduced the uptake of calcium by the forage crops to rather low levels.

In some of your northeastern areas this has been a continuing problem. The conclusion I submit is that the effect of potassium on the uptake of strontium is similar to the mechanism of the effect of potassium on calcium. Again, strontium is following the calcium to a great extent without any particular need by the plant for strontium.

Senator AIKEN. You pointed out that phosphorus retards the uptake of cesium.

Dr. REITEMEIER. Yes.

Senator AIKEN. You did not say anything about strontium. Have you tried it with strontium 90?

Dr. REITEMEIER. Actually, these experiments were done elsewhere. The particular results that you are referring to are from the Soviet Union and I do not know that we have repeated that particular type of experiment.

Senator AIKEN. Then all the various trace elements that are used to fertilize these days—manganese, and a little of everything in fact—is there a possibility that some of those will have a fixing character as far as fallout is concerned?

Dr. REITEMEIER. I would not expect much direct interaction between the minor elements and the fission products. I would think that the uptake of fission products would usually be determined more by the predominant cations and nutrients in the soil such as nitrogen, phosphorus, calcium, and potassium.

Senator AIKEN. Potash retards the uptake of strontium. You say strontium is not partial to potatoes. Potatoes are partial to potash. Tobacco is partial to potash. Have you experimented with a plant like tobacco?

Representative HOLIFIELD. Here is a tobacco man here. He says yes; quite a bit of it.

Senator AIKEN. Yes, but he comes from a place where they have little fallout. What is good is experience.

Representative HOLIFIELD. I think they have some fallout in North Carolina as well as Vermont.

Representative DURHAM. There has been quite a bit of work done on tobacco.

Representative HOLIFIELD. The filters in the cigarettes take all that out—I suppose some would claim.

As I understand, the experiment you referred to in the last sentence of your statement contemplated scraping 1 or 2 inches of the soil completely off the used ground; and by removing that top 1 or 2 inches from the natural fallout residue, you decontaminate the field. The point that you make there that this plowed land was less easily decontaminated is because the surface of the soil being broken, the material has a chance to sift down or to be carried down by rainwater more than if the top of the soil is a firm crust, such as it would be on unplowed land. Is that approximately correct?

Dr. REITEMEIER. Actually, we feel the biggest effect here is merely the effect of the degree of roughness of the land on the efficiency of the removal of the surface soil by the scraping procedure.

Representative HOLIFIELD. It is mechanical?

Dr. REITEMEIER. It is mechanical, primarily.

Representative HOLIFIELD. But there would be the factor of plowed or disked land having the surface fallout carried down as deep as it is disked by rainwater pushing it down.

Dr. REITEMEIER. This is possible—except, as we have indicated here, the movement of strontium under rainfall is relatively slow.

With the large amounts of contamination we are thinking of here, there might be a substantial movement of strontium because so much was initially deposited.

Representative HOLIFIELD. And the same way, in the cutting of crops that had been exposed to a heavy fallout, you would contemplate moving the stems and the leaves and so forth completely off the land and burning in an isolated place in order to concentrate the radioactivity off the vegetable-growing land.

Dr. REITEMEIER. By burning, or some other disposal method.

Senator AIKEN. Doctor, would there be a possibility that you could develop some machine that you could drive over the land that has a particular attraction for fallout particles that would just scoop them up and carry them to a safe place somewhere?

Is that carrying imagination too far?

Dr. REITEMEIER. This would be a novel method of decontamination. In some cases some of the fallout particles are magnetic. Perhaps a strong magnet might remove some of the magnetic particles.

Senator AIKEN. I think that would be entirely possible, and necessary, as well.

Representative HOLIFIELD. I can understand the Senator's concern about that thin topsoil in Vermont. In California we have such thick topsoil that we can easily spare the top 2 inches.

Senator AIKEN. We can grow bigger crops on 2 inches of our topsoil than you can on 2 feet in some places.

Senator HOLIFIELD. Thank you very much, Doctor.

Dr. REITEMEIER. Thank you, Mr. Chairman.

(Dr. Reitemeier's formal statement follows:)

SOIL AND PLANT RELATIONSHIPS OF FISSION PRODUCTS

R. F. Reitemeier*

Introduction

The fission products contained in fallout particles enter the food chain of man primarily through plants and soils. Some particles are deposited initially on the plants, the remainder on the soil. It is proposed to discuss here mechanisms of intake of fission products by plants, reactions with soils, and land reclamation and decontamination measures.

A fraction of the particles deposited on food crops will be ingested by animals and humans if surface decontamination measures are not employed. Rain and wind will move some of the particles from plant to soil. The extent of this movement will depend on the shape of the plant and the characteristics of the plant surfaces. Actually, the meteorological conditions occurring during the fallout affect the initial distribution of particles between plant and soil. Fallout during a moderate or heavy rain, for example, will be expected to be deposited on the soil to a greater extent than is dry fallout. The particles remaining on the plants are subject to dissolution, to a degree governed by the solubility characteristics of the particles and the weather conditions occurring after deposition. Light rains, mist, fog, and dew increase the opportunity for dissolution. Some of the fission products made soluble by this action are absorbed into the plant, and a fraction of these may be translocated to other parts of the plant.

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Subsequent rains may leach some of the absorbed radioisotopes from the plant onto the soil. These, being already in a soluble form, can participate immediately in the reactions with the soil. Those isotopes in particles deposited on the soil must first be liberated by solution of the particles, by water, acids, or exchange reactions. The mobilized fission products in the soil are then free, to varying degrees, to move through the soil, and to enter plant roots and be transported to other plant parts.

Some of our present knowledge has been obtained under actual fallout situations, the rest with completely soluble sources of radioisotopes. In the latter case, it is presumed that maximal biological availability of the isotope is manifested.

Foliar Absorption

The extent of absorption of a radioisotope by aboveground parts of plants is usually determined by spraying or painting a particular part, or the entire aerial portion, with a solution containing a known amount of the isotope, and after washing or skinning, measuring the residual amount of radioactivity by Geiger counting or radioautography. Translocation of the isotope following absorption is detected in a similar manner, by treating only one plant part and later measuring the radioactivity in various other parts. Moderate absorption of isotopes of strontium, barium, ruthenium, and rubidium by the foliage of bean, beet, and tomato plants has been demonstrated (19). The translocation of the absorbed strontium to other parts, however, was very slight, and negligible in the direction of the roots. Absorption of radiostrontium into

tomato fruits from applications painted on their surfaces also occurred, as shown by radioautographs. The moderate leaching of root-absorbed strontium 90 from leaves by water sprays also has been established (18).

Following applications of solutions containing strontium 90, ruthenium 106, cesium 137, and cerium 144, to some leaves of sunflower and kidney bean plants, the movement of cesium to the other leaves and other above-ground parts was one hundred times as great as that of the other isotopes (5). Middleton (8) found that radiostrontium sprayed on the foliage of wheat, potato, bean, cabbage, sugar beet, and swede plants moved to untreated parts to only a limited extent, while radiocesium moved to all parts of the plant. The spraying of radiostrontium on immature heads of wheat effected a much higher strontium content of the grain than did spraying of plants before emergence of the heads.

Stem-Base Absorption

The relatively high strontium 90 content of the foliage of permanent grass pastures in the United Kingdom has not been explainable by soil uptake or foliar absorption. The concentration in the grass of some pastures appears to be ten times as high as would be expected by way of the soil. A further mechanism, called stem-base absorption, has been proposed to explain this situation (16). Established grass pastures in humid climates develop a root mat consisting of upper roots, the basal portion of the stems, and other organic matter. This mat lies just above the soil surface. According to the proposed theory, it intercepts strontium 90 washed from the surfaces of the grass leaves by rain, and

detains it long enough for a considerable fraction to be absorbed into the roots or stem, and transported into the leaves. Soil reactions which would reduce the availability of strontium to roots, as adsorption, fixation, and dilution with calcium, are thereby avoided. A consequence, in addition to the increased efficiency of uptake by the grass, is that the strontium 90 content of the plant depends primarily on the current rate of deposition of fallout and not on the cumulative amount of strontium 90 in the soil. This mechanism would be expected to be less important in other areas. Pastures in the United States, for instance, are replanted relatively frequently, and the vegetation usually is a legume or a grass and legume mixture.

Soil-Plant Factors

The customary problem in soil fertility is the increase of uptake of chemical elements by plants. The main goal of research on radioactive contaminants, however, is the reduction of uptake of elements. The absorption of a radioisotope from soil by plant roots requires the simultaneous occurrence of three conditions. The isotope must be in the active root zone of the plant, its binding to soil particles must not be too tight, and the plant must have a mechanism for the absorption of the particular element. Most fission products of greatest importance in the food chain, for example strontium, cesium, barium, ruthenium, and rare earths, occur in soils and plants as cations, positively charged atoms. They become attached to soil particles, to varying degrees, by such reactions as adsorption, cation exchange, and chemical precipitation. Of this group of elements, strontium is consistently found to be absorbed the most. The others are absorbed only to a relatively slight extent.

Strontium behaves similarly to the chemically related nutrient element and important exchangeable cation, calcium. Where both the exchangeable strontium and calcium are uniformly distributed through the entire root zone, as in most pot experiments, the ratio of the two elements in the shoots is approximately the same as on the exchange complex of the soil (13), that is, discrimination between the two is slight. In the field, however, the strontium 90 and calcium seldom are uniformly distributed in the root zone of any crop. Further, the root habits of crops vary with soil condition. Where the strontium 90 is located near the soil surface, shallow rooted crops, as many grasses, will have a higher strontium to calcium ratio than deeper rooted crops (9). Where it has been moved to a lower depth, as by plowing, the ratio in shallow rooted crops will be reduced.

Dilution of the strontium 90 by natural or applied available calcium often reduces the strontium 90 content of the crop. In this country differences among soils are more striking than effects of liming of a particular acid soil. The range of natural exchangeable calcium levels is wide, while even in very acid soils the exchangeable calcium level usually can be raised only several fold by lime applications. Additions of lime in excess of the cation exchange capacity are of no benefit (14). It is currently recommended that lime be applied only in amounts expected to provide better crops (2).

Fertilizer applications have diverse effects on the uptake of fission products from soils. Nitrogen fertilizers were found to increase the strontium 90, cesium 137, and cerium 144 content of oat plants, but

phospharus fertilizer decreased the cesium 137 uptake (5). Potassium additions have been reported to reduce the uptake of radiostrontium by radish plants (6) and wheat plants and the uptake of cesium 137 by wheat and pea plants (5). These effects of potassium are in accordance with the depressing effect of excessive potassium on the uptake of calcium and with the chemical similarity of potassium and cesium.

The uptake of cesium from soils usually is only one-hundredth to one-tenth as great as that of strontium (15). This apparently results primarily from the fixation of most of the cesium in a non-exchangeable form (11), that is, not extractable by concentrated salt solutions. Current experiments by Overstreet (12) suggest that radiocesium in low concentrations may be irreversibly fixed by precipitation in the crystal lattice of the surfaces of micaceous minerals. This is a different process than those causing the fixation of large amounts of cesium, potassium, and ammonium.

The possibility of a mechanism for the non-exchangeable fixation of strontium 90 also has been under investigation. In order to be of appreciable benefit in the reduction of uptake, most of the strontium eventually would have to be fixed. On the other hand, even a relatively slow rate of fixation might be important, because of the 28 year half-life of strontium 90. Certain experiments have shown no reduction of availability of strontium to plants with time (1, 4). Intensive extraction of California soils with concentrated calcium chloride and ammonium acetate solutions removed all but several percent of radiostrontium applications, but this does not preclude the possibility of long-time fixation effects (17). Salt extractions of

soils containing strontium 90 from fallout or applications of strontium 89 have shown a greater retention of radiostrontium by southeastern soils than by Ohio soils (13). Efforts to determine the existence of slow fixation reactions, and of more rapid precipitation reactions by the addition of chemical compounds, are continuing.

Land Reclamation

If agricultural lands suddenly become extremely contaminated with fission products, especially strontium 90, drastic reclamation methods might have to be employed. Two possible approaches which have been investigated are the leaching of the strontium below the root zone, and the removal of contaminated crops and surface soil from the land.

Under leaching with rain, the downward movement of strontium is slow. During the first years of the strontium 90 monitoring program, the strontium 90 was located primarily in the zero to two inch layer of uncultivated soil. Since then, increasing fractions of the total are found in the two to six inch layer (3). In experimental soil columns, a dilute calcium concentration, as occurs in many irrigation waters, has been found to increase the rate of leaching of radiostrontium over that due to rain (10), but enormous volumes of such water would be required to move most of the strontium below the root zones of most soils. Although large applications of acids and salts to soils in outdoor lysimeters moved radiostrontium to considerable depths, it was concluded that the cost for treating large areas would be excessive (12).

The physical removal of contaminated crops and soil would be most effective immediately after the fallout, because with time rain would redistribute particles and leach fission products into the soil. Field experiments in 1958 indicated that the harvesting of contaminated row crops, such as soybeans and sudan grass, can remove about three-quarters of the contamination (7). Cutting and rolling of bluegrass sod and raking of heavy mulches removed over 90 percent. The most effective soil removal procedure tested was the scraping of level harrowed soil by a road grader, which removed virtually all the applied radioactivity. Disked or plowed land was less easily decontaminated.

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Representative HOLIFIELD. Our next witness is Dr. C. L. Comar, Cornell University.

Now we come to the subject of animals, persons—discrimination or rejection, as we call it, Senator. I think your word is better.

Senator AIKEN. Rejection rather than discrimination.

Representative HOLIFIELD. Yes. We use that so much in civil rights legislation, I think rejection is better.

Senator AIKEN. Discrimination is a kind of tricky work around here.

Representative HOLIFIELD. Doctor, you may proceed.

STATEMENT OF DR. C. L. COMAR,* CORNELL UNIVERSITY

Dr. COMAR. Mr. Chairman, members of the committee, you have my statement before you. In the interest of economy of time, I propose to read through it rather rapidly, perhaps skipping some of the parts that you can read for yourself.

This statement is limited to consideration of the mechanism by which the important fallout radiocontaminants move through the food chain to become deposited in the human population. Many of the details have been reported in the scientific literature (see references) and have been mentioned in whole or in part during the 1957 hearings of this subcommittee. It is the intention to present an overall concept of these mechanisms, to indicate where the uncertainties and disagreements lie, and to emphasize any change of concepts or data during the past 2 years. In order to permit more detailed discussion, many aspects considered less important have been omitted. It should be noted, however, that in their work scientists are giving considerable attention to any situation that may conceivably have a bearing as insurance for future welfare.

Actual experimentation with fission products in farm animals was started as early as 1948 by the writer in a program supported by the U.S. Atomic Energy Commission. Even though there was no recognition of the fallout problem as it now exists, there was at that time an intuitive feeling that we should develop more data about the behavior of the "bone seeking" fission products in the food chain. Since that time, studies have been done with radioisotopes of strontium, cesium, iodine, and others, using beef cattle, dairy cattle, sheep, swine, poultry, and so forth, in addition to laboratory animals. In later years, vari-

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In recent years Dr. Comar has served on many international committees and is presently a consultant on matters of radiation and health to the United Nations Food and Agriculture Organization, United Nations Scientific Committee on the Effects of Atomic Radiation, American Institute of Biological Sciences, Health and Safety Laboratories of the USAEC, U.S. Public Health Service, Cincinnati, Oak Ridge Institute of Nuclear Studies, and the Stanford Research Institute.

Dr. Comar was one of the first to do radioisotope research with farm animals, and for many years has been conducting studies on the movement of fission products through the animal phase of the food chain. He is the author of over a hundred scientific papers dealing with these subjects and of a textbook entitled "Radioisotopes in Biology and Agriculture." He is also editor of an AAAS symposium volume entitled "Atomic Energy and Agriculture" and a book in preparation entitled "Mineral Metabolism—An Advanced Treatise."

ous phases of this program have been supported by other agencies in addition to the Atomic Energy Commission; namely, the Department of Defense, the Department of Agriculture, and the Public Health Service.

The ideas presented here, it should be noted, have been derived from longstanding associations with many colleagues whose names are to be found in the reference citations and are well known in the literature.

Studies of the metabolism, movement, and effects of fission products in the food chain may be of value in the following ways:

(*a*) Predictions are made possible of the relationships between the level of radioactivity in parts of the food chain and the level to result in the human population.

(*b*) A point of departure is provided for the study of ways by which the levels in the human population may be minimized.

(*c*) A basis is provided for setting up environmental sampling programs and for interpreting the results.

(*d*) Results with animals are made available and may have to be used where direct observations are not possible on human beings.

(*e*) If environmental levels should ever become high enough to cause observable effects, it is most likely that effects would be seen first in the grazing animal population.

RADIOISOTOPES OF IMPORTANCE IN ENVIRONMENTAL CONTAMINATION

As shown in table 1, it is considered that iodine 131, barium 140, strontium 89, strontium 90, and cesium 137 will be of primary importance from the standpoint of hazard to the human population from fallout. Other radioactive substances are of lesser importance because of (*a*) low fission abundance, (*b*) short half-life, and (*c*) nonentry into food chain. Before confining our interests to the radioisotopes listed in table 1, however, a general comment or two may be in order.

TABLE 1.—*Radioisotopes of importance in environmental contamination*

		Physical half-life
Iodine 131	days	8
Barium 140	do	13
Strontium 89	do	51
Strontium 90	years	28
Cesium 137	do	30

Radioactive materials that are poorly absorbed by plants from soil and any that are poorly absorbed by animals from their feed may still reach the human diet as a result of consumption of vegetation carrying adsorbed radiocontaminants. Any hazard would result from exposure of the gastrointestinal tract of man to radiation from the unabsorbable material passing through. Experiments with goats and dogs have indicated that this exposure would be small compared to the simultaneous exposure from materials deposited within the body. The reasons are: (*a*) relatively rapid movement of substances through the gastrointestinal trace; (*b*) absorption of the radiation by the intestinal contents, thus giving a shielding effect; and (*c*) the high prob-

ability that any radioactive substance adjacent to the mucosal tissue would be in contact with less active cells.

The problem of carbon 14 in the food chain has not been studied in detail. It would seem that the specific activity of carbon 14 in the vicinity of plants would ultimately be reflected as such in the carbon of the human body. In the event of rapidly changing carbon 14 levels, then, one would have to consider the timelag in passage of carbon atoms from the vicinity of plants to the human population. Also, there is the matter of rates of carbon turnover. Work needs to be done along these lines, but these considerations appear to be of lower priority at this time.

Representative HOLIFIELD. Is it not true that the carbon 14 element in the collection of isotopes is very, very small?

Dr. COMAR. Yes, sir.

Representative HOLIFIELD. This is one of the reasons it is the smallest of the amounts in comparison with strontium and other long-lived isotopes, which makes it of not too much importance at this time.

Dr. COMAR. This is exactly why I suggest that this is of lower priority. I would only make the point that we should always try to be farsighted and look ahead to any situations that may have a bearing later on.

Representative HOLIFIELD. For the purpose of the record, can you give us the half-life of carbon 14?

Dr. COMAR. It is of the order of 5,600 years.

Representative HOLIFIELD. So we are very fortunate that there is a very little amount of it in the bomb explosion, are we not?

Dr. COMAR. Yes, sir.

Again turning attention to table 1 it is noted that iodine 131 and barium 140 have relatively short half-lives (8 and 13 days respectively); strontium 89, an intermediate half-line of 51 days, and strontium 90 and cesium 137 have relatively long half-lives of 28 to 30 years.

Tables 2 and 3 indicate the metabolic behavior of these radioisotopes in the body. Iodine 131 is well absorbed, collects in the thyroid gland, and is fairly rapidly removed by physical decay and biological action. Barium 140 and the two radioisotopes of strontium are only partially absorbed, but most of that which is absorbed is deposited in the skeleton. Barium 140 rapidly disappears by physical decay, and strontium 89 in the skeleton decreases somewhat more slowly by the same process. Strontium 90 is slowly removed by physical and biological processes, and substantial amounts of any strontium 90 once deposited will be present throughout the life of the individual. The rate of removal would be faster in a growing individual than in a mature individual. Cesium 137 is well absorbed into the body, collects in muscle and other soft tissues, and enters cells in a manner similar to that of potassium. It is removed from the body by biological action with a half-time of removal that has been estimated at about 140 days in man.

TABLE 2.—*Metabolic behavior*

Iodine 131	Collects in thyroid.
Barium 140, strontium 89, strontium 90	Like calcium, collect in bone.
Cesium 137	Like potassium, collects in muscle.

TABLE 3.—*Removal rate from body*

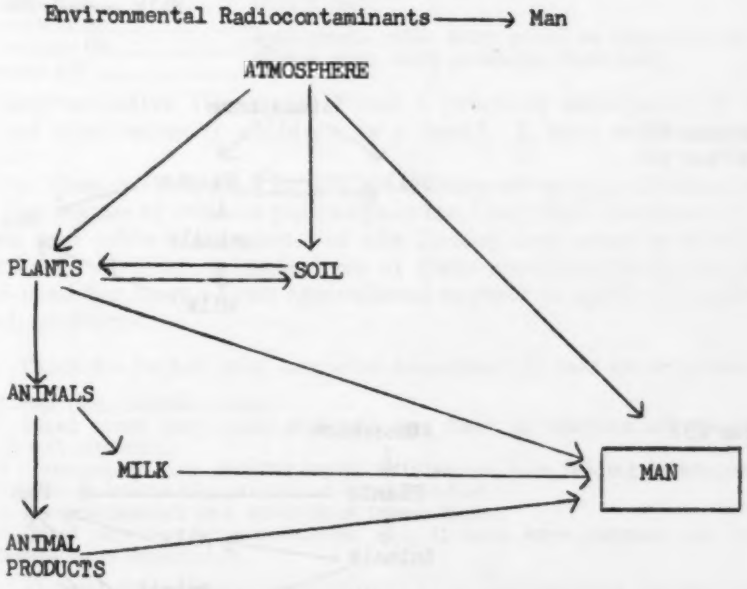
Iodine 131	Fast.
Barium 140	Fast (physical decay).
Strontium 89	Relatively fast (physical decay).
Strontium 90	Slow.
Cesium 137	Relatively fast (biological half-time of 140 days).

THE FOOD CHAIN

Figure 1 presents a schematic diagram of the major terrestrial pathways by means of which environmental radiocontaminants can reach man. The following list of possible pathways sums the information in this diagram:

FIGURE 1

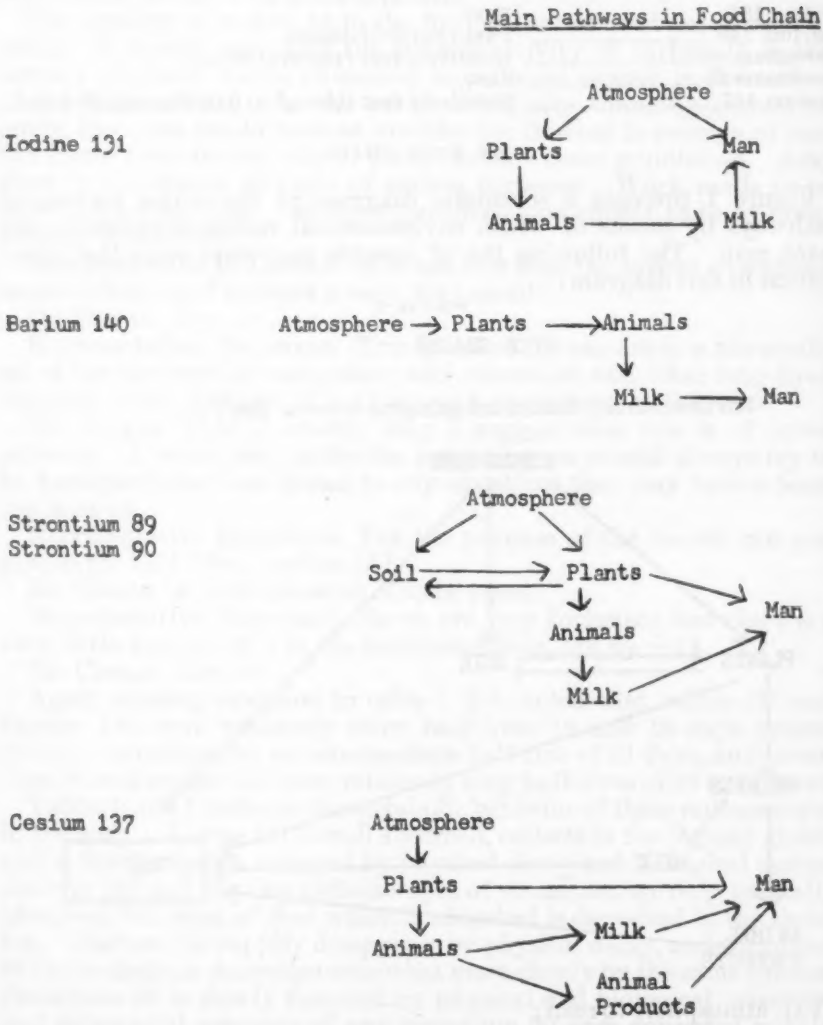
FOOD CHAINS



(*a*) atmosphere to man;
(*b*) atmosphere to plant to man;
(*c*) atmosphere to soil to plant to man;
(*d*) atmosphere to plant to animal to milk to man;
(*e*) atmosphere to soil to plant to animal to milk to man;
(*f*) atmosphere to plant to animal to animal products to man.
(*g*) atmosphere to soil to plant to animal to animal products to man.

Figure 2 summarizes the pathways that are important for the various radioisotopes.

FIGURE 2



Iodine 131: Because of the relatively short half-life, the soil step is not important and only pathways (*a*) and (*d*) are significant.

Senator AIKEN. Does the amount of the contamination decrease with each step?

Dr. COMAR. Yes. This depends entirely upon the specific radioisotope, and if I may defer your question particularly on strontium we will consider that in some detail in just a few minutes.

Senator AIKEN. It never builds up though in the various steps?

Dr. COMAR. That is right.

Barium 140: Again, because of the short half-life only pathway (*d*) is significant.

Strontium 89, 90: Because of the low concentration in muscle tissue, the important pathways are (*b*), (*c*), (*d*), (*e*).

Cesium 137: Because cesium 137 is fixed in soil and markedly unavailable to plants, and because this radioisotope is deposited in both muscle tissue and milk, the important pathways are (*b*), (*d*), (*f*).

Senator AIKEN. Does this include eggs?

Dr. COMAR. Yes, eggs.

Consideration of the most likely pathways enables an appropriate choice of environmental samples. These are summarized in table 4. It is noted that milk is useful for analysis of all the radioisotopes listed. With iodine 131 and barium 140 the radioassay must be carried through before loss of radioactivity by physical decay. It follows from various physical and biological considerations that thyroid tissue is useful for monitoring of iodine 131 and whole body counting for monitoring of cesium 137.

TABLE D.—*Samples of importance for monitoring*

Iodine 131	Plants, milk, thyroid.
Barium 140	Plants, milk.
Strontium 89	Soil, plants, milk, dairy products, bone, aquatic food.
Strontium 90	
Cesium 137	Plants, milk, dairy products, whole body.

Representative HOLIFIELD. From a practical standpoint, it has a broad application to children as a food? I have reference to the milk.

Dr. COMAR. Yes, sir. It is a good integrating type of sample.

The matter of relative pathways in the food chain is extremely complex and table 5 lists some of the factors that must be taken into account. Mention is made here of these uncertainties to emphasize the need for more of our agricultural experts to apply themselves to such problems.

TABLE 5.—*Factors that determine importance of food chain pathway*

1. Soil (e.g., calcium level).
2. Plant cover (e.g. heavy root mat may delay Sr reaching soil for dilution with soil calcium).
3. Management (e.g. plowing depth, fertilization, barn feeding versus pasture).
4. Dietary habits (e.g. proportion of milk in diet).
5. Food technology (e.g. washing of frozen foods).
6. Time pattern of contamination (e.g. if tests were stopped soil reservoir becomes more important).

(*a*) Soil and climatic conditions: It is obvious that the soil characteristics, calcium level, terrain and meteorological conditons will have an important bearing on the amounts of various fallout contaminants that reach the plant via the soil, or that stay on the plant.

(*b*) Plant cover: As an example, a heavy root mat may delay radiostrontium reaching the soil for dilution with soil calcium and lead to higher Sr*/Ca ratios in the plant than would otherwise be expected. (Sr* refers to any radioactive strontium.)

(*c*) Management: The levels of environmental contamination at various steps of the food chain in any given situation may well depend upon such factors as plowing and fertilization procedures and feeding practices (e.g., barn feeding, pasture, watering in shallow ponds).

(*d*) Dietary habits and food technology: The actual amounts of radiocontaminants that reach the human body will depend upon such things as: (1) proportion of milk in diet, (2) amounts of leafy vege-

tables and extent of washing, and (3) use of frozen foods which are well washed.

(*e*) Time pattern of contamination: At short times after production of radiocontamination, the shorter-lived radioisotopes such as iodine 131, barium 140 and strontium 89 may be of most importance. At longer times the strontium 90 and cesium 137 become more important. If contamination ceases, then the soil reservoir becomes of more importance and direct fallout on vegetation is minimized. Thus the relative importance of the atmosphere to plant pathway versus the soil to plant pathway depends upon the accumulated soil reservoir as compared to the fallout rate at any time given.

Representative HOLIFIELD. This chart shows the simple activity of washing foods before frozen and packaged would eliminate a great deal of surface contamination.

Dr. COMAR. That is exactly right. And the removing of the outer leaves. If in one culture there was the practice of removing the outer leaves this would automatically remove a lot of the surface contamination. These are just a few of the more important variables. There are many others.

I should like now to discuss strontium 90 relationships.

STRONTIUM 90 RELATIONSHIPS

Since strontium 90 is probably the most important of the environmental radiocontaminants, the remainder of the discussion will be mainly concerned with this substance.

We are interested both in the amount of strontium 90 (Sr^{90}) that reaches the population and the Sr 90/Ca ratio (strontium 90 to calcium ratio) as it is laid down in the human body. These quantities are interrelated; however, there are some advantages in dealing with Sr 90/Ca ratios. This is because exposure of the population occurs primarily by way of strontium 90 contamination of dietary calcium, and strontium metabolism generally parallels and is interrelated with calcium metabolism. The amount of strontium 90 involved can always be calculated from the Sr 90/Ca ratio when the amount of calcium is also known; stable calcium values are invariably determined in experimental and survey work.

Table 6 presents data that have led to an important concept. According to the values for animals it is clearly seen that if the animal is raised on a diet of 100 strontium per 100 calcium then the body of the animal will contain about 25 strontium per 100 calcium.

TABLE 6.—*Comparative use of strontium and calcium*

	Diet		Body	
	Strontium*	Calcium	Strontium*	Calcium
Rat	100	100	28	100
Mouse	100	100	35	100
Rabbitt	100	100	22	100
Sheep	100	100	24	100
Goat	100	100	29	100
Cow	100	100	18	100
Man	100	100	50	100
			25	100

NOTE.—Observed ratio = OR for rat = 0.28.

I think this is a reflection, Senator Aiken, of what you have called rejection and what we have called discrimination.

The early data on rats were obtained by tedious incorporation of strontium 90 into the rat diet, and feeding animals on this contaminated diet for two to three generations. These results have been confirmed by independent methods and are generally accepted. It should be noted that this ratio can be changed by artificial conditions, but one is impressed with the constancy of this ratio on commonplace diets.

The values for man are particularly important and call for especial comment. Obviously, it has been impossible to do careful studies on numerous normal individuals as has been done with animals. Examination of the work that has been done leads to the conclusion that the reduction in Sr/Ca ratio in man under usual conditions is probably not less than two nor more than four; one group has recently reported a reduction of as high as 5.5 based on population studies. However, from the preponderance of data and from a conservative point of view it is recommended that a reduction value of between two and four be used until more definite data are available (i.e., if the diet contains 100 Sr* to 100 Ca then the body of man would be expected to contain 25–50 Sr* to 100 Ca).

Senator AIKEN. I do not know what you use for units. They are not the same size units, are they? It would not be by weight.

Dr. COMAR. This is a very good point. In our experiments we have tried to make the situation comparable to the fallout situation. We have done the work so that these values will be valid for normal calcium levels in the diets as you or the animals would eat them, and the strontium units would be about the levels of strontium 90 that we would expect from fallout. So that if we put these in absolute amounts we would be talking about micromicrocuries of strontium 90 per gram of calcium.

Senator AIKEN. That refers to the calcium units in the diet and not the calcium units in the man, then.

Dr. COMAR. Yes. In the diet it refers to the calcium units in the diet. On the side of man it refers to the calcium units in man. Is that clear, sir.

Senator AIKEN. Yes.

Dr. COMAR. Table 7 presents some information on two factors of some import that may change the discrimination against strontium relative to calcium. In rats it has been definitely shown that very young animals do not discriminate as much against strontium.

TABLE 7.—*Factors that change discrimination*

AGE

	Sr*	Ca
Rats:		
15 days old	78	100
194 days old	45	100
Man:		
9 to 73 years old	No change.	
Infants	No data.	

DIET

Rats:		
Nonmilk diet	28	100
Milk diet	57	100
Man:		
Nonmilk diet	44	100
Milk diet	54	100

In the young rats we ended up with 78 strontium to 100 calcium as compared to adults with 48 strontium to 100 calcium. This would indicate that in very young individuals one tends to lose some of this discrimination. From limited studies on man the discrimination in a 9-year-old child was the same as measured in adults up to 73 years of age. However, we have no data on very young children and this is another reason for conservatism.

In rats it can be seen that milk in the diet tended to reduce the discrimination against strontium. However, from limited studies with man, milk did not appear to show as marked an effect as it did with rats.

One notices in the rats 28 units of strontium for every 100 units of calcium, but on a milk diet it was increased and one got more strontium. This has been studied in fair detail and we think we know why this happens.

Senator AIKEN. May I get straight on that milk diet and nonmilk diet?

Dr. COMAR. Yes, sir.

Senator AIKEN. On a milk diet, you can absorb more strontium than on a nonmilk diet, without harm.

Dr. COMAR. We are not talking about harm at these levels. There is no question of harm involved. This increased absorption does occur and is one of the reasons that milk is such a good food. We find that calcium absorption itself is much better from milk than from other sources. It turns out that we lose a little bit of the discrimination on the milk diet.

I would emphasize that the data are not yet clear for man so that we really do not know whether this effect holds with man. There is a suggestion.

Representative HOLIFIELD. There are two things involved here, as I see it. The milk is richer in calcium and the calcium has an affinity for strontium 90 which other elements do not have. As calcium is a bone seeker, it is more effective in the younger person who is building his skeleton structure than it is to the older person who is rejecting calcium but the natural process of not feeding to build a skeleton structure, is that not correct?

Dr. COMAR. This is essentially correct. It is a rather complex problem to explain the whole thing and I would rather not go into too much detail.

Representative HOLIFIELD. When I make these summaries occasionally, it is for the layman who may try to understand something which is quite technical. If I at any time make a summary that is generally not right I hope I will be corrected. There will be no embarrassment on my part.

Dr. COMAR. The physiological processes within the bodies of animals and man that are responsible for the differential behavior of calcium and strontium have been widely studied. These findings are illustrated in the next table, No. 8.

TABLE 8.—*Processes that discriminate against strontium*

1. Absorption from alimentary canal	Large.
2. Urinary excretion	Do.
3. Movement from blood to milk	Do.
4. Movement from mother to fetus	Do.
5. Movement between blood and bone	Small.
6. Endogenous, biliary, pancreatic excretion	Do.

In general, processes that involve physiological handling and most likely active transport of calcium are the ones that tend to move calcium in preference to strontium. The processes of known importance are: (*a*) absorption from the alimentary tract, (*b*) urinary excretion as a result of efficient tubular reabsorption of calcium and poor reabsorption of strontium, (*c*) secretion from blood into milk and (*d*) movement from blood of the mother to the developing fetus. These, then, are the processes that should be studied if one wants to alter artificially the differential behavior of calcium and strontium.

The Sr/Ca ratio that is developed in the fetus is shown in table 9. It is noted that the diet of the mother rat contains 100 strontium-100 calcium, the body of the mother contains 28 strontium-100 calcium, and the developing fetus then contains about 17 strontium-100 calcium. Similar data have been obtained with rabbits, and strontium 90 survey data of human stillbirths confirm the idea that the fetus will have about one-half the Sr/Ca ratio of the mother.

TABLE 9.—*Comparative secretion of strontium and calcium in fetus of rat*

	Sr*	Ca
Diet of mother	100	100
Body of mother	28	100
Body of fetus	17	100

More specifically, if I might add, the diet of the mother during the period of gestation will really determine the strontium-calcium ratio in the fetus, and this may have a practical implication.

The comparative secretion of strontium and calcium in the milk is very important from a practical standpoint.

Representative HOLIFIELD. May I stop you for a question that just came up between Congressman Durham and me. Does the strontium 90 deposit in the fetus have somatic effect or genetic effect on the fetus?

Dr. COMAR. This is a little out of my field. I would guess that the effect would be somatic at that time.

Representative HOLIFIELD. But in the case of an X-ray of a prospective mother, the fetus would be more susceptible to harm than the mother would, would that not be true?

Dr. COMAR. Again I am not sure. I would think that there might be an increased radiosensitivity certainly in the embryo. I think this is true, that there is an increased radiosensitivity at these times.

Representative HOLIFIELD. I think there have been some studies on that although I am not quite sure.

Senator AIKEN. I notice in your statement, Doctor, at the bottom, you say that actual amounts of radiocontaminants that reach the human body would depend on such things as the proportion of milk in the diet, amounts of leafy vegetables, and extent of washing and use of frozen foods. What does that mean with particular reference to the proportion of milk in the diet?

According to one of your charts if the calcium is absorbed through milk, it is better than to absorb it in some other way. This statement could be interpreted as belying that.

Dr. COMAR. Again may I defer this for later discussion because I am going to discuss exactly this point as to what the milk contributes compared to what the vegetation and the others contribute.

Senator AIKEN. Very well.

Representative HOLIFIELD. You may proceed, sir.

Dr. COMAR. Table 10 shows that if the diet of the lactating cow or goat contains 100 strontium per 100 calcium, then the milk will contain about 11 to 12 strontium per 100 calcium. These findings have been confirmed many times and are generally accepted. We do not have values for human milk, and these would be helpful in evaluation of the body burden of strontium 90 that will develop in the breast-fed child. Arrangements are now in progress by the New York Health and Safety Laboratory of the Atomic Energy Commission to obtain actual data on mother's milk.

TABLE 10.—*Comparative secretion of strontium and calcium in milk*

	Diet		Milk	
	Sr*	Ca	Sr*	Ca
Cow	100	100	11	100
Goat	100	100	12	100
Woman	100	100	(?)	(?)

We think that this information will be forthcoming before too long.

It may be of interest to show what happens to the strontium 90 that is eaten by the dairy cow. As seen in table 11, about 95 percent is excreted in the feces, about 1.5 percent in the urine, and about 0.9 percent appears in the milk. Each liter (or quart) of milk contains about 0.1 percent of the amount of strontium 90 eaten daily by the cow. These are results of a recent experiment on 10 dairy cows, supported by the U.S. Department of Agriculture.

TABLE 11.—*Fate of strontium 90 eaten by cow*

Milk (percent of daily dose/liter)	0.1
Milk (percent of daily dose/day)	0.9
Feces (percent of daily dose/day)	95.0
Urine (percent of daily dose/day)	1.5

EFFECT OF DIETARY CALCIUM AND STRONTIUM ON STRONTIUM METABOLISM

It seems important to have an understanding of factors that govern the movement of strontium in the food chain. The first consideration would naturally be the level of stable strontium in the diet. Experiments have shown, however, that levels of stable strontium in the feed of animals necessary to cause any significant decrease of ingested strontium 90 are detrimental.

The next step is to ascertain the effect of calcium levels. Table 12 shows the results of a study just completed on dairy cows and goats that had been placed on varying levels of calcium. It is noted that regardless of the level of stable calcium or strontium in the diet, there was obtained the same value of 9 to 12 Sr* per 100 Ca as compared to 100 Sr* 100 Ca in the diet. The implications of this are indicated in table 13. If a cow is fed a diet containing 100 strontium to 100 calcium then the milk will contain about 12 strontium to 100 calcium; according to the concept of the previous table if the diet of that cow were supplemented with stable calcium to contain 100 strontium to 200 calcium, the behavior would be the same as for 50 strontium to 100 calcium, and the milk would contain only 6 strontium to 100 calcium.

TABLE 12.—*Effect of calcium level*

	Diet		Milk	
	Sr*	Ca	Sr*	Ca
Cow:				
Low calcium	100	100	10	100
Normal calcium	100	100	9	100
High calcium	100	100	11	100
High strontium	100	100	12	100
Goat:				
Low calcium	100	100	12	100
Normal calcium	100	100	12	100
High calcium	100	100	12	100
High strontium	100	100	12	100

TABLE 13.—*Implications of raising dietary calcium.*

Diet		Milk	
Sr*	Ca	Sr*	Ca
100	100	12	100
100	200	-------	-------
50	100	6	100

Senator AIKEN. May I ask if the strontium would remain in any part of the cow except the milk? Would it leave the cow or would it be in the meat?

Dr. COMAR. There is a definite pattern of deposition of strontium in animals and a certain proportion stays in the body. I would say

possibly 98 percent of that which stays in the body is in the bone and the rest would be circulating in the blood and muscle tissue. The amount in the meat would be very low.

Representative PRICE. If we had three times the number of strontium units as against calcium units, what would be the result.

Dr. COMAR. We feel that if one does not change the mass the same relationships would hold. So if you had 300 units of strontium to a hundred units of calcium then you would end up with 36 units of strontium to a hundred units of calcium.

Representative PRICE. That is theory.

Dr. COMAR. It is more than theory, sir. I think that there are actual experiments that would support this contention. The important thing is that if we then raise the calcium level compared with the strontium we would essentially have the strontium level in the milk compared with the calcium. I think you can see the possible implications here. This would be one possible way of lowering the strontium to calcium ratio in the milk.

Table 14 gives the results of a relatively long term study in which rats grown on a higher calcium diet accumulated a lower body burden of ingested radiostrontium than those grown on lower calcium diets.

TABLE 14.—*Effect of dietary calcium level on Sr⁹⁰ retention*

Percent calcium in diet	Percent of ingested Sr^{90} in rat
0.5	17.7
1.0	7.0
2.0	3.8

It must be noted that there is disagreement among workers about this sort of result. These arise mainly from experimental difficulties in evaluation of long-term effects from results of short-term studies. Active experimentation is underway, however, and the practical implications will one day be properly understood.

It should be emphasized at this point that from the standpoint of human nutrition there is no better recommendation than presently accepted practices. If ever necessary, it would be more practicable to modify the diet of the milk-producing animals than that of the human population.

I would under no circumstances want to leave the impression that the behavior that we have been talking about here should be advocated for human use or even for animal use at this time. This is information that people should know about and follow.

Senator AIKEN. Does this mean, Doctor, that the time is coming when every one will carry a package of pills in his pocket and if they have reason to believe that they have gone through fallout take a couple of pills and mitigate the effect of it. That chart would indicate such would be possible.

Dr. COMAR. This is why I should like to make the very strong statement and repeat what I have just said; this is experimental work. There are many other nutritional and physiological factors that would have to be taken into account before one could recommend this type of thing as a practical approach.

Senator AIKEN. But this would indicate the possible opening of a new field for medicine, would it not?

Dr. COMAR. This indicates, as you suggest, sir, an approach that people might investigate.

Representative DURHAM. The thing to do is to do away with strontium 90.

Dr. COMAR. This certainly would be preferable.

RELATIONSHIPS BETWEEN SR/CA IN THE VEGETATION AND IN THE HUMAN POPULATION

It is possible to make some estimates of the relationships that will exist between the Sr/Ca ratio of the vegetation and that in the human population when steady-state conditions have been attained. We have chosen to use vegetation as a base because of the uncertaintities and exceedingly long times that will be required for steady-state distribution to be attained in soil.

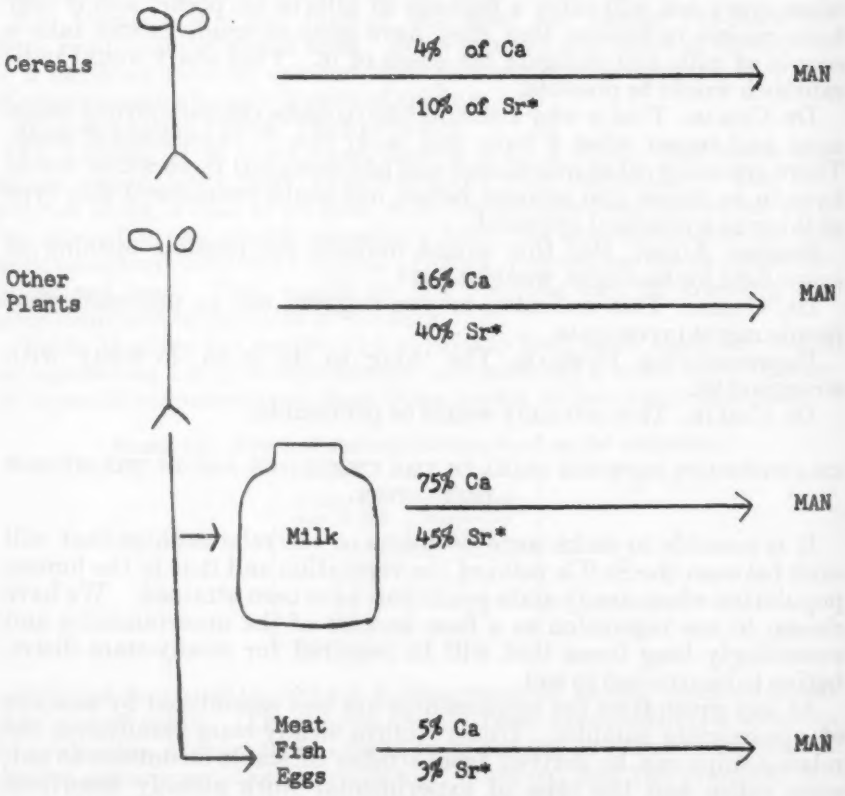
At any given time the relationships are best established by analysis of appropriate samples. Under future steady-state conditions, the relationships can be derived from studies of stable strontium to calcium ratios and the type of experimental work already described. At intermediate times one can only modify the steady-state relationships by educated guesses from present day surveys.

Figure 3 presents an estimation of the relative contributions of calcium and radiostrontium from dietary constituents in the United States. It appears that the sources of dietary calcium are approximately as follows: 4 percent from cereals, 16 percent from other plant products, 75 percent from dairy products, and 5 percent from meat, fish, eggs, etc.

FIGURE 3

Contribution of Dietary Calcium

and Sr* to Man at Equilibrium



You will understand these are overall estimates and various people's estimates differ but in the main they are of this order.

It can be first noted that meat, fish, and eggs contribute to the calcium and strontium content of the diet to only a small extent and are therefore not considered further. If the plant material of the biosphere has the same Sr/Ca ratio, then milk, while contributing 75 percent of the calcium would contribute only 45 percent of the radiostrontium; plant material would contribute about 20 percent of the calcium but 50 percent of the radiostrontium.

It is emphasized, therefore, that under these conditions the contribution of milk should be placed in proper perspective.

As I have heard the other speakers, this point has been made before, and I think it is an important one. We cannot assume that because milk contributes the major part of the calcium that it contributes the major part of the strontium.

Senator AIKEN. That chart shows that the percentage of strontium in proportion to the amount of calcium is less through milk than the other foods.

Dr. COMAR. Yes, sir.

The same considerations are presented in a more usable form in table 15.[1] This represents a scheme by means of which one can calculate using discrimination factors of each part of the diet. I shall not go into detail except to indicate that one comes to a conclusion that the overall reduction in strontium calcium ratio from the vegetation to the human body is about a factor of 10. Other people have estimated in this region and there seems to be agreement that there is only a slight chance that such a reduction factor could be as low as 5 or as high as 20.

TABLE 15.—*Relation between Sr*-Ca of vegetation (100 Sr*-100 Ca) and Sr*-Ca of man*

	Percent of Ca from	Plant product	Diet body	Sr* per 100 Ca in body
Cereals	4	1	0.25	1.0
Other plants	16	1	.25	4.0
Dairy products	75	.12	.5	4.5
Meat, fish, eggs	5	.25	.25	.3
Total Sr* per 100 Ca				10

I would like to point out that these data can easily be modified if there is some knowledge as to special conditions. For example, if a child were getting 100 percent of its calcium from milk, then it can be calculated that the strontium to calcium ratio in its body would be 6 units of strontium per 100 of calcium instead of 10.

Similarly, these figures can be modified if, for example, the cereals were found to average higher and the vegetation that man eats was found to average lower than the vegetation that the cow eats.

This, then, represents a way of evaluating the contribution of the various parts of the diet.

Table 16 presents two other important situations. Because of placental discrimination against strontium, it is expected that the newborn would have about one-half the strontium-calcium ratio existing in the blood of the mother during the period of fetal calcification. It should be remembered that it is the diet of the mother shortly before and during the gestation period that is the most important factor.

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[1] The same considerations are presented in a more usable form in table 15. By multiplication (for each food contributor of calcium) of the following (*a* times *b* times *c*), one arrives at the contribution of each food item to the body in terms of amount of strontium per 100 calcium in the body, assuming again that all plant material contained 100 strontium per 100 calcium:

(*a*) The percent o calcium that the food contributes;

(*b*) The biological discrimination factor in going from the plant to the food contributor; and

(*c*) The biological discrimination factor in going from the food as consumed to the human body.

The first point is that one arrives at an overall reduction in Sr/Ca ratio of about 10 in going from the vegetation to the human body. This seems reasonably conservative; there is agreement that there is only a slight chance that the reduction factor could be as low as 5 or as high as 20.

The data as arranged in table 15 can easily be modified if there is some knowledge as to special conditions. For example, if a child were getting 100 percent of its calcium from milk, then it can be calculated that the strontium-calcium ratio in its body would be 6 strontium per 100 calcium instead of 10 strontium per 100 calcium ($100 \times 0.12 \times 0.5 = 6$). Similarly, these figures can be modified if, for example, the cereals average higher and the vegetation that man eats average lower than the vegetation that the cow eats.

TABLE 16.—*Relations between Sr*-Ca of vegetation (100 Sr*-100 Ca) and Sr*-Ca of newborn and 6-month-old*

I. (Sr*-Ca) of newborn=½ (Sr*-Ca) of mother.
II. Breast feeding may decrease Sr*-Ca of young child by factor of 10 as compared with formula feeding.

It is emphasized that breast feeding of the infant instead of using cow's milk will markedly lower the strontium-calcium ratio that is laid down in the infant. This is because of the added discrimination that takes place during passage of the calcium through the mother's body to the mother's milk.

METABOLISM OF CESIUM AND POTASSIUM

The comparative metabolism of cesium and potassium have not been studied in as much detail as for strontium and calcium.

Some recent data, however, are presented. Table 17 shows the fate of these two elements when ingested by the dairy goat. About 6 percent of the ingested radioactive cesium appears in the milk; other work with dairy cows has given values of up to 13 percent appearing in the milk.

TABLE 17.—*Fate of cesium 137 and potassium eaten by goat*

	Potassium	Cesium
Milk (percent of daily dose per day)	4.4	6.0
Urine	35.0	21.0
Feces	6.0	32.0

Table 18 presents data on comparative metabolism of the two elements based on a diet of 100 cesium—100 potassium. It is of interest to notice that there is a preferential secretion of cesium into milk as compared with potassium. The cesium–potassium ratios among tissues appear to vary considerably, indicating that the comparative metabolic behavior is more complex than it is for strontium and calcium.

TABLE 18.—*Comparative metabolism of cesium and potassium in goat*

	Diet	Metabolism
Milk	100 Cs–100 K	140 Cs–100 K.
Whole body	do	90 Cs–100 K.
Muscle	do	50 Cs–100 K.
Ovary	do	120 Cs–100 K.
Kidney	do	360 Cs–100 K.

POSSIBILITIES OF REDUCING STRONTIUM 90 LEVELS

The possibilities of minimizing strontium 90 movement in the food chain by treatments of soil have been discussed by others. Practical steps to reduce the strontium 90 burden in the population by procedures with man and animals would require considerable research along nutritional and physiological lines. The following are suggested as possible approaches for further work; none are now practicable.

(*a*) Use of uncontaminated calcium to contribute a maximum proportion of the dietary calcium of animals and man.

(*b*) Increasing the biological discrimination against strontium relative to calcium.

(*c*) Avoidance of surface-contaminated vegetation or thorough washing.

(*d*) Use of plants low in calcium relative to calorific value.

(*e*) Increasing the intake of uncontaminated calcium, primarily in milk-producing animals.

(*f*) Removal of strontium from food products.

Mr. Chairman, in conclusion I would only say that much of this work is experimental and we hope has led to some understanding. I would reemphasize that from the nutritional standpoint our best bet is to follow the practices that now seem to be accepted.

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Representative HOLIFIELD. Thank you, Dr. Comar.

Are there any questions?

Representative DURHAM. Doctor, is the university doing the work on its own, or does it have a contract with the AEC?

Dr. COMAR. Cornell University is doing this work with contracts from several Government agencies; from the Atomic Energy Commission, from the Department of Defense, and from the Department of Agriculture. We also have some small construction funds from the Public Health Service; and the AEC has also given us some construction money.

Representative DURHAM. You have given us a very interesting discussion of the whole problem. What is your staff? Is it a university staff or is it part time or full time?

Dr. COMAR. Sir, we endeavor to have our staff become an integral part of the university. We feel they can do their best creative work under a university atmosphere. We do have the problem, if I may mention it, of continuity; our contracts run year by year, and we hope that they will always be renewed. But this is a real problem, to get scientific personnel and ask them to come to work when they have to be paid from a contract that comes year by year.

Representative DURHAM. Is there any other university doing the same type of research work at the present time?

Dr. COMAR. Yes, sir. There are other universities that are doing this work. Much of this work was done in the program at the University of Tennessee, when I was associated there, at the University of California, and many other agricultural people are getting interested in this type of program.

Representative DURHAM. It is a very important field, of course. I hope we pursue it with vigor.

Representative HOLIFIELD. Thank you very much, Dr. Comar.

Dr. COMAR. Thank you, sir.

Representative HOLIFIELD. I might say for the benefit of our audience that we are putting a biography of each of these witnesses in the printed record, and Dr. Comar has a very distinguished record—as have the other witnesses. He has been a consultant and is now, I think, on matters of radiation health to the United Nations Food and Agricultural Organization, United Nations Scientific Committee on Effects of Atomic Radiation, American Institute of Biological Sciences, Health and Safety Laboratory of the U.S. Atomic Energy Commission, the U.S. Public Health Service at Cincinnati, the Oak Ridge Institute of Nuclear Studies, and the Stanford Research Institute. He was also one of the first to do radioisotope research with farm animals and for many years has been conducting studies which he testified about today. So along with other witnesses we have had we are very appreciative of his testimony.

Now we have the concluding part of our hearing today. We are going to have a roundtable on the fallout mechanisms and uptake in plants, soils, animals, and people, to try to bring this into focus. The participants will be Dr. Wright Langham, Dr. William Neuman, Dr. C. L. Comar, Dr. Robert Reitemeier, Dr. Arthur Wolff, and Dr. Harry A. Claypool.

Gentlemen, will you take your places at the panel table and proceed?

To start off, the discrimination factors are applied as averages in evaluating the relationship between strontium 90 in the soil and the diet of people. Is it possible from present information to estimate the maximum or minimum values that might occur in individual cases?

ROUNDTABLE ON FALLOUT MECHANISMS AND UPTAKE IN PLANTS, SOILS, ANIMALS, AND PEOPLE—PARTICIPANTS: DR. WRIGHT LANGHAM, DR. WILLIAM NEUMAN, DR. C. L. COMAR, DR. ROBERT REITEMEIER, DR. ARTHUR WOLFF, AND DR. HARRY A. CLAYPOOL

Dr. COMAR. I don't think, sir, it is possible to really evaluate maximum and minimum value. From our own experience, as I have said, we have been impressed with the constancy under conditions of a normal diet. If a person has an abnormal diet, the discrimination factor may vary. But from values of our own, values from other laboratories in this country, and from values from abroad, we all seem to be coming out with similar results. I would like to hear any comment on this point.

Representative HOLIFIELD. Does anybody else care to volunteer on this point?

Dr. NEUMAN. I would like to say that this is an attempt to apply statistics to the individual case. It is impossible for a scientist to take something that is drawn from a population and apply it to an individual.

Representative HOLIFIELD. Let us trace this discrimination factor, if we can, in meaningful words. Let us take 100 strontium units in the feed to cattle and trace it down to a discrimination factor to the child going through the milk channel, to the adult, and milk channel to the child. If you can, give us some percentages for the amount that is eliminated from the hundred units taken in by the cow from the feed or the foliage that it eats.

Dr. COMAR. I would prefer to do this first in terms of the ratios because if one talks about amounts eliminated it becomes very difficult. This is one reason for using the ratio.

Representative HOLIFIELD. Could you give us an illustration on the board, Dr. Comar? This seems to be a matter of confusion among some of the members of the committee.

Dr. COMAR. Yes, sir. If in the cow we had 100 units of strontium for every hundred units of calcium—or rather in the cow's diet—then we would expect in the milk from that animal to find about 12 units strontium for every 100 units of calcium. I think that this figure is quite firm. It has been confirmed by independent methods.

Now if a person, say man, consumes this particular milk, then we have to use a range of discrimination. If we assume that the discrimination from man's food to his body is only a factor of 0.5, then in the body of this individual we would find 6 units of strontium for every 100 units of calcium. On the other hand, if we use a factor of 0.25—and I have suggested that we think that the real value would probably fall somewhere within 0.5 and 0.25—then we would expect only 3 units of strontium for every hundred units of calcium. So if the feed that the cow is eating contained 100 of strontium to 100 of calcium, then in the bone of the man we would expect the ratio to be 3 to 6 units of strontium for every 100 units of calcium.

Representative HOLIFIELD. When you use the term "man" there, do you wish to differentiate a man, where the skeletal structure is complete, and a child? Would it be higher in the child than the figures you have given, or is that an average?

Dr. COMAR. This brings up a very important point that I would like to comment on, if I may. If we consider this [pointing to diagram] to be the skeleton or the bone of a young child and this to be the bone of an adult; if this child is making all of its bone from the strontium and calcium that is coming in, then the entire bone will very soon reach equilibrium and will have a given value as compared to the strontium and calcium that he is eating.

In man only a small part of the bone is being metabolically active in this sense. But I would remind you that the strontium-calcium ratio in that part that is metabolically active would be essentially the same as the ratio in the bone of the young child. So the strontium-calcium ratio here [entire child's skeleton] would be the same as here [part of man's skeleton]. When one takes a sample—if you were to analyze the whole bone—then obviously since there is dilution by all of this stable calcium, the overall effect would be that you would get a lower result, and this is what happens. I would like to raise a question, that is, if we are considering the biological response in these systems, is the biological response in man dependent upon this concentration in this area of new bone, or is it dependent upon the concentration in the whole bone? In any event, we feel that the concentration in this newly forming bone could not be greater than the concentration in the new bone of the child. So we feel if we look at children's bones, then we are looking at the situation at its worst from this standpoint and will be on safe ground as far as the adult is concerned.

Representative HOLIFIELD. Of course, our concern is primarily with the child and in relation to the pounds of bone in the child's skeleton, and the pounds of bone in the man's skeleton, your percentage is much higher in the child's skeleton.

Dr. COMAR. Exactly. Because all of this calcium was laid down before the contamination started.

Representative HOLIFIELD. Yes. Are there any among the witnesses who wish to comment on this or elaborate on it? Is it the general consensus of the witnesses that this has been fairly stated in view of the knowledge we have at this time?

Dr. LANGHAM. Mr. Holifield, the situation that Dr. Comar just explained is why we insist, in talking of extrapolating bone levels to the point of maximum. That is what we mean when we use the word "equilibrium." What Dr. Comar has just mentioned is that the child is more nearly apt to be in equilibrium throughout his bone than the adult is and that is why in estimating these hazards we extrapolate to equilibrium values. This extrapolation takes children into consideration. I noticed you were quite concerned about that before lunch.

Representative HOLIFIELD. Yes. Are there any further comments on that point? If not, some foods are reported both in strontium units, micromicrocuries of strontium 90 per gram of calcium, and in micromicrocuries of strontium 90 in relation to a gram of food. What is the practical meaning of these two methods of reporting, and the use of discrimination factors, and in interpreting these values in terms of permissible concentration?

Dr. COMAR. I hesitate to monopolize, Mr. Chairman.

Representative HOLIFIELD. You are doing so by invitation. Go right ahead.

Dr. COMAR. One of the main advantages of expresing the relationship of strontium to calcium is as I have just indicated, that you end up with a value that tells you what is going on in the active part of the new bone, so this may be very important. The other is that because of the peculiarities of calcium uptake, it is very hard to get a measure of actual strontium retention. For example if we consider an adult after 20 years of age, he has all of his bone formed already, and so he may eat a gram of calcium a day but over the years he will eliminate one gram of calcium a day. So there is no net retention of calcium over the 40 or 50 years of this individual's life. It becomes very hard, then, to say if a certain amount of strontium is taken in how much strontium will be put out and what will be the retained figure. For this reason there is value in expressing results as a strontium to calcium ratio.

On the other hand, I think it is very important that we have in our data knowledge of absolute levels and amounts of strontium. So I feel that all the data should be taken with enough analytical results so that we can go back and calculate the amounts if we should ever need them.

Representative HOLIFIELD. The maximum permissible concentration, then—its overall application—would be all food intake. The attempt to find where that concentration originated would go into the consideration of milk, potatoes, and different items of food.

Dr. COMAR. Yes. Perhaps I can illustrate the convenience of this procedure. If we know that a maximum permissible level has been set for body burden—if we say that the body burden permissible level is 100 micromicrocuries of strontium, for example, or I guess it should be 1 micromicrocurie of strontium—then knowing that the skeleton contains roughly a thousand grams of calcium, one can find out what the level would have to be in the diet to develop this particular level in the body, and it becomes a fairly easy thing to do on this basis.

Representative HOLIFIELD. In discussing the difference between food and milk, could I pin it down to this question? Is there a different discrimination factor in milk than there is in other items of food? I am not talking about the quantities absorbed but I am talking about the factor of discrimination.

Dr. COMAR. As I indicated before, in some work with rats we have found a definite difference between animals on a milk diet and animals on a non-milk diet. In the limited work that we have done with human beings, we have not been able to see this difference. I cannot give you a more precise answer than that.

Representative HOLIFIELD. Would there be any member of the panel that wishes to comment on that point?

Dr. NEUMAN. I agree it is unsettled, but the uncertainty involves only a factor of two.

Representative HOLIFIELD. It is an approximate factor of two?

Dr. NEUMAN. There is only a factor of two between the two results that have been reported. If anything, I think we should rely on the human data more heavily than the rat data because I feel that a milk diet for a rat is rather unusual.

Senator AIKEN. How old rats do you use for this experiments?

Dr. COMAR. We have used various ages. One is either interested in the young growing rat or the mature rate. Depending on the

experiment we have used various ages. In the early work we actually started with pregnant females and carried them on a strontium 90 diet through two generations, so we have results at all ages.

Representative HOLIFIELD. Dr. Langham, I will address this question to you. You indicated that you expect the bone levels in Far Eastern peoples to be higher than those of Western peoples. These estimates are based on human bone analyses. Is it not possible that the high concentration of strontium 90 found in Eastern peoples might be due to foliar adsorption of strontium 90?

Dr. LANGHAM. It is certainly possible that it could be due to foliar absorption which could be responsible for the high levels in the diet. In making the calculations I have given you, the average values have been diet weighted. Assuming that the diet of the Far Eastern peoples has cereals as the principal source of calcium, that being the case, one must consider the discrimination factors involved. A person drawing his calcium from cereal sources, in which there was no milk incorporated in the diet would have a high calcium content in his bone because of the greater discrimiation via the milk cycle. This has been recognized, and in giving these estimates of population average bone values the nature of the diet has been considered. That is why the predicted values show that Western countries may have a maximum level of 5 micromicrocuries per gram of calcium and the rice diet peoples may have 10. It does not matter whether this is coming from foliate absorption or whether it is coming through the root system of the plant. What it means is primarily if one takes into consideration discrimination factors in going from diet to bone, regardless of what the source of strontium is, whether it is direct foliate or root absorption, the Eastern countries will run higher.

Representative HOLIFIELD. If foliate absorption is an important factor, then, how would this affect the assessment of a biomedical hazard for a large scale nuclear war, or is this getting too far afield for an estimate at this time?

Dr. LANGHAM. In reality we have got to assume that the levels, whether they are coming from foliate absorption or from the soil, are in some respect directly proportional to the integrated fallout. In that case it does not make any difference what the source is, because we saying if the fallout is doubled, then the foliate absorption will be doubled, the diet will be doubled, and so will the concentration in the bone.

Representative HOLIFIELD. Of course, for people eating a vegetarian diet, they would be exposed to more than people theoretically who have larger percentages of meat and milk in their diet, because of the discrimination factor in the animal products area of their diet.

Dr. LANGHAM. That is right. One can usually assume that the concentration in the bone at equilibrium will probably be about 25 percent of the concentration of the strontium 90 in the calcium of the diet. If the strontium 90 comes from direct fallout, as may be the case in a nuclear war in which the concentrations in the diet are increased greatly, then the concentrations in the bones of the people will increase also. But there is one thing about this, sir, that I would like to explain, that may have something to do with the situation in the event of war. It applies also perhaps to the Minnesota wheat question. That is, if an individual's diet is essentially constant in stron-

tium 90 and then, due to direct fallout the diet goes very high for a few weeks or even a month or so, and then (because it was direct fallout and not uptake from the soil) return to something more or less like what it was before the peak, people's bones will not have time to come into equilibrium with the higher dietary level. The strontium 90 bone concentration will level off, depending on the width of this peak at some position a little higher than it was before but not at the maximum indicated by the diet. In other words, this hot spot fallout problem, high levels in Minnesota wheat, high levels in this food or that food, when it is caused by direct fallout, if transient is not nearly as serious as the measured values would indicate it is. Because, if the dietary level goes back down after a reasonable length of time, people's bones will not have had time to come into equilibrium with the higher dietary level.

Representative HOLIFIELD. So a general statement would be that any type of fallout which would be long lasting enough to become part of the muscle or the bone structure of the body would be of less importance than the violence of the short-lived beta and gamma emitting isotopes, which would attack externally, and do other types of damage, rather than residual deposit in bone or muscle?

Dr. LANGHAM. You are talking in terms of nuclear war.

Representative HOLIFIELD. Yes, the same situation you described. The immediate danger would be from gamma rays.

Dr. LANGHAM. Yes. It would be my feeling definitely that in the event of war our problem is going to be the immediate fallout problem and the external exposure of the population. That does not mean that these long lived isotopes will not be of secondary importance, but I think definitely they will be secondary.

Representative HOLIFIELD. If the deposit of these longer lived isotopes is heavy, there will be a general buildup in the years to follow?

Dr. LANGHAM. That is right, yes.

Representative HOLIFIELD. Of the the long lived deposit in the bone?

Dr. LANGHAM. With regard to strontium 90; yes, sir.

Representative HOLIFIELD. Is there any comment on that?

Dr. COMAR. I would like to make one comment: Are we quite sure again whether any deleterious effect would be dependent upon the local concentration in the bone? If this were so, then would we get a peaking at a small area? I think that this is still an open question.

Dr. LANGHAM. I assume that Dr. Comar is referring to a problem that has plagued us for some time, and that is, is the biological effect that we expect to see dependent on the dose received by the hottest spot in an individual's bones or on the average dose to the entire skeleton. Is it worse to irradiate a small bit of one's bone with a high radiation level than all of the bone with a low radiation level?

Representative HOLIFIELD. Is that correctly stated, Dr. Comar?

Dr. COMAR. Yes. This is the fundamental point.

Dr. LANGHAM. Certainly I think we could all agree that this is a problem that we have had lots of interesting evenings arguing about and there is no need of bothering this committee with the argument. We certainly have not settled this question yet.

Representative HOLIFIELD. I understand that there is a complete book written on this subject by a Swedish author or a group of Swedish authors interested in this subject.

Dr. LANGHAM. Yes, I am very well acquainted with that.

Representative HOLIFIELD. Mr. Ramey has a question.

Mr. RAMEY. This one might be perhaps to Dr. Claypool or Dr. Langham. How much research work has been done on iodine 131 absorption in the thyroid of children and adults? We have had testimony for the last 2 days now that the faster fallout rate makes these short-lived isotopes more important. Dr. E. B. Lewis of Cal Tech has indicated that the selective absorption of iodine 131 may concentrate radioactivity to higher levels. (See statement by Dr. E. B. Lewis, p. 1552.)

Dr. CLAYPOOL. We have been doing none of this work locally at Walter Reed. Some work on urinalysis of individuals who were located at Mercury, Nev., during a test season was done and small levels of concentration of iodine 131 were found in the urine of these individuals. Beyond that, I will have to pass the question to Dr. Langham.

Dr. WOLFE. I would like to comment on that, if I may. There has been quite a bit of work done during the past several years on the levels of iodine 131 in the thyroids of grazing animals throughout the country. So we have a fairly good record as to what the levels of environmental contamination have been insofar as iodine 131 is concerned. We are generally agreed that strontium 90, as an environmental contaminant, certainly deserves primary attention, because this problem will persist for many years following the cessation of nuclear weapons testing. But I think data collected during the past few years does indicate that some of the shorter lived isotopes, particularly iodine 131, are not necessarily insignificant just because they are short lived. There is evidence to indicate that relatively high levels of iodine 131 have periodically been present in the biosphere.

The Public Health Service has taken this into consideration in its milk surveillance program, and we have assayed for iodine 131 as well as other short lived isotopes, and we have found, though most probably not hazardous levels, certainly levels sufficiently high to indicate that while weapons tests are in progress—we should consider these short lives isotopes for their contribution to the total dosage. I believe Dr. E. B. Lewis has submitted a statement for the record giving his assessment of the possible hazard and dosages associated with the drinking of milk containing the levels of iodine 131 that the Public Health Service has reported in its milk surveillance program. It can be estimated that during the past several years the average dose to the thyroids of infants and children probably has been about equivalent to background—roughly 100 to 200 millier per year. It is particularly important to consider the younger age groups because the proportion of milk consumed relative to the weight of the thyroid gland is considerably higher than in the case of adults and therefore the thyroid dose to infants and children would be considerably greater than adults. Almost all of the work done in the past on the evaluation of iodine 131 in humans has been based on urine examinations and the autopsy specimens of adults.

Representative HOLIFIELD. Let the staff read to you at this time a summary on this very point which seems to agree with what you say.

Mr. RAMEY. I think he was going to provide a statement for the record. This is a summary of his paper that is in process.

Mr. WOLFF. This is Dr. Lewis?

Mr. RAMEY. Yes.

On the basis of published data on iodine 131 levels during a 16-month period ending September 1958, the thyroid glands of average infants and children in the United States are estimated to have experienced over the last few years annual doses of about one-tenth rad to two-tenths rad from the radio iodine fallout. These doses are roughly one to two times the annual dose to such organs from natural background radiation. For a number of reasons individual thyroid dose rates from fallout are expected to show wide deviations from the average rate.

The reason for asking about the research on this, from a layman's sort of fashion, was that these were more or less deduced rates from how much fallout there was rather than from any counting from whole-body counters or any other measurements on actually what was in the thyroid of children.

Dr. WOLFF. Yes, sir. These are calculated dosages but they are deduced on the basis of our milk data. We have reliable data with respect to the levels of iodine 131 in milk from our various milk sampling stations. In table 8 of Dr. Straub's report, which was submitted for the record, you will find a monthly summary of the iodine 131 levels in milk for the various milk stations. It gives a month-by-month breakdown of these levels. During the last 16 to 18 months an overall average in the order of 60 micromicrocuries per liter of milk was detected. There were peak values as high as 900 micromicrocuries per liter in some of our stations during a particular month.

Representative HOLIFIELD. Now we are going to ask you a question based on what Senator Aiken asked a few minutes ago, when he asked you if there was a pill that you could take that would get rid of radiation. I will ask you it in this form: Is there any reason to believe from biomedical information presently available that by simple or practical means discrimination in a human could be increased? I notice the readiness to pass the microphone to you, Dr. Comar.

Dr. COMAR. My answer would be "No," sir.

Dr. WOLFF. I think this might be feasible with iodine. One might discriminate against radioactive iodine by simultaneously taking stable iodine. This procedure could reduce the intake of radio-iodine in the human.

Representative HOLIFIELD. Is there any any further comment on that point?

Dr. NEUMAN. I agree with Dr. Comar, but there is such a chorus of agreement this time compared to the last hearings that I would like to go back, if I might, to Dr. Langham's remark, and take issue with him on his predictions for the Far Eastern populations. If foliate absorption is an important cause of the present bone levels of Far Eastern people and children, then this is a dose rate dependent phenomenon—that is, it is dependent on the rate of fallout. In the event that there is no more fallout, then there is no more uptake and our equilibrium calculations and predictions for the maximum uptake in the 1960's would not give high levels in the eastern population

of bone, but possibly even lower than the general average we have calculated.

Representative HOLIFIELD. Dr. Langham, would you care to comment on that?

Dr. LANGHAM. Yes. I am afraid I oversimplified the problem. This is certainly right. If foliate absorption—or, for that matter, if anything is, as foliate absorption would be, dependent on fallout rate, then the minute we quit testing the rate of fallout will decrease and so will the levels in bone. I think the best solution to all this is to agree with Mr. Durham.

Representative HOLIFIELD. Not to have any fallout. Are there any further comment on that? If not, is there any new information available on soil to plant discrimination? I will ask Dr. Reitemeier on that.

Dr. REITEMEIER. With regard to strontium and calcium, as I indicated previously, I believe the overriding factor is not the so-called simple discrimination factor, but the actual situation in the soil profile in the particular field situation. That is, what is the relative location and distribution of the strontium 90 in the soil profile? What is the specific distribution of calcium in the soil profile? This will vary tremendously between different soils, different crops, and management practices. So I feel that it is virtually impossible to assign a single discrimination factor to plants as they are grown actually in the field. For the lack of a better value than 1, we are usually inclined to say the overall value is 1. But it can range upward or downward from 1. Averaging all of our data, if it comes out to 1, that does not mean that this is always the value. It certainly will vary upward and downward.

Representative HOLIFIELD. Are there any further comments on that? If not, this morning I questioned Dr. Libby, I believe, on the high values observed in the Minnesota wheat and in the so-called hot spots or hot areas. If I understand him, he stated that he believed this resulted from more foliate absorption than anything else. Do you concur with this evaluation?

Dr. LANGHAM. Yes; I just can't see any other explanation for it, because if it were coming in through the root system, then calcium in Minnesota soil would have to be fantastically low or the strontium 90 values in the Minnesota soil extremely high, and neither of these seems to be the case.

Representative HOLIFIELD. In other words, the rainfall that occurred at that particular time in that area undoubtedly deposited fallout upon the foliage.

Dr. LANGHAM. Yes.

Representative HOLIFIELD. This question I also will address to Dr. Langham. Do you believe the cesium 137 to strontium 90 ratio which you have given in your report to be approximately 1.8, corresponding to relative fission yield, will remain stable in the biosphere; or is it possible that this ratio will change with passing time?

Dr. LANGHAM. We can't even say with certainty that fractionation has not already occurred. In other words, 1.8 is the ratio one would expect if no fractionation had occurred at the time of detonation or during diffusion through the stratosphere and troposphere.

Representative HOLIFIELD. And in the event no further testing occurs.

Dr. LANGHAM. Yes. There are indications of fractionation with altitude, also. I can only say what Dr. Libby has said on a number of occasions; and that is, if one looks at the data—good, bad, or indifferent—and considers the average, the ratio is tending to be the expected average from fission product production. With respect to fractionation with time, I would rather not say what will happen. It is just that right now the indication seems to be that fractionation may be occurring but the best number we can give is to assume the average ratio of production.

Representative HOLIFIELD. Will you define "fraction" for the record?

Dr. LANGHAM. I mean by "fractionation" that the strontium and cesium get separated from each other by one method or another. Right now we have no better values to rely on than to say that the ratio is approximately the ratio of production.

Representative HOLIFIELD. Is there any comment on that point? If not, this morning, Dr. Langham, you indicated that the equilibrium value of children's bones could be taken as two micromicrocuries per gram of calcium. How does one deduce an equilibrium value in a mixed population during a period of changing strontium levels?

Dr. LANGHAM. What one has to do is use bone data, of which we have quite a bit, and normalize these either to the level in fetal bone, which Dr. Comar says has a discrimination factor of 2, or normalize to the level in adult bone which is age independent. If we accept his discrimination factor of 2, and we take all the fetal bone analyses we have, we may say that the equilibrium level in bone would be twice that observed for fetal bone. Then we can test this theory by adjusting for the rate of fallout and the rate of growth that occurs with time—the rate at which the child lays down its skeletal calcium as it gets older—we can test our hypothesis to see if the bone data for all ages—all of the bone data fit the theoretical points or the calculated points when normalized to the fetal bones, assumed to have a discrimination factor of 2. When we do this, we are amazed to find that we can, on this assumption, make bone data from any age group fit essentially where we calculated it on the basis of the bone growth model. One can also do it the other way and assume certain discrimination factors and work back from adult bone data and make the points for all age groups fit the growth model. It is a matter of fitting the observed data to a model of bone growth plus the fallout rate.

Representative HOLIFIELD. I have been informed that Dr. Alice Stewart of the United Kingdom has done pelvimetric X-rays as low as 3 roentgens given in the eighth month of pregnancy, and it doubles the cancer rate of children between birth and 10 years. Is there any comment you want to make on that? I recognize this is more for tomorrow but as long as we have been talking about the fetus, I thought I might ask if anybody here wants to comment on it.

(No response.)

Representative HOLIFIELD. We will get to the witnesses tomorrow, and we will put this on their shoulders tomorrow.

If there are no further questions, Dr. Claypool, would you have any particular comment to make on any of the testimony today?

Dr. CLAYPOOL. No, sir. I came here primarily because of the Walter Reed whole body counter. We have been counting people there, human beings. Our counter is designed after the Los Alamos human counter and we have been concerned primarily with cesium 137. Since nothing directly came up as concerns cesium 137 during the roundtable, I did not have any comment.

Representative HOLIFIELD. Why don't you make a short statement on that? We have been more concerned with the strontium 90. If you care to make a short statement on cesium 137, you may proceed.

Dr. CLAYPOOL. I surely thank you. We became fully operational on the 14th of July last year. We have been fully operational for approximately 9 months. During that period we have counted a total of 1,278 essentially normal people, but by selection criteria we chose 800 of these and prepared our data and compared our data with those of the Los Alamos counter. I think the best news I bring here is that we seem to agree unusually well with the data obtained from the Los Alamos counter. So unusually well it almost looked like collusion in view of the fact that the two counters are 2,000 miles apart horizontally, and a mile apart vertically. So I don't bring any new data, particularly, but I wish to support the data that Los Alamos has presented.

Representative HOLIFIELD. Thank you very much. Would any member of the panel like to comment on any testimony that has been given here today, either critically, modifying, or clarifying? I see no takers.

Thank you, gentlemen, for your help today, and the committee is deeply appreciative of your assistance.

Tomorrow at 10 a.m., we take up new developments in radiation biology, somatic effects, and the genetic effects. In the afternoon we will discuss the basis of permissible exposure levels and have a roundtable on that subject. Following that, we take up the future implications of testing and have a roundtable on that subject. I invite you back.

I have a statement by Dr. Wolff which I will place in the record at this point, also three papers submitted by Dr. Claypool and Walter Reed Army Institute of Research.

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(The statement referred to follows:)

FALLOUT AND UPTAKE OF IODINE-131

Arthur H. Wolff*

Generally, in considering the uptake mechanism for fallout, almost exclusive attention has been given to strontium-90 and to a lesser extent cesium 137. The problem of these radionuclides as environmental contaminants certainly deserves our primary attention because the problem will persist for years following the cessation of nuclear weapons testing. Even though strontium-90 is the isotope of major significance, the potential hazard of some of the shorter lived isotopes should not be overlooked. Some of the other fission product isotopes, just because they are short lived, are not necessarily insignificant; they may present a contamination problem if they occur in the biosphere at relatively high levels.

Of particular importance in this regard is iodine-131, a fission product radioisotope with a half life of approximately eight days. Iodine is unique among the fission produced radioisotopes in that it concentrates in a very small gland, the thyroid. Consequently, extremely small amounts of the isotope taken into the animal body will result in relatively high dosages to a single gland as compared to equal amounts of other radioisotopes more widely distributed in the body.

As a result of its eight-day half life the concentration of iodine-131 in a gross fission product mixture gradually diminishes and the amount of

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iodine-131 reaches negligible proportions several weeks to months following its creation. It is likely, therefore, that any iodine-131 in the biosphere results primarily from rather fresh fallout.

Several investigators have measured the iodine-131 levels in the thyroid glands of grazing animals as an index of fresh fallout in various parts of the world. The most work in this field has been done by Dr. Lester van Middlesworth, University of Tennessee, Memphis. Since 1954 he has collected thyroid glands from animals in slaughter houses from various countries and particularly from Tennessee in our country. Based on his data it would appear that from 1955-1958 the average concentrations sustained in U. S. cattle and sheep were in the order of 700 and 3000 μμc per gram of thyroid respectively. The average weekly dose has been about 50 millirad per week for cattle and 210 millirad per week for sheep. These concentrations are average values; considerable fluctuations occurred depending on the type and location of weapons testing in progress just prior to the time of collection. Other investigators 1/ have found similar levels in other parts of the United States.

It is apparent that despite its short half life iodine-131 has been readily detectable as a biospheric contaminant reflecting current weapons tests. The levels found are quite likely innocuous insofar as the health

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1/ Gunther, R. L. and Jones, H. B., University of California Radiation Laboratory Report (UCRL-2689) Berkeley 1954

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of the animals is concerned. However, iodine-131 as a biospherical contaminant, probably has resulted in a higher dosage level to a limited volume of tissue than any other fission product isotope.

The levels of iodine-131 in grazing animals are considerably higher than any levels concurrently reported in humans because grazing animals consume large quantities of foliage upon which fallout and rainout were directly deposited.

The values in cattle are of particular interest because, theoretically, we should also expect detectable quantities of iodine-131 to be secreted into the milk and consequently higher levels of iodine 131 to occur in the thyroids of infants and children than in adults. The Public Health Service has taken this into consideration and has included iodine-131 as well as certain other short-lived radionuclides in its milk surveillance program.

Summarized below are the average iodine-131 levels detected in milk from the various sampling stations.

Average Iodine-131 Concentrations in
Micromicrocuries per Liter Based on Monthly Sampling

Milk Shed	June 1957 - April 1958 Average Level ($\mu\mu c/l$)	May 1958 - Jan. 1959 Average Level ($\mu\mu c/l$)
Cincinnati	136	41
New York	79	31
Sacramento	30	40
Salt Lake City	249	33
St. Louis	258	99
Atlanta	First Sample, June '58	22
Austin	" " June '58	39
Chicago	" " July '58	38
Fargo	" " June '58	38
Spokane	" " Aug. '58	32

I believe that Dr. E. B. Lewis, California Institute of Technology, is submitting a statement for the record which estimates the human dosages

and possible biological significance that would be associated with consumption of milk containing the levels of iodine-131 reported here.

Iodine-131 levels in cattle thyroid may provide a presumptive index of the levels being secreted into milk in the same area. This is one parameter which the Public Health Service hopes to investigate in its future work in radiation surveillance of the environment.

The technique for measuring iodine-131 levels in thyroid glands is relatively simple. Perhaps greater reliance should be given to this technique for it may serve, not only as an early and sensitive index of the biological incorporation of iodine-131 but also may provide an index of the biological accumulation of other fission products. Iodine-131 as an index of environmental contamination, however, is only applicable to fission products of recent origin.

Contemporary Levels of Cesium^{137} in Man

Submitted by the Division of Nuclear Medicine, Walter Reed Army Institute of Research, Development of Defense.

INTRODUCTION

The weapons test program and contemplated nuclear power developments within the next several years have forcefully shown the need for an extensive biomedical monitoring network for people and foodstuffs. The Division of Nuclear Medicine, Walter Reed Army Institute of Research has been engaged in evaluating excretion levels and body burden of certain fission fragments in humans since early 1954.

BIOLOGICAL EXCRETION STUDIES

During weapons testing at the Nevada Test Site in 1955 (Operation TEAPOT) weekly monitoring of urine samples from personnel throughout the United States and from many overseas sites was carried out for the test period. Excretion levels of iodine^{131}, cesium^{137}, cesium^{144}, and strontium^{90} were determined. The radioactive air mass from nominal and low yield weapons seldom exceeds 42,000 feet and the passage of the air mass over a monitored population generally resulted in an increase in iodine^{131} urinary excretion (1). For high yield weapons detonated at the Pacific Test Site during 1957 (Operation REDWING) urine levels of iodine^{131} were consistently detectable. There was a general rise in the excretion levels of cesium^{137} toward the end of the weapons test period. Over 7500 individual urine samples were processed during the TEAPOT - REDWING Operations. The mean excretion levels of cesium^{137} in the northern hemisphere indicated that body burden would be about 0.002 microcuries. Experiences with monitoring human excretion levels

have been summarized and presented to the Special Subcommittee on Radiation of the Joint Congressional Committee (2), the Army Science Symposium (3) and the Symposium on the Shorter-Term Biological Hazards of a Fallout Field (4).

DIRECT HUMAN MEASUREMENTS

Because of the inherent difficulties in low level assays of large numbers of bulk biological samples and the uncertainties in estimating total body burden from excretion levels, a facility for determining gamma ray activity directly in humans was constructed. The design of the facility emulated the counter at Argonne National Laboratory and at Los Alamos Scientific Laboratory and incorporated the experiences and techniques of whole body counting evolved by Miller, Marinelli, and Anderson. The facility was sponsored jointly by The Surgeon General, U. S. Army and The Armed Forces Special Weapons Project.

Since June of 1958, 1100 people have been processed at the Walter Reed Army Institute of Research for cesium^{137} body burden in the Human Counter. The crystal spectrometer has been used for selected cases to confirm the presence of cesium^{137} and to determine the levels of various gamma emitters in humans from reactor exposures and clinical diagnostic procedures.

Of the people assayed for cesium^{137} body burden during the period July - December 1958, 474 subjects residing in the United States had a negative medical history and no known exposure to radioactive materials. The mean age of the group was 31 years (sigma = 32%) and a frequency distribution of body weight as shown in Figure 1.

The Human Counter is operated as a two channel analyzer simultaneously determining cesium^{137} and potassium^{40}. Naturally occurring potassium^{40} is in most cases the most abundant gamma emitting radioisotope in the human body and is a direct measure of "fat-free" mass or muscle mass. Cesium^{137} is an abundant fission product and its behavior in the biosphere should be quite similar to potassium. Anderson has shown that the major source of potassium and cesium^{137} in the American diet is milk and dairy products. Figure 2 shows the quantitative dietary intake of milk and dairy products for the U. S. group with an average amount considered to be 1 or 2 servings of milk, cheese, or butter per day.

Figure 3 shows the frequency distribution of cesium^{137} in normal U. S. subjects measured during 1958. The histogram fits a Gaussian distribution and the average disintegration rate is 1.66 gamma rays per second per pound of body weight. This equals 0.007 microcuries body burden for a 159 pound subject. For non-industrial exposure the average body burden is less than 1/1000 of the recommended lifetime exposure of 9 uc.

Since cesium^{137} body burden would be expected to vary with the body content of potassium it is pertinent to express cesium^{137} results as the ratio (Cs/K) in micromicrocuries per gram. Figure 4 shows the cesium^{137} levels in people according to geographic location of residence. The U. S. average during the latter half of 1958 was 70 uuc/gm K. Summarized in Tables 1 - 4 are results for people residing outside the continental U. S. The mean cesium^{137} level for Germany - France was

76 uuc/gm and for Japan 72 uuc/gm. There are too few people from other areas to be considered indicative. It is of interest to note, however, the Brazilian and Iranian points.

RECOMMENDATIONS FOR FUTURE RESEARCH

The Human Counter Facility at the Walter Reed Army Institute of Research will continue to measure the levels of gamma emitting isotopes utilizing the methods described above. A minimum of 1500 persons a year will be studied and in addition, "controls" will be following weekly to determine changes in the isotope levels in the Washington area with time.

In June 1959 a Human Counter Facility will be activated by the U. S. Army Medical Research Unit Number 1 at Landstuhl, Germany. The counter was built by Dr. Ernest Anderson, Los Alamos Scientific Laboratory for the 1958 Geneva meeting and was turned over to the Army. Data from this counter will be processed using the same IBM forms as those at WRAIR. This will result in the capability of determining gamma emitting isotopes directly in man at Los Alamos, New Mexico, Washington, D. C. and Landstuhl, Germany.

It is becoming apparent to medical radiobioligists that the only certain way of establishing levels of fallout radioactivity in man is through the direct measurement of man himself. This obviously avoids the inherent difficulties in sticky paper, pot and other methods of collection and avoids concern over discriminating factors from animal and agriculture products to man. In addition, it avoids the requirement of examining uniform distribution of the particular isotope

throughout the body as is necessary when single bones or other parts of the anatomy are surgically removed and analyzed for radioactivity. It is recommended that effort be undertaken to construct counters for use in the Far East and in the southern hemisphere. Data presented in the paragraph above indicates there is a real difference in levels of activity in the few individuals measured from the southern hemisphere. If at all possible these new counters should be mobile. In appropriate areas of the world they could be transported on railroad cars. In addition, currently plans are under way to convert an United States Navy hospital ship for a trip throughout southeast Asia. This will be staffed with civilian physicians and health workers who will provide medical care as requested by the countries of this geographical area. A counter designed for this ship would be most useful from a clinical standpoint and could gather information on the levels of radioactivity in people of this area of the world. It is estimated that less than $200,000 would be required to finance these two additional counters. Data obtained from the five counters would then provide for the first time a direct worldwide coverage of radioactivity.

The state of the art of measuring radioactivity in man has been developed to the point where it is theoretically possible to measure strontium^{90} utilizing external counting techniques. The levels of this isotope in the world's people have increased to the point where the Bremstrahlung can be detected. It is recommended that appropriate development contracts with government agencies or industry could result

in a suitable counter within the next two years. This is obviously a high priority item as it is the only method that holds promise of establishing the actual levels of strontium in man. In our opinion current data is accurate only within an order of magnitude; therefore, biological variation is so great that methods must be devised to determine the actual levels of activity in individuals.

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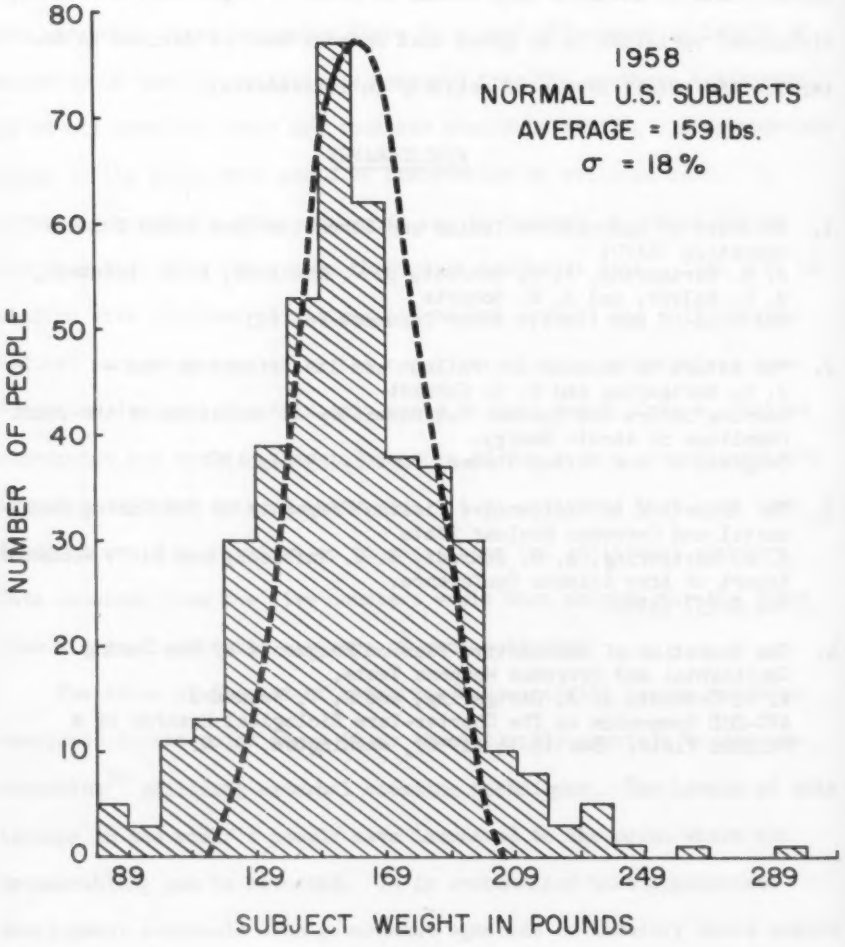

1958
NORMAL U.S. SUBJECTS
AVERAGE = 159 lbs.
σ = 18 %
NUMBER OF PEOPLE
0
10
20
30
40
50
60
70
80
89
129
169
209
249
289
SUBJECT WEIGHT IN POUNDS

Fig. 1

THE INTAKE OF MILK-DAIRY PRODUCTS OF NORMAL U. S. SUBJECTS MEASURED DURING 1958

Large Amount	24.9%
Average Amount	61.8%
Small Amount	12.1%
None	1.3%

Fig. 2

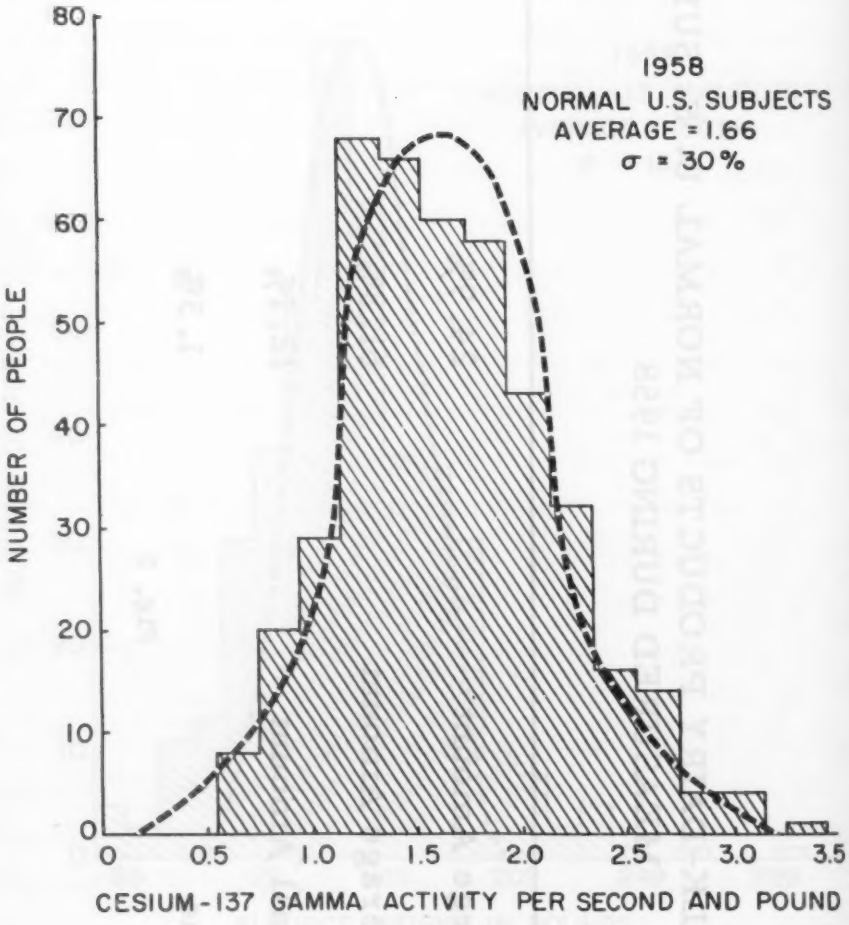
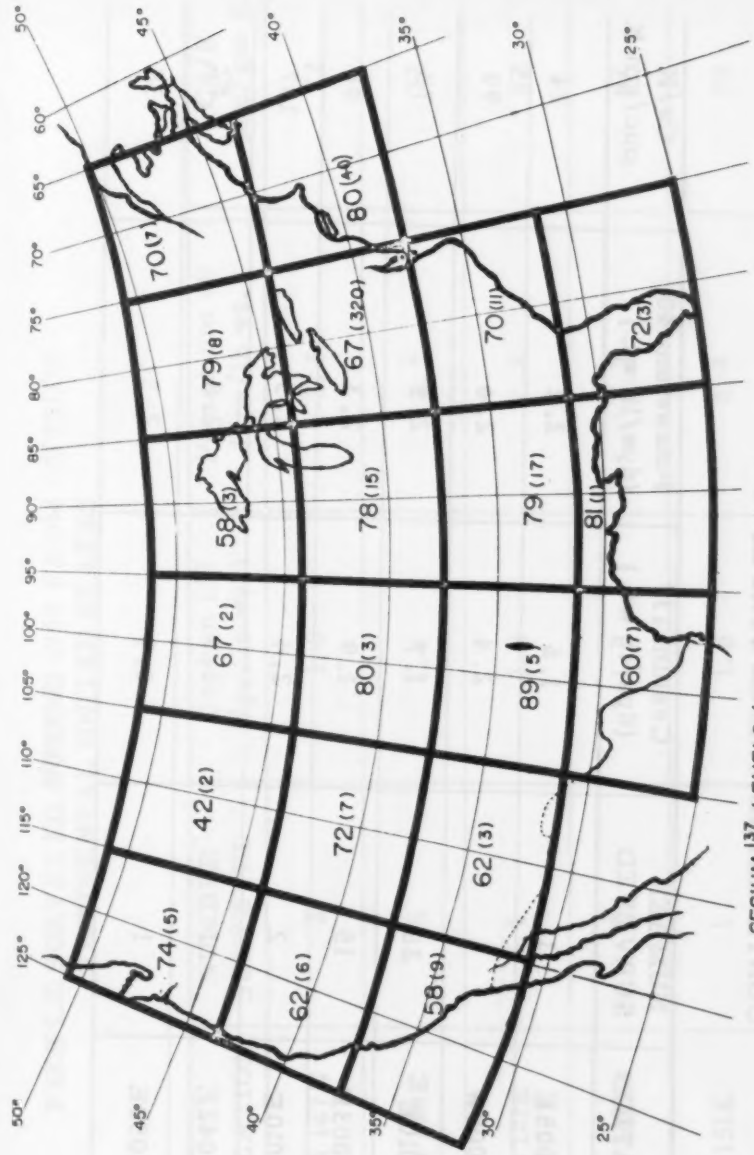

1958
NORMAL U.S. SUBJECTS
AVERAGE = 1.66
σ = 30%
NUMBER OF PEOPLE
0
10
20
30
40
50
60
70
80
0
0.5
1.0
1.5
2.0
2.5
3.0
3.5
CESIUM-137 GAMMA ACTIVITY PER SECOND AND POUND

Fig. 3



CESIUM ^{137} LEVELS ($\mu\mu c/gmK$) IN NORMAL U.S. SUBJECTS DURING 1958.

Fig. 4

PEOPLE SURVEYED DURING 1958 FROM OUTSIDE CONTINENTAL UNITED STATES

COUNTRY	LOCATION	NUMBER SURVEYED	Cesium 137 (dps/lb wt.)	Potassium 40 (dps/lb wt.)	Cs/K μμc/gm K
Holland	52N 005E	1	1.6	2.2	74
England	52N 000W	1	2.4	2.4	99
Germany	51N 010E	28	1.7	2.5	72
France	47N 003E	15	1.9	2.3	86
Italy	43N 010E	2	2.4	2.3	107
Turkey	38N 042E	1	2.3	2.8	82
Iran	35N 050E	1	0.9	2.5	38

Table 1

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PEOPLE SURVEYED DURING 1958 FROM OUTSIDE CONTINENTAL UNITED STATES

COUNTRY	LOCATION	NUMBER SURVEYED	Cesium 137 (dps/lb wt.)	Potassium 40 (dps/lb wt.)	Cs/K μμc/gm K
Korea	38N 127E	2	1.6	2.3	72
Japan	36N 140E	15	1.8	2.5	72
Phillipines	14N 121E	1	1.9	2.3	82
Guam	13N 145E	2	1.2	2.3	52
Australia	33S 151E	1	1.8	2.4	75

Table 2

PEOPLE SURVEYED DURING 1958 FROM OUTSIDE CONTINENTAL UNITED STATES

COUNTRY	LOCATION	NUMBER SURVEYED	Cesium 137 (dps/lb wt.)	Potassium 40 (dps/lb wt.)	Cs/K μμc/gm K
Puerto Rico	19N 065W	1	1.8	2.8	63
Panama	09N 080W	1	2.1	2.7	78
Venezuela	09N 070W	1	1.8	2.6	67
Colombia	01N 074W	1	2.8	2.4	116
Brazil	23S 044W	4	0.3	2.3	15

Table 3

PEOPLE SURVEYED DURING 1958 FROM OUTSIDE CONTINENTAL UNITED STATES

COUNTRY	LOCATION	NUMBER SURVEYED	Cesium 137 (dps/lb wt.)	Potassium (dps/lb wt.)	Cs/K μμc/gm K
Alaska	65N 150W	3	1.9	2.7	69
Hawaii	22N 155W	6	1.5	2.4	65

Table 4

The Determination of Internally Deposited Radioactive Isotopes in the Marshallese People by Excretion Analysis#

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Washington 12, D. C.

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Present Address: Nuclear-Chicago, Chicago, Illinois

INTRODUCTION.

Following the detonation of a thermonuclear device at the Pacific Test Site on 1 March 1954, 239 Marshallese people were exposed to significant levels of gamma radiation from fallout. Estimated total exposures ranged from 175r on Rongelap to 14r on Utirik (1).

These populations were evacuated to Kwajalein for decontamination and care. During the two days of fallout exposure before evacuation was completed, the Marshallese also received some radioactive materials internally by ingestion and inhalation. Estimates of the internal body burden from fallout were obtained from the analysis (1) of urine samples collected soon after exposure.

These data indicated that the acute hazard from internally deposited fission fragments was quite small as compared to the whole body gamma radiation exposure. Although the radioactivity levels in the urine were low, the activity was sufficient to obtain reasonable precision and to warrant additional long term studies of the activity levels and excretion patterns of this rather large and well isolated population.

The people from Alinginae and Utirik were returned to their home islands in June 1954. Radiation intensities on Rongelap, however, precluded an early return to this atoll and the Rongelap people lived on Majuro from June 1954 until July 1957.

Basic data on the food crops of the Marshallese indicated that after resettlement on the contaminated atolls the intake of strontium^{90} would be increased considerably, and that cesium^{137}, zinc^{65}, and cobalt^{60} were dietary constituents of island and ocean foodstuffs, and also would be assimilated (2). The expected increases in the trace amounts of radionuclides in the food supply of a large population would afford an opportunity to investigate the rate of equilibration and the discrimination factors operating between food supply and man. Urinary

excretion levels of cesium^{137} and strontium^{90} were measured from March 1954 through March 1958. Zinc^{65} levels were first measured in 1958 samples.

MATERIALS AND METHODS.

One hundred and forty one individual urine samples collected from 24 March 1954 through 7 September 1954 were obtained by the Health and Safety Laboratory, AEC. Urine volumes were small (about 350 ml) and it was necessary to pool samples. This was done according to the age of the subjects and 19 samples of pooled urine were assayed. A 57 liter pooled urine sample from Rongelap was collected and assayed in 1956 (3). Three pooled samples and seven individual samples were assayed in 1957. Thirty individual urine samples were assayed in 1958.

In samples collected in 1954 and 1957 cesium^{137} was scavanged by nickel ferrocyanide (urine made strongly alkaline) and counted in a crystal well counter. A twenty channel gamma-ray spectrum was determined for each sample and the cesium^{137} photo spectrum count rate used. The 1958 samples were assayed directly for Cs^{137}, Zn^{65}, and K^{40} in 2.5 liter plastic containers placed on an 8 x 4 inch (TH activated) sodium iodide crystal. The activity for each radio-isotope was determined by gamma-ray spectral analysis. Sample activities were compared with known radio-active standards (± 5 percent) counted in the same geometry.

Strontium^{90} was precipitated from urine as the carbonate. Yttrium^{90} was separated and identified by its half-life using thin walled gas flow counters.

Urine samples were corrected for radio-active decay to the time of collection.

There is some uncertainty as to the completeness and the duration of time over which samples were collected and therefore twenty-four hour urine volumes are not accurately known. Potassium^{40} excretion, using 360 d/m or 2 gm K/day indicates an average daily volume of about 1180 ml (± 56 percent). It was convenient to use one liter as an average 24 hour urine volume and to express radio-assays in micromicrocuries per liter.

RESULTS AND DISCUSSION.

Cesium^{137} Excretion Levels and Body Burden

The urinary excretion levels of cesium^{137} for the years 1954, 1957, and 1958 are shown in Tables 1 - 4. On 24 - 25 March, 1954, the mean excretion level of cesium^{137} for all age groups was 405 uuc per liter. With an excretion rate of 0.46 percent (4) of cesium^{137} body burden per 24 hours, the mean body burden from fallout 24 - 25 days after exposure was $405/4.6 \times 10^{-3} \times 10^{6}$ or 88 muc (± 54 percent). This value is about 20 times the average body burden reported by E. C. Anderson, et al (5, 6) for people measured during 1956 - 1957 in the United States. The cesium^{137} urinary excretion levels for the six months following exposure can be expressed as an exponential function, and a best line of fit drawn through the data resulted in a half time for elimination of about 110 days (Fig 1). A biological half time of about 140 days has been observed on volunteers who ingested one microcurie of radio-cesium (4).

From the 1957 Cs^{137} excretion levels (Table 2) the Rongelap group exposed to fallout was estimated to have an average burden of about 7 muc, whereas the Rongelap control group was about 2 muc. Body burden in either group in 1957 is comparable to levels measured in the U. S. population (6). With a half time for elimination of the order of 150 days, the body burden of the exposed Rongelap group should have decreased from the March 1954 level to 7 muc in about 550 days, or late in 1955. A body burden of 7 muc for this group in March 1957 could then indicate a continuing exposure to Cs^{137} during 1956 of the order of 32 micro-microcuries per day from stratospheric-tropospheric fallout while residing on Majuro.

Since the Utirik group was returned to their atoll in 1954, the mean body burden in 1957 was elevated to an estimated 337 muc, some 48 times the Cs^{137} burden of the exposed Rongelap people who resided on Majuro. This long residency time on Utirik atoll after fallout contamination, as compared to the excretion

rate of Cs^{137} should have resulted in an equilibrated Cs^{137} burden, with an estimated daily intake of about 1560 uuc of Cs^{137} Unfortunately no systematic survey of foodstuffs grown on these atolls has been reported. Data available, however show that cocomut grown on Rongelap contained about 9 uuc Cs^{137} per gram, and arrowroot (Utirik) contained about 8 uuc Cs^{137} per gram. The daily intake of several hundred grams of either staple would be sufficient to account for the 1957 excretion level in the Utirik group.

The Rongelap groups had been resettled for about nine months at the time of the March 1958 medical survey, and urinary excretion levels of Cs^{137} had increased about one hundred fold over 1957 levels. Mean body burden for the two groups at this time was 0.9 uc (± 27 percent) and 1 2 uc (± 47 percent) (Tables 3 and 4) Cesium^{137} body burden may have equilibrated by late 1958 and predicted burdens were about 1.3 and 1.6 uc respectively

Strontium^{90} Excretion Levels and Body Burden

Urinary excretion levels of strontium^{90} are presented in Tables 1, 2, and 3. The strontium^{90} excretion level in 1956 was 0.5 uuc/liter as determined in a pooled sample of 57 liters Figure 2 shows the excretion of Sr^{90} for the three years following fallout exposure. Although there is considerable variation in the data for the various age groups at early times, mean values for all groups plotted suggest that the excretion pattern can be expressed conveniently as the sum of two exponential terms The larger portion of Sr^{90} was excreted with a half time of about 40 days, and a small fraction, 20 percent, was excreted with a half time of about 500 days. This is similar to Covan's (7) urinary excretion study of an accident case involving inhaled Sr^{90}.

As was noted in the March 1958 Cs^{137} levels, the excretion levels of Sr^{90} were also increased to 3.5-4 0/0.2, or about 20 fold. Since Cs^{137} levels increased 4300 - 5200/34, or about 140 fold, the ratio is about seven in favor of cesium^{137} With the increases in urinary Sr^{90} excretion levels in 1958, it was

pertinent to estimate body burden, burden expected at equilibrium, and daily intake of Sr^{90} from these excretion levels.

The metabolic behavior of strontium as outlined in Supplement #6 of the British Journal of Radiology was used to estimate body burden, etc. from urinary excretion levels of strontium^{90} (Appendix). The fraction of strontium absorbed from the gastro-intestinal tract is 0.6 and the biological excretion rate from the total body is 190 days. Of the absorbed fraction, 0.25/0.60, about 42 percent is deposited in bone and the biological half-life is 4000 days. Assuming that the absorbed fraction is excreted entirely in urine, the mean body burden of the exposed Rongelap group in March 1958 was 2 muc (± 52 percent). This is about nine percent of the expected equilibrium value of 23 muc. The estimated burden of strontium^{90} for March 1958 is probably too low and compares with levels measured in stillborn children in the U. S. several years ago (8). The daily intake of strontium^{90} is estimated to be about 15 micromicrocuries or 15 Sunshine Units (assuming a daily calcium intake of one gram).

Dunning (2) reported that the average concentration of strontium^{90} in the Marshallese food supply could be about 360 Sunshine Units, but this would reduce to well under 100 Sunshine Units if the consumption of high Sr^{90} content foods were eliminated. With the elimination of pandanus and land crabs the diet used by Dunning indicated that the intake of strontium^{90} would be 17 Sunshine Units per day. This compares favorably with the estimated intake of about 15 micromicrocuries from excretion analysis.

Zinc^{65} Excretion Levels and Body Burden

In early 1957 Miller (9) detected Zn^{65} in selected residents of Rongelap and Utirik by whole body gamma-ray spectrometry. Body burden ranged from 29.5 to 73.0 muc for the Rongelap residents, and 482 and 229 muc was detected in two subjects from Utirik. The Rongelap subjects were residing on Majuro at this time.

Miller obtained an effective half-time of 110 days for the elimination of Zn^{65}, and for the two subjects from Utirik the urinary to fecal excretion ratio was 1/9.

Assuming the excretion to be entirely exponential and 10 percent of the body burden of Zn^{65} excreted in urine, the March 1958 urinary excretion levels of 174 and 342 micromicrocuries indicate body burden, equilibrium body burden, and daily intake as follows:

	RONGELAP	
	1954 Exposed Group	Control Group (Unexposed 1954)
Body Burden (March 1958):	280 muc (± 49%)	540 muc (± 90%)
Equilibrated Body Burden:	330 muc	650 muc
Daily Intake:	2100 uuc/day #	4100 uuc/day #
Percent Equilibration:	85.0 percent	83.0 percent

The mean body burden estimated from 1958 excretion analysis for all Rongelap subjects showed a ten-fold increase over the 1957 whole body measurements. This increase correlates with the return of these people to Rongelap atoll from Majuro. Also the 1958 Rongelap Zn^{65} burdens are comparable with the Utirik subjects in 1957, and the Utirik subjects would have been in equilibrium in 1957 (half time of 110 days for the elimination of $zinc^{65}$).

The estimated intake of $zinc^{65}$ (2000 to 4000 uuc per day) can not be accounted for by Zn^{65} activity levels reported in foodstuffs. Although this radio-nuclide reportedly accounts for a large fraction of the total activity in fish, this amounts to only about six uuc per pound of muscle up to 75 uuc per pound of whole fish (2) or at most four percent of the estimated intake.

CONCLUSIONS.

Since resettlement of the Marshallese people on Rongelap atoll in July 1957, the urinary excretion level of $cesium^{137}$ has increased about 140 fold and about

#Assuming 100 percent absorption from the GI Tract

20 fold for strontium^{90}. Zinc^{65} was readily detected in samples from the March 1958 medical survey.

The estimated mean body burden at equilibrium for cesium^{137} is about 1.5 microcuries or about 1/6 of the tolerance recommended by the International Commission for Radiological Protection for non-industrial populations. For strontium^{90} the mean body burden of the exposed Rongelap group in March 1958 was estimated to be two millimicrocuries. This is about nine percent of the expected equilibrium value of 23 millimicrocuries. The equilibrated strontium^{90} burden is about 1/5 of tolerance. The estimated mean body burden of zinc^{65} for Rongelap subjects in March 1958 is about 85 percent of the equilibration value of 0.6 microcuries and the equilibration value is 1/70 of tolerance.

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TABLE 1

EXCRETION LEVELS OF URINARY CESIUM^{137} AND STRONTIUM^{90}
(uuc per liter) IN THE MARSHALLESE AT VARIOUS TIMES AFTER EXPOSURE IN 1954

DATE OF COLLECTION	AGE GROUPS										MEAN ±S.D.	
	< 5 yrs		5-16 yrs		16-24 yrs		24-40 yrs		> 40 yrs			
	Cs^{137}	Sr^{90}	Cs^{137}	Sr^{90}	Cs^{137}	Sr^{90}	Cs^{137}	Sr^{90}	Cs^{137}	Sr^{90}	Cs^{137}	Sr^{90}
24 March 1954	-	-	889	11.0	294	5.4	372	3.9	258	-	405±218	7.1±2.4
25 March 1954	-	-	-	-	-	-	268	7.7	352	7.7		
17 April 1954	794	16.4	780	-	431	1.7	311	1.2	323	2.3	528±214	5.4±6.5
14 or 31 May 1954	-	-	255	13.4	427	4.2	434	0.9	543	5.5	415±100	6.0±4.6
6 or 7 Sep 1954	-	-	118	2.0	281	1.9	86	0.5	141	0.5	157±73	1.2±0.6

TABLE 2

EXCRETION LEVELS OF URINARY CESIUM^{137} AND STRONTIUM^{90}
IN THE MARSHALLESE DURING MARCH 1957

SOURCE	MEAN SAMPLE VOLUME	ACTIVITY (uuc/liter)	
		CESIUM^{137}	STRONTIUM^{90}
Exposed - Rongelap	4,100 ml	34.	0.2
Controls - Rongelap	3,664 ml	8.	< 0.2
Exposed - Utirik	2,875 ml	1535.	0.2
	TOTAL SAMPLE VOLUME		
BILLIET #9	5,400 ml	62.	0.5
IROJI #26	10,200 ml	168.	0.6
JOHN #40	2,700 ml	128.	-
TIMA #79	5,400 ml	103.	< 0.2
TOTAK #82	8,800 ml	120.	< 0.2
ALEK #2123	2,700 ml	3,759.	-
LEMAN #2125	5,400 ml	1,698	< 0.2

TABLE 3

EXCRETION LEVELS OF URINARY CESIUM^{137}, POTASSIUM^{40}, ZINC^{65}, and STRONTIUM^{90} DURING MARCH 1958

1954-EXPOSED GROUP
RONGELAP

SUBJECT	CASE NO	SEX	AGE (1958)	URINE VOL.	ACTIVITY CESIUM^{137}	POTASSIUM^{40}	Cs/K	ZINC^{65}	STRONTIUM^{90}
				(ml)	uuc/l	gm K/l	uuc/gm	uuc/l	uuc/l
BELLA	7	M	41	2680	2181	1.0	2203	162	1.6
BILLIET	9	M	27	5700	1233	0.7	1665	100	3.8
ALMIRA	12	F	23	6745	2924	1.3	2232	264	1.5
ETRI	22	F	21	5525	5917	2.5	2357	345	6.0
IROJI	26	M	16	5915	4330	1.6	2706	223	2.1
JANWOR	31	M	36	2580	3393	2.3	1488	238	1.2
JIMAKO	39	F	19	130	13130	--	--	155	NDA
JOHN	40	M	34	1740	2275	0.9	2615	148	6.1
JOJIA	41	M	48	2690	2245	--	--	107	5.3
MONNA	66	F	34	2665	2413	1.4	1664	22	3.1
NIKTIMOS	73	M	22	4125	5584	--	--	147	5.7
NORIO	76	M	13	2665	11708	0.3	45031	237	2.8
TIMA	79	M	49	1015	3717	2.1	1796	121	2.0

TABLE 4

EXCRETION LEVELS OF URINARY CESIUM^{137}, POTASSIUM^{40}, ZINC^{65}, and STRONTIUM^{90} DURING MARCH 1958

CONTROL GROUP
(UNEXPOSED-1954)
RONGELAP

SUBJECT	CASE NO.	SEX	AGE (1958)	URINE VOL.	CESIUM^{137}	POTASSIUM^{40}	Cs/K	ZINC^{65}	STRONTIUM^{90}
				(ml)	uuc/l	gm K/l	uuc/gm	uuc/l	uuc/l
TOLMIA	818	M	7	1880	7674	0.3	24755	99.	6.4
REKO	825	F	16	400	9928	2.8	3546	337.	10.2
KEJAI	830	M	20	4275	5165	1.9	2662	553.	6.7
LOLE	831	M	18	1430	7342	1.4	5003	306	2.7
BWIO	836	M	24	585	7028	3.8	1835	88.	2.5
SAMSEN	838	M	26	10515	1867	1.1	1652	324.	3.5
ARTICLE	840	M	31	2355	3393	1.4	2458	1262.	4.1
LATA	843	F	33	6490	2068	0.7	3041	75.	1.7
NAMON	849	M	39	2640	3880	3.3	1158	948.	5.0
MONEAN	855	M	60	2655	3176	1.2	2669	120.	2.5
JOHNI	865	F	25	2125	4624	1.7	2688	319.	4.1
BATOL	872	M	14	5275	7736	1.9	3947	195.	1.3
AMTY	874	M	10	4650	6141	0.9	6533	131.	3.2
HANAKO	876	F	20	2155	2909	0.3	9091	163.	3.3
ATARIK	877	M	20	5245	3311	1.5	2164	233.	2.0
JEKOB	883	M	46	2615	3386	2.1	1626	271.	2.7
KETO	887	M	13	2630	11733	2.2	5357	398	4.7

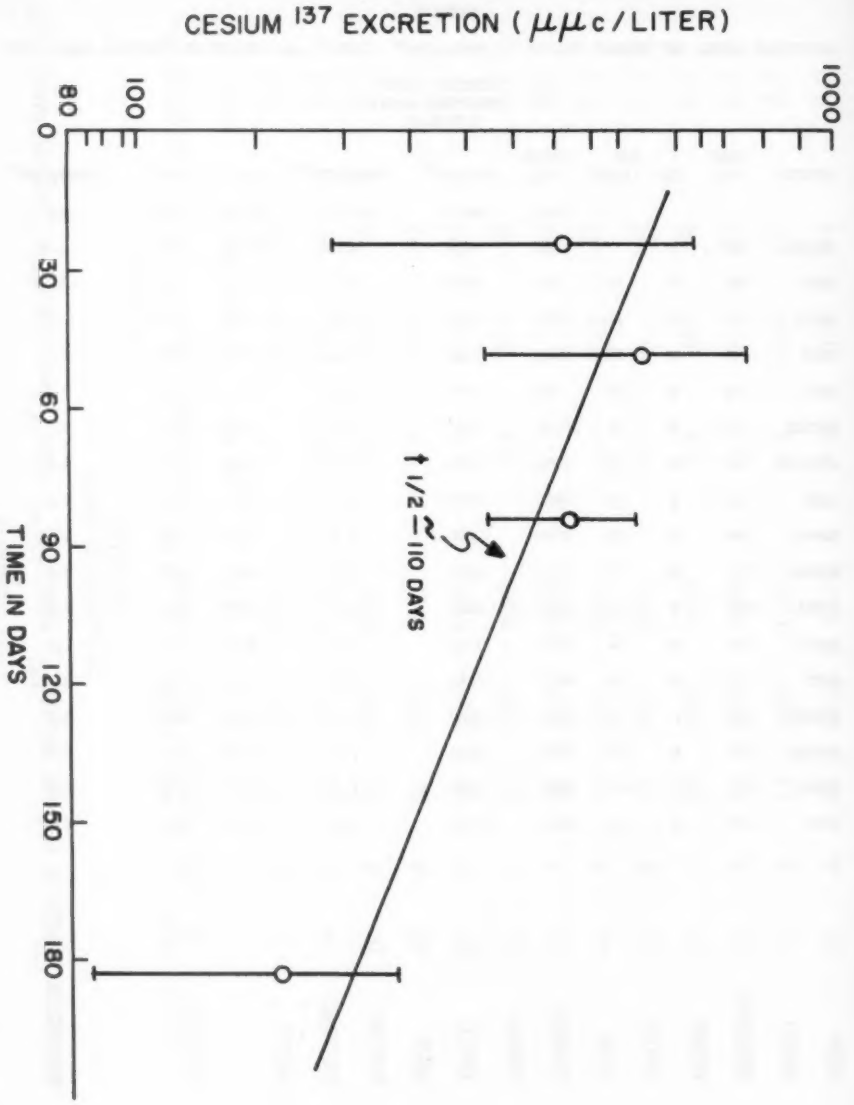


Figure 1. Excretion Levels of Urinary Cesium^{137} at Various Times After Exposure

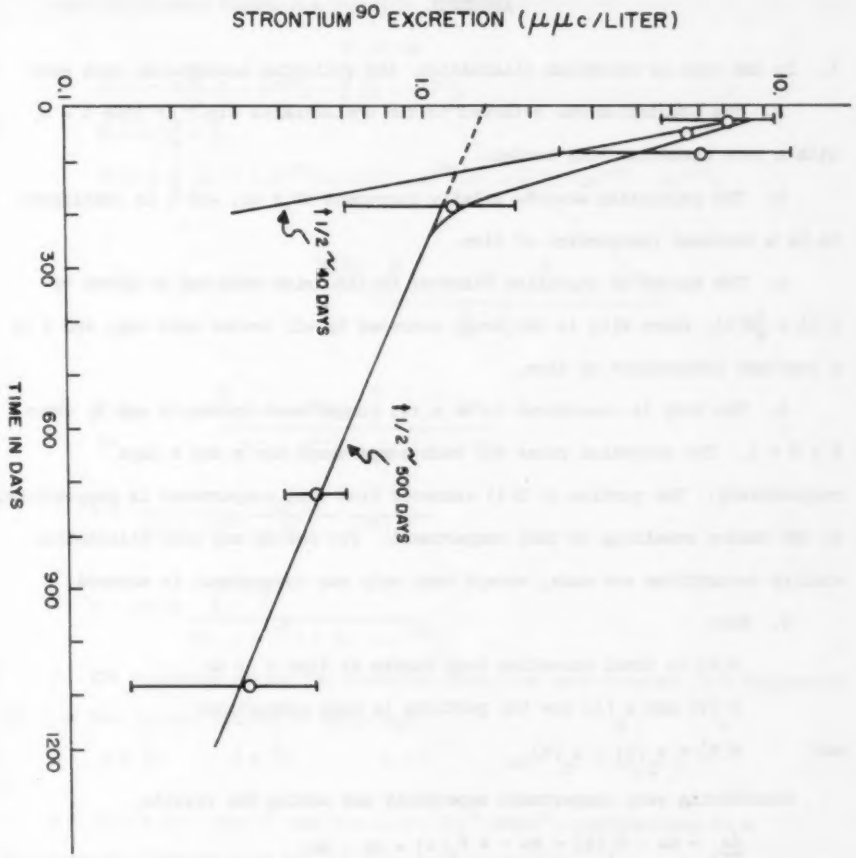


Figure 2. Excretion Levels of Urinary Strontium^{90} at Various Times After Exposure

APPENDIX

I. In the case of strontium elimination, the following assumptions were made:

a. The population was returned to the contaminated atoll at time t = o, with a zero strontium body burden.

b. The population absorbs a daily increment of x uc, and x is considered to be a constant independent of time.

c. The amount of strontium excreted in the urine each day is given by $P(t) = \frac{1}{k}E(t)$, where E(t) is the total excreted by all routes each day, and k is a constant independent of time.

d. The body is considered to be a two compartment system, A and B, where A + B = 1. The excretion rates for each compartment are a and b $days^{-1}$ respectively. The portion of E(t) excreted from each compartment is proportional to the burden remaining in that compartment. For cesium and zinc elimination similar assumptions are made, except that only one compartment is assumed.

e. Now:

$s(t)$ is total strontium body burden at time t in uc

$s_1(t)$ and $s_2(t)$ are the portions in each compartment

and $s(t) = s_1(t) + s_2(t)$

Considering each compartment seperately and adding the results,

$$\frac{ds}{dt1} = Ax - E_1(t) = Ax - k\,P_1(t) = Ax - as,$$

hence $s_1(t) = \frac{Ax}{a}(1 - e^{-at})$ and $s_2(t) = \frac{Bx}{b}(1 - e^{-bt})$

Since $E(t) = as_1 + Bs_2$

$$P(t) = \frac{1}{k}(as_1 + bs_2)$$

equilibrium body burden $M = \lim_{t \to \infty} s(t)$

$$s(t) = x\left[\frac{A}{a}(1 - e^{-at}) + \frac{B}{b}(1 - e^{-bt})\right]$$

$$M = x\left(\frac{A}{a} + \frac{B}{b}\right)$$

$$P(t) = \frac{x}{k}\left[A(1 - e^{-at}) + B(1 - e^{-bt})\right] = \frac{x}{k}(1 - Ae^{-at} - Be^{-bt})$$

1. $y = \%$ of equilibrium $= \dfrac{s(t)}{M} = \dfrac{\frac{A}{a}(1 - e^{-at}) + \frac{B}{b}(1 - e^{-bt})}{\frac{A}{a} + \frac{B}{b}}$

2. $s(t) = kP(t)\dfrac{\frac{A}{a}(1 - e^{-at}) + \frac{B}{b}(1 - e^{-bt})}{A(1 - e^{-at}) + B(1 - e^{-bt})}$

3. $x = \dfrac{kP(t)}{A(1 - e^{-at}) + B(1 - e^{-bt})}$

4. $M = kP(t)\dfrac{\frac{A}{a} + \frac{B}{b}}{A(1 - e^{-at}) + B(1 - e^{-bt})}$

f. The following values for strontium metabolism were obtained from Supplement No. 6 of the British Journal of Radiology:

$A = \frac{7}{12}$ $B = \frac{5}{12}$ $k = 1$ and $s_0 = 0$

$a = 3.65 \times 10^{-3}$ days^{-1} and $b = 1.73 \times 10^{-4}$ days^{-1}, corresponding to a half-time of elimination of 190 and 4000 days respectively.

$x = 0.6\ x'$ and x' is total daily intake.

At $t = 270$ days:

$P(t) = 3.45 \times 10^{-6}$ uc/day (1954 Exposed Rongelap Subjects)

3.9×10^{-6} uc/day (control Rongelap Subjects - Unexposed 1954)

II. In the case of cesium and zinc:

$$s(t) = \frac{x}{a}(1 - e^{-at}) + s_o e^{-at}$$

x is the daily accretion in uc/day, and s_o is the body burden in uc at t = o.

$$\frac{ds}{dt} = -as + x = -a\left[s_o e^{-at} + \frac{x}{a}(1 - e^{-at})\right] + x$$

$$= -E(t) + x$$

$$kP(t) = E(t) = a\left[s_o e^{-at} + \frac{x}{a}(1 - e^{-at})\right]$$

$$M = \lim_{t \to \infty} s(t) = \frac{x}{a}$$

$Zinc^{65}$

$a = 6.3 \times 10^{-3}$ days^{-1} (tl/2 = 110 days) k = 10

s_o = 0.03 uc

$Cesium^{137}$

$a = 4.6 \times 10^{-3}$ days^{-1} (tl/2 = 150 days) k = 1#

s_o = o

#The urinary/fecal ratio of radiocesium for human subjects is about 5/1, so that estimates of body burden are too low by about 20 percent.

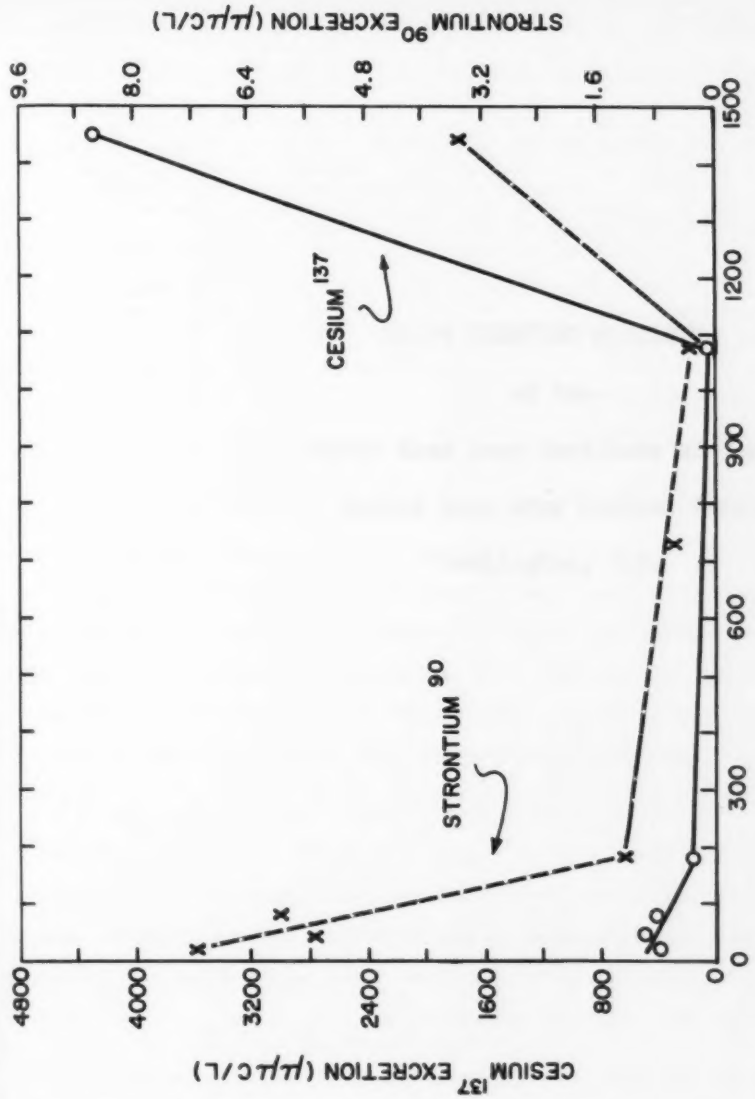

STRONTIUM 90 EXCRETION (μμc/L)
9.6
8.0
6.4
4.8
3.2
1.6
0
1500
1200
900
600
300
0
TIME IN DAYS
CESIUM 137
STRONTIUM 90
4800
4000
3200
2400
1600
800
0
CESIUM 137 EXCRETION (μμc/L)

Figure 3

HUMAN COUNTING FACILITY

at the

Walter Reed Army Institute of Research

Walter Reed Army Medical Center

Washington, D.C.

Kent T. Woodward, Major MC

Harry A. Claypool, Captain MC

James B. Hartgering, Lt. Colonel, MC

The Whole Body Counter at the Walter Reed Army Institute of Research became fully operational on 14 July 1958. During the period 14 July 1958 through 31 March 1959, a total of 1278 persons was measured for levels of Cesium-137 and Potassium-40. Of these, 797 were selected from the total persons studied, by the criteria of having no history of possible radioactive contamination through occupation, and being physically normal, without history of significant disease.

The data has been prepared utilizing the quotient of the two measurements as the most reliable normalization of the results. Since Cesium has the same valence as Potassium, and presumably the same whole body distribution in man, the ratio of these two measurements expressed as micro-microcuries of Cesium-137 per gram of natural potassium, eliminates weight as a variable, and makes it possible to compare levels in people regardless of their total weight, muscle mass, age and sex.

Cesium-137 taken by human volunteers gave a half-times for excretion of 150[1] days. On the basis of this value, the residence for an individual is taken to be that area in which he has most recently resided for a period of at least six months. In other words, it is assumed that if geographical location has an effect on total body burden of Cesium-137, a person must live for at least six months in an area before their body burden would reflect the local levels.

The Walter Reed Facility (Figure 1) was designed to count large numbers of people, since the normal biological variation in levels of this isotope is of the order of 30%. Statistical studies on 425 individuals measured from July to December of 1958 showed a mean age of 31 years (σ=32%) and a mean weight of 159 pounds (σ=18%). The population studied is weighted toward younger people because of our large military component. (Figures 1 and 2)

Detailed results of the data are presented in Figures 2 through 9. Figure 4 shows the variation in radioactivity in the population measured due to the inherrent

potassium-40. The mean was 2.45 disintegrations per second per pound of total body weight (σ = 14%). This result is 6% lower than the value of 2.60 obtained by Dr. E. Anderson and the Los Alamos group.

Figure 5 shows the distribution of the results for Cesium-137 activity. A mean of 1.66 dps per pound (σ = 30%) was obtained. Figure 6 shows the spread of results obtained for potassium-40 activity versus total body weight.

A group of ten Reed College students provided an excellent opportunity for comparison of the results from the Los Alamos and Walter Reed Facilities. This group was measured 25 February 1959 at Los Alamos and was again counted on 2-3 April 1959 at Walter Reed. The results of this study are presented in Table 1. The agreement is considered excellent, particularly in view of the fact that two entirely independent methods of calibration were used. The somewhat lower value for potassium-40 activity obtained at Walter Reed is believed to be caused by the somewhat better geometry presented to the counter by our standards than is the case for a human being. New standards are being prepared which will more closely approximate the geometry of a man. This should result in a steeper slope to the mass versus efficiency curve, with a lower efficiency at higher total body weights and a resulting higher value for the calculated activity due to potassium-40. The lower value for potassium activity also directly explains the slightly higher value obtained for uuc Cs^{137} per gram of potassium.

Table 2 summarizes the data on 656 residents of the U.S. measured between July 1958 and March 1959, and Table 3 in like manner presents the results obtained on residents of Foreign countries. Most of the so-called Foreign residents actually represent U.S. troops returned from overseas tours of duty in excess of six months.

Tables 4,5 and 6 present a detailed geographical breakdown of all the data. Figures 7,8 and 9 present the results from the United States for the time periods

indicated, plotted in the arbitrarily chosen geographical subdivisions.

The most impressive thing about this large collection of data on human beings is the effect of the normal biological variation on results obtained from small groups of people. With a few exceptions, any analysis accomplished by increasing the size of the group, whether by combining small geographical areas into larger ones, or by increasing the period of time over which the average is taken, results in the mean level for Cs^{137} activity in the group approaching that determined for the largest group studied, namely, the Washington, D.C. area.

Considering the normal distribution of the results (Figure 5) it is apparent that if two areas are to be compared, large numbers of individuals must have been measured from each of the two areas. The only two U.S. areas satisfying this criterian are areas 7 and 8, in which 52 and 420 individuals were counted respectively. The levels of 77 and 67 uuc Cs^{137} per gram of potassium suggest that perhaps a real difference exists between these two areas. However, since the areas are contiguous, the reason for the difference is not readily apparent. The possibility of a significant difference in rainfall must be considered, since the great majority of people measured in Area 8 are from the immediate Washington, D.C. area.

Comparing larger geographical subdivisions shows little difference in the results except perhaps for the southwestern part of the nation. Dr. Anderson's results should clarify this question, since most of his subjects should be from this area.

The World Wide results also follow this pattern, with two exceptions. Levels in troops returned from Europe appear higher than those obtained for the resident U.S. population. Results on seven cases from South America indicate a significantly lower level for the southern hemisphere than for the northern hemisphere. However, considerably larger numbers of people from these areas should be counted to confirm this impression. A similar result was obtained by Marinelli et al on 23 May 1957 on

11 South American subjects who showed a mean of 14.6 uuc Cs^{137}/gram K as opposed to a mean of 35 for Chicago subjects at this same time.(2)

Figures 7,8 and 9 show the results for the three time periods indicated plotted on a U.S. Map in twenty arbitrary geographical subdivisions. Because of the small numbers of individuals measured from many of these areas, it is not possible to draw conclusions regarding variation in levels over the nation. For the same reason, over the short period of nine months of operation, no conclusion has been attempted regarding possible increase or decrease in the average national level over the past nine months.

Figure 10 is a composite curve obtained from the data of Anderson(3) and Marinelli(2), plotted from March of 1955 to the present.

The curve from Marinelli's data was obtained by calculating means from the curves for his 8 control subjects. Anderson's data was then plotted to the same scale, as were the two points representing the data from Walter Reed Army Institute of Research.

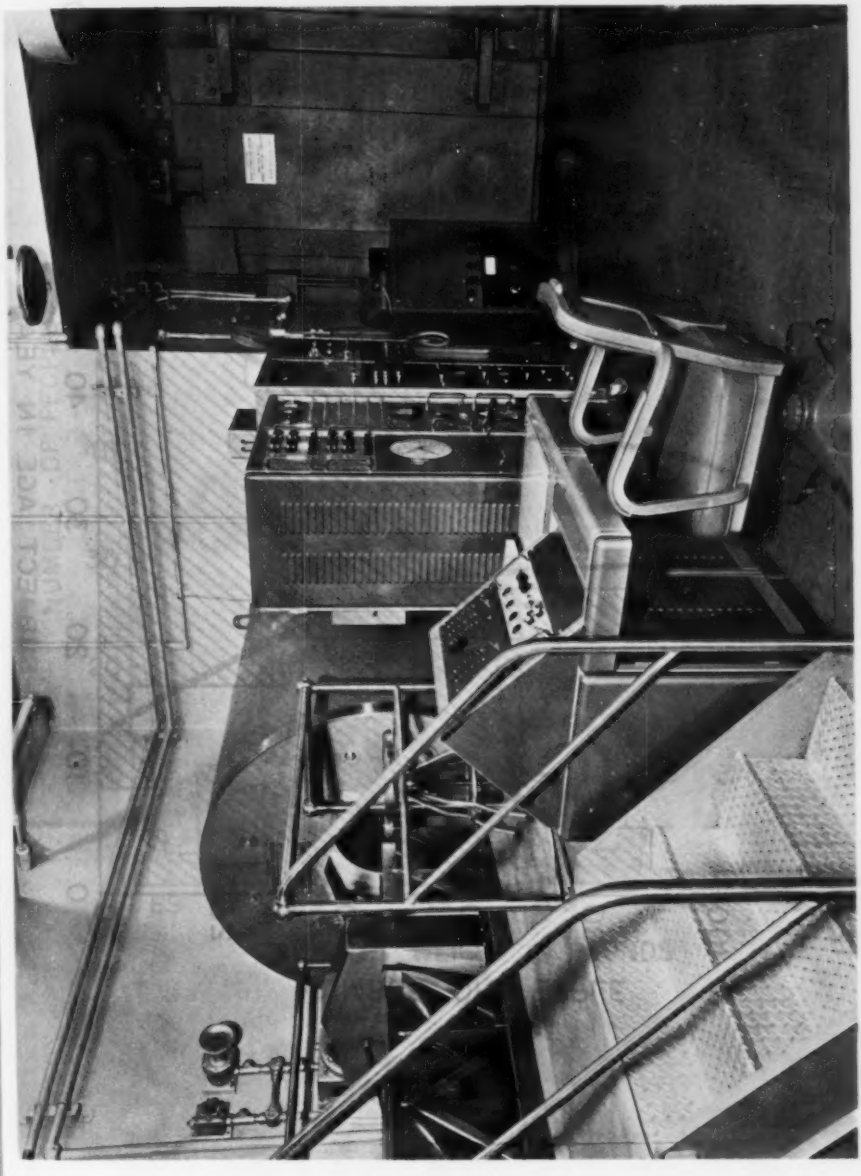
It is hoped that the Facility at the Walter Reed Army Institute of Research will provide data from large numbers of people. In conjunction with the Whole Body Counter to be placed in operation in June of 1959 at Landstuhl, Germany by the U.S. Army, considerably more data will be obtained in the next few years. Only a long range program will permit detailed analysis of the variables pertinent to this problem.

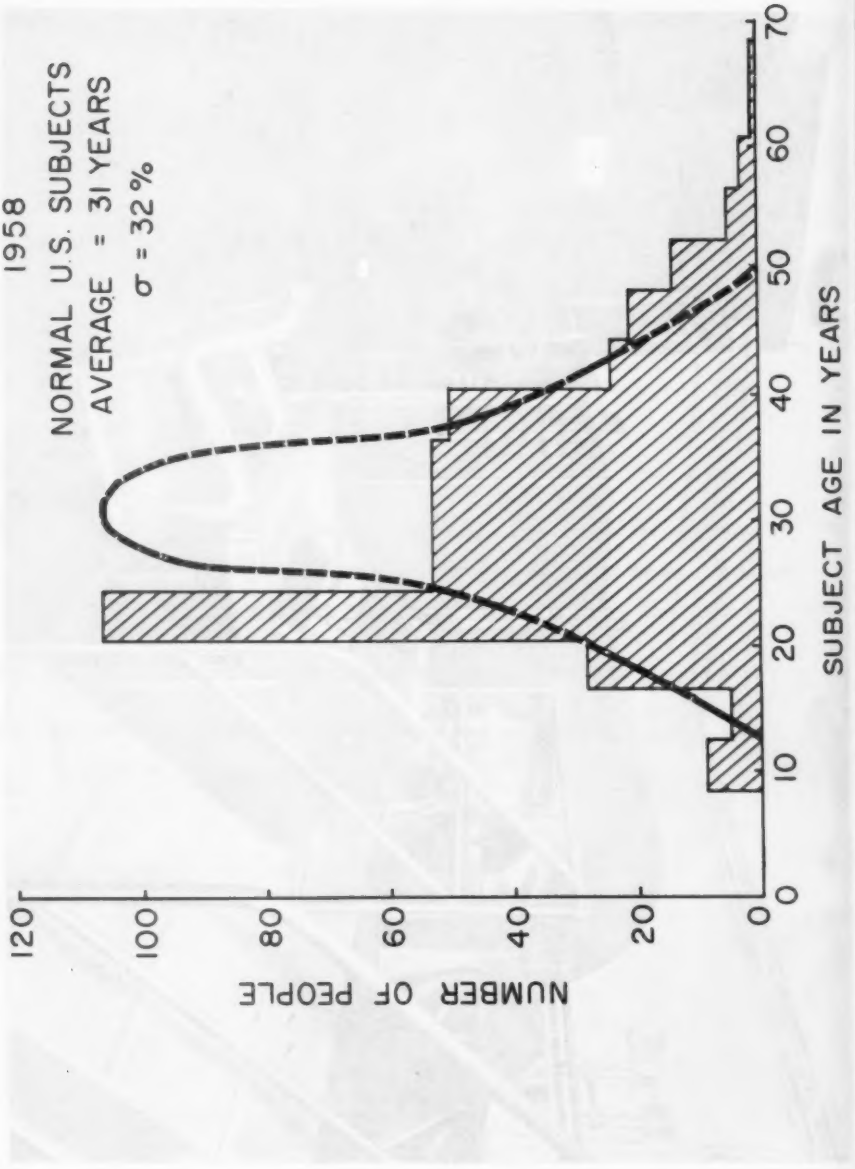
Bibliography

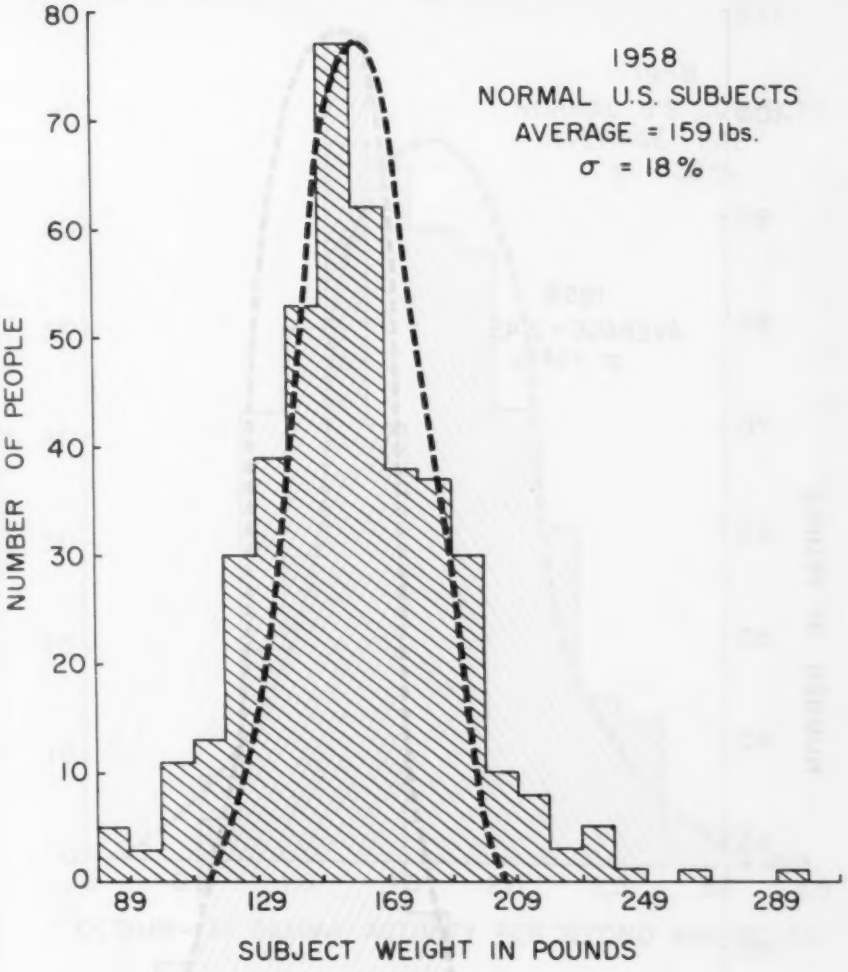
(1) Woodward, Richmond and Langham, "Measurement of Retention and Excretion of Radioisotopes of the Alkali Metals by Mice and Rats using an Annular Liquid Scintillation," Proceedings of the Health Physics Society, June 1956

(2) Argonne National Laboratory semi-annual report, Jan-June 1957, ANL-5755

(3) Anderson, E.C., "Radioactivity of People and Milk", 1957, Science, Oct. 17, 1958, Vol. 128 No. 3329, pp 882-886




1958
NORMAL U.S. SUBJECTS
AVERAGE = 31 YEARS
σ = 32 %
NUMBER OF PEOPLE
0
20
40
60
80
100
120
SUBJECT AGE IN YEARS
0
10
20
30
40
50
60
70



SUBJECT AGE IN YEARS

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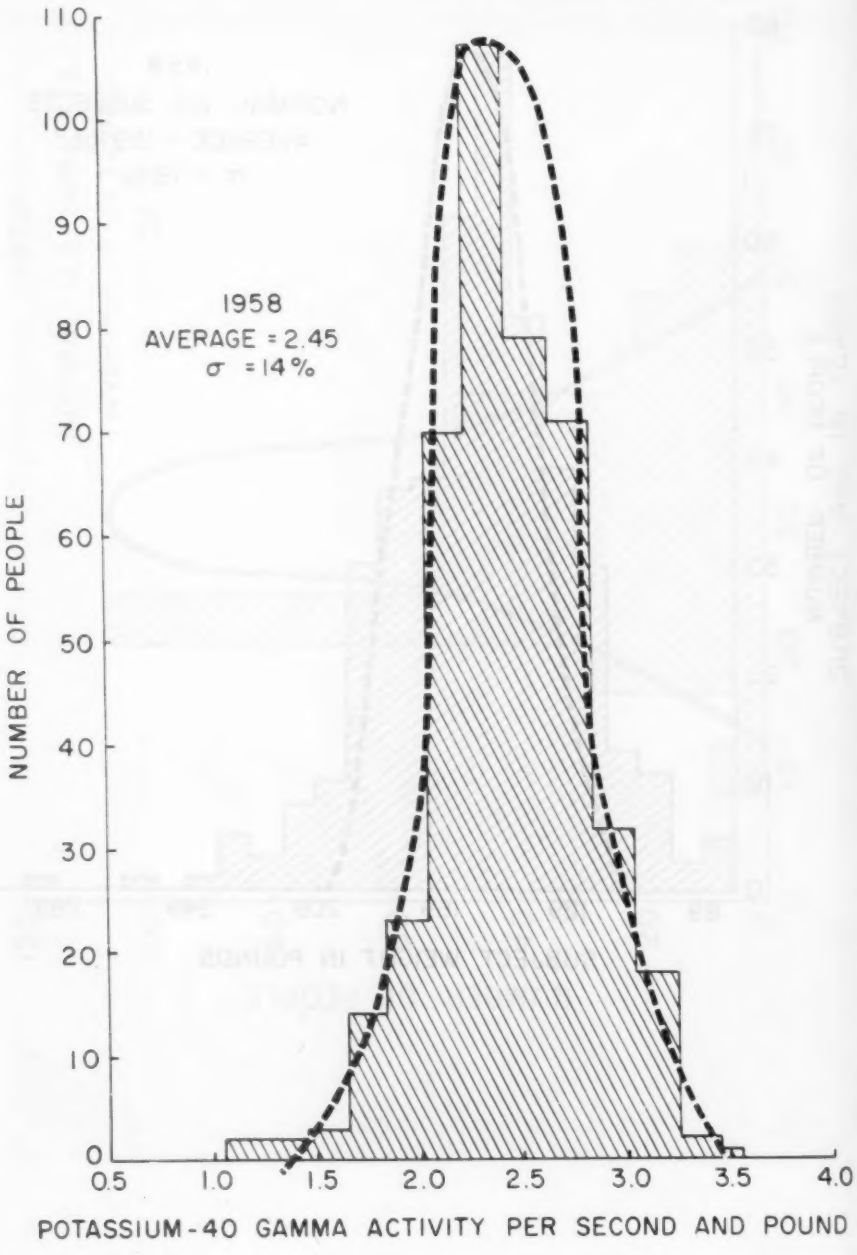
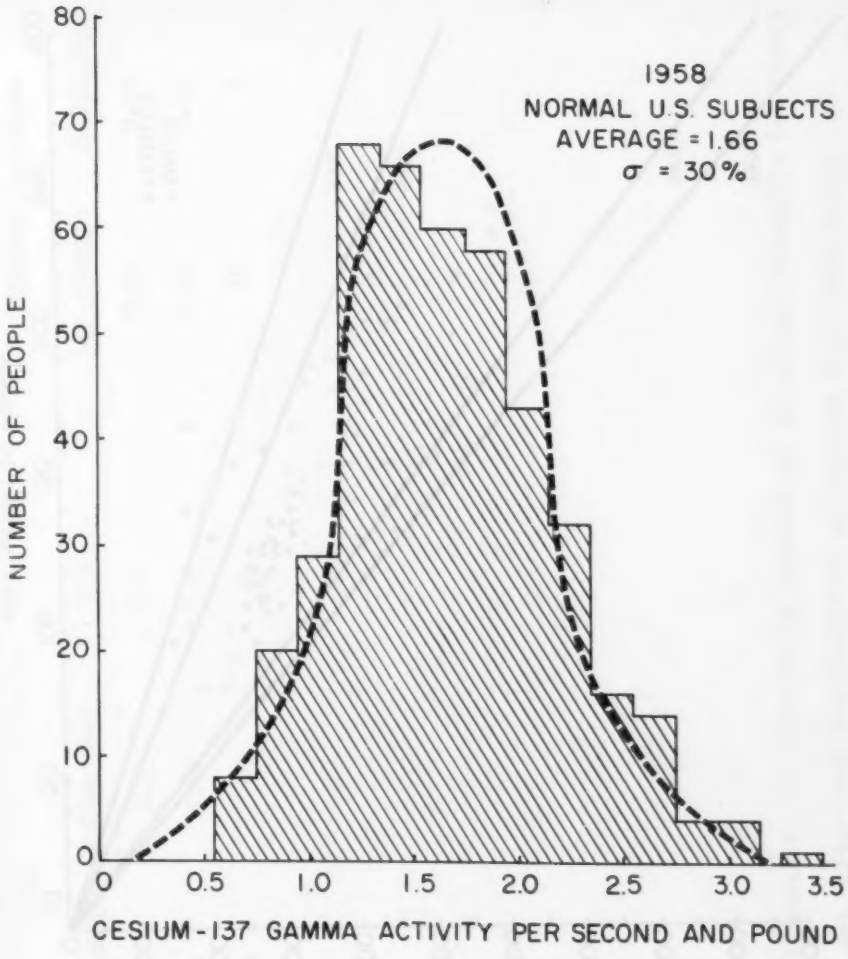
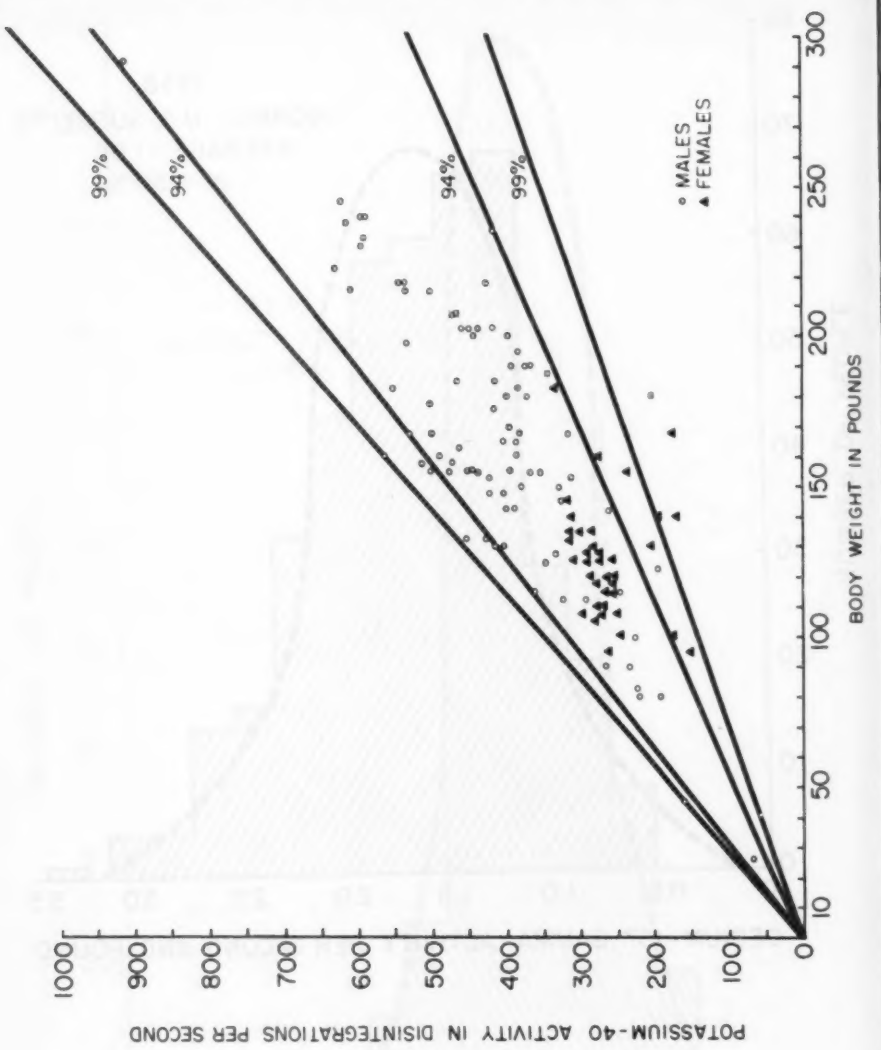

1958
AVERAGE = 2.45
σ = 14%
NUMBER OF PEOPLE
0
10
20
30
40
50
60
70
80
90
100
110
0.5
1.0
1.5
2.0
2.5
3.0
3.5
4.0
POTASSIUM-40 GAMMA ACTIVITY PER SECOND AND POUND


1958
NORMAL U.S. SUBJECTS
AVERAGE = 1.66
σ = 30%
NUMBER OF PEOPLE
80
70
60
50
40
30
20
10
0
0
0.5
1.0
1.5
2.0
2.5
3.0
3.5
CESIUM-137 GAMMA ACTIVITY PER SECOND AND POUND


99%
94%
94%
99%
MALES
FEMALES
BODY WEIGHT IN POUNDS
POTASSIUM-40 ACTIVITY IN DISINTEGRATIONS PER SECOND
0
10
50
100
150
200
250
300
100
200
300
400
500
600
700
800
900
1000

BODY WEIGHT IN POUNDS

	Mean			Standard Deviation	
Parameter	Los Alamos	Walter Reed	% Diff.	Los Alamos	Walter Reed
DPS Cs^{137}/lb.	1.77	1.75	1.1	0.29	0.26
DPS K^{40}/lb.	2.84	2.62	7.8	0.24	0.19
uuc Cs^{137}/gram K	63	67	6.4	10	9

Table 1. Cs-137 and K-40 Levels in Ten Individuals Measured at Los Alamos Scientific Laboratory (25 Feb. 1959) and Walter Reed Army Institute of Research (2-3 April 1959).

Area	Cesium-137 Levels: 1958 July-Dec.	1959 Jan.-March	Average July-March	Number Persons Measured: 1958 July-Dec.	1959 Jan.-March	Total July-March
Northeast	69	69	69	375	118	493
Southeast	70	85	74	14	4	18
N. Central	75	67	71	23	19	42
S. Central	76	57	70	30	15	45
Northwest	65	78	71	7	6	13
Southwest	63	55	60	25	20	45
Washington, D.C.	67	69	67	320	100	420
Total U.S.	69	67	68	474	182	656

Cesium-137 Levels in uuc Cs^{137}/gram K

Table 2. Cesium-137 Levels in Man - United States - July 1958 - March 1959.

Area	Cesium-137 Levels: 1958 July-Dec.	1959 Jan.-March	Average July-March	Number Persons Measured: 1958 July-Dec.	1959 Jan.-March	Total July-March
Northeast	69	69	69	375	118	493
Southeast	70	85	74	14	4	18
N. Central	75	67	71	23	19	42
S. Central	76	57	70	30	15	45
Northwest	65	78	71	7	6	13
Southwest	63	55	60	25	20	45
Washington, D.C.	67	69	67	320	100	420
Total U.S.	69	67	68	474	182	656

Cesium-137 Levels in uuc Cs^{137}/gram K

Table 2. Cesium-137 Levels in Man - United States - July 1958 - March 1959.

Table 2. Cesium-137 Levels in Man - United States - July 1958 - March 1959.

Area	Cesium-137 Levels 1958 July-Dec.	Cesium-137 Levels 1959 Jan.-March	Cesium-137 Levels Average July-March	Number Persons Measured 1958 July-Dec.	Number Persons Measured 1959 Jan.-March	Number Persons Measured Total July-March
United States	69	67	68	474	182	656
Europe	78.5	77	78	47	18	65
Middle East	60	71	66	2	3	5
Far East	69	58	64	26	23	49
Central America	72	56	62	2	3	5
South America	40	64	44	6	1	7
Other	70	65	67	4	6	10
All Non U.S.	72	66	70	87	54	141
Total World	70	67	68.5	561	236	797

Table 3. Cesium-137 Levels in Man - World-Wide - July 1958 - March 1959.

Area	$\mu\mu c\ Ca^{137}$/gram potassium			Number Surveyed		
	14 July '58 to 12 Dec. '58	12 Dec. '58 to 31 March '59	Cumulative Average	14 July '58 to 12 Dec. '58	12 Dec. '58 to 31 March '59	Total
Northeast	69	69	69	375	118	493
Area 1	70	53	68	7	1	8
2	79	69	75	8	5	13
7	80	69	77	40	12	52
8	67	69	67	320	100	420
Southeast	70	85	74	14	4	18
Area 13	70	104	73	11	1	12
18	72	79	76	3	3	6
N. Central	75	67	71	23	19	42
Area 3	58	69	65	3	6	9
4	67	-	67	2	0	2
9	78	67	74	15	10	25
10	80	63	72	3	3	6
S. Central	76	57	70	30	15	45
Area 14	79	71	78	17	3	20
15	89	48	71	5	4	9
19	81	62	67	1	3	4
20	60	52	57	7	5	12
Northwest	65	78	71	7	6	13
Area 5	42	-	42	2	0	2
6	74	78	76	5	6	11
Southwest	63	55	60	25	20	45
Area 11	72	66	70	7	4	11

Area	uuc Cs^{137}/gram potassium			Number Surveyed		
	14 July '58 to 12 Dec. '58	12 Dec. '58 to 31 March '58	Cumulative Average	14 July '58 to 12 Dec. '58	12 Dec. '58 to 31 March '58	Total
Southwest continued						
Area 12	62	57	60	6	5	11
16	62	50	55	3	4	7
17	58	51	55	9	7	16

Table 4. Micro-Micro Curies Cs^{137}/gram K over the U.S. July 1958 through March 1959

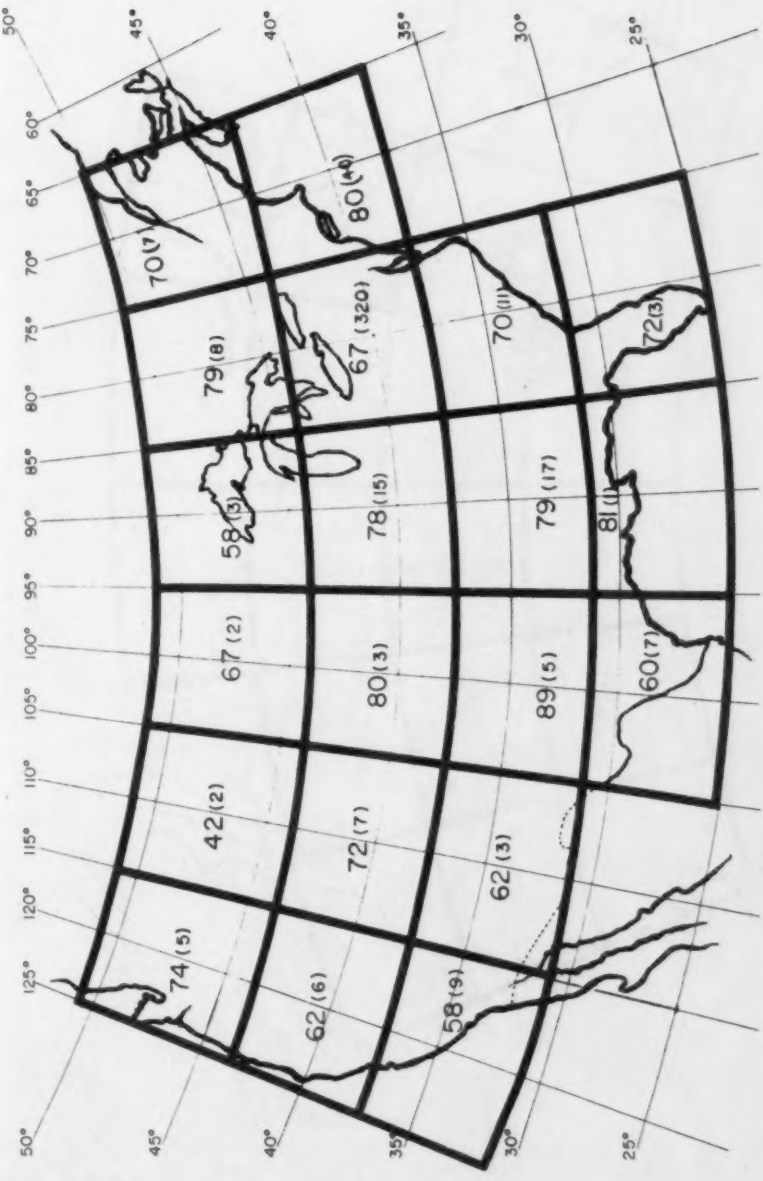
Area	uuc Cs^{137}/gram potassium			Number Surveyed		
	14 July '58 to 12 Dec. '58	12 Dec. '58 to 31 March '59	Cumulative Average	14 July '58 to 12 Dec. '58	12 Dec. '58 to 31 March '59	Total
U.S. Area 1	70	53	68	7	1	8
2	79	69	75	8	5	13
3	58	69	65	3	6	9
4	67	-	67	2	0	2
5	42	-	42	2	0	2
6	74	78	76	5	6	11
7	80	69	77	40	12	52
8	67	69	67	320	100	420
9	78	67	74	15	10	25
10	80	63	72	3	3	6
11	72	66	70	7	4	11
12	62	57	60	6	5	11
13	70	104	73	11	1	12
14	79	71	78	17	3	20
15	89	48	71	5	4	9
16	62	50	55	3	4	7
17	58	51	55	9	7	16
18	72	79	76	3	3	6
19	81	62	67	1	3	4
20	60	52	57	7	5	12
Area 8	67	69	67	320	100	420
All other U.S.	74	64	70	154	82	236
Total U.S.	69	67	68	474	182	656
All Non U.S.	72	66	70	87	54	141
Total World	70	67	68.5	561	236	797

Table 5. Micro-Micro Curies Cs^{137}/gram K in Man - World Wide - July 1958 through March 1959

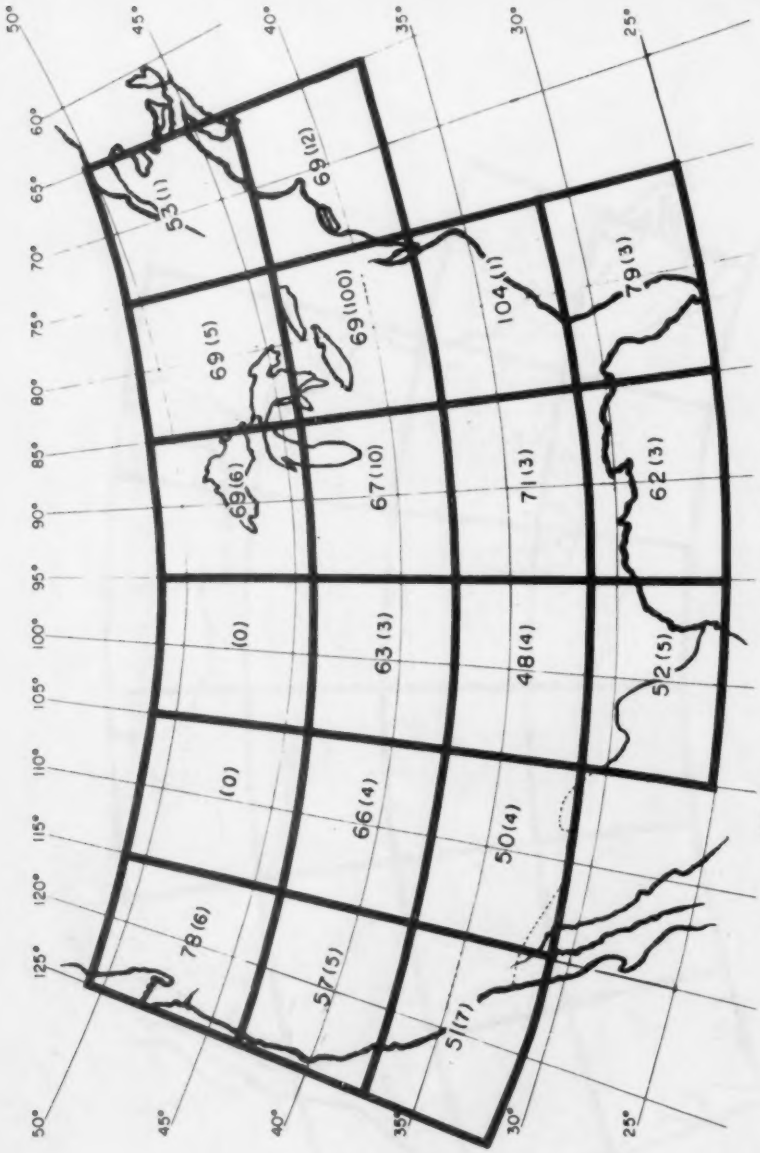
Area	uuc Cs^{137}/gram potassium			Number Surveyed		
	14 July '58 to 12 Dec. '58	12 Dec. '58 to 31 March '59	Cumulative Average	14 July '58 to 12 Dec. '58	12 Dec. '58 to 31 March '59	Total
Europe	78.5	77	78	47	18	65
Italy	107	53	89	2	1	3
France	86	109	89	15	2	17
Germany	72	74	73	28	15	43
England	99	-	99	1	-	1
Holland	74	-	74	1	-	1
Middle East	60	71	66	2	3	5
Turkey	82	54	68	1	1	2
Iran	38	-	38	1	-	1
Pakistan	-	79	79	-	2	2
Far East	69	58	64	26	23	49
Hawaii	65	71	66	6	2	8
Guam	52	-	52	2	-	2
Philippines	82	51	66	1	1	2
Japan	72	60	69	15	6	21
Korea	72	55	58	2	11	13
Thailand	-	46	46	-	1	1
Formosa	-	67	67	-	1	1
Okinawa	-	58	58	-	1	1
Central America	72	56	62	2	3	5
Panama	78	-	78	1	-	1
Puerto Rico	63	62	62	1	1	2
Cuba	-	57	57	-	1	1

Area	uuc Cs^{137}/gram potassium			Number Surveyed		
	14 July '58 to 12 Dec. '58	12 Dec. '58 to 31 March '58	Cumulative Average	14 July '58 to 12 Dec. '58	12 Dec. '58 to 31 March '59	Total
Central America continued						
Guatemala	-	50	50	-	1	1
South America	40	64	44	6	1	7
Colombia	116	-	116	1	-	1
Venezuela	67	64	66	1	1	2
Brazil	15	-	15	4	-	4
Other	70	65	67	4	6	10
Alaska	69	51	64	3	1	4
Australia	75	-	75	1	-	1
Canada	-	73	73	-	2	2
Midway	-	53	53	-	1	1
Newfoundland	-	70	70	-	2	2

Table 6. Micro-Micro Curies Cs^{137}/gram K in Man - World Wide - July 1958 through March 1959.



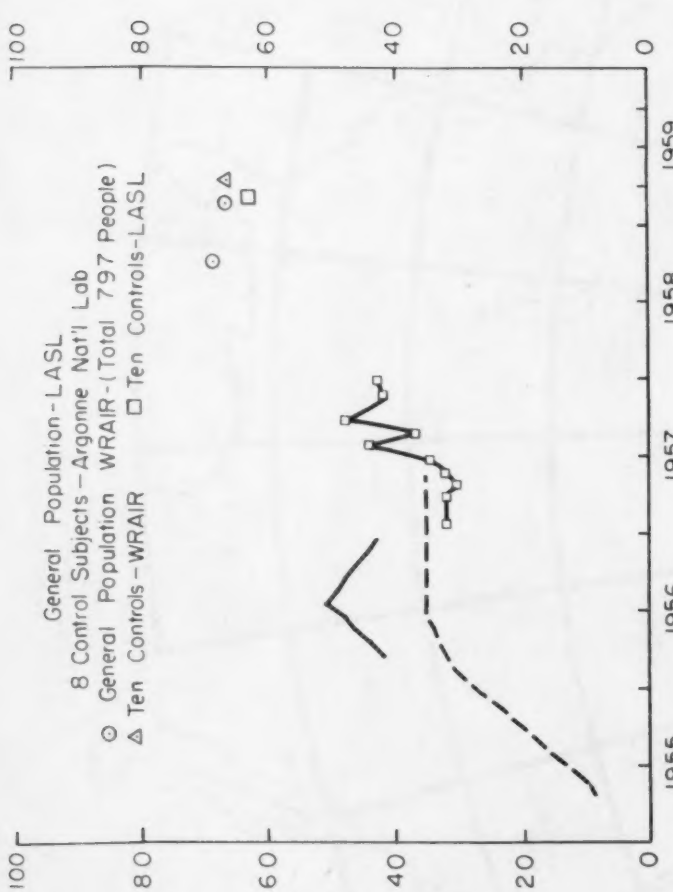
CESIUM ^{137} LEVELS ($\mu\mu c/qmK$) IN NORMAL U.S. SUBJECTS DURING 1958.



CESIUM ^{137} LEVELS ($\mu\mu c/gmK$) IN NORMAL
U.S. SUBJECTS 12 DEC. 58 - 31 MARCH 1959
TOTAL 182 INDIVIDUALS

TOTAL 182 INDIVIDUALS

CESIUM ^{137} LEVELS ($\mu\mu c/qmK$) IN NORMAL U.S. SUBJECTS JULY 1958 - MARCH 1959 (TOTAL 797 INDIVIDUALS)



COMPOSITE DATA – CESIUM – 137 LEVELS

IN UNITED STATES POPULATION

(Thereupon at 4:40 p.m., Wednesday, May 6, 1959, a recess was taken until Thursday, May 7, 1959, at 10 a.m.)

IN UNITED STATES POPULATION

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FALLOUT FROM NUCLEAR WEAPONS TESTS

THURSDAY, MAY 7, 1959

CONGRESS OF THE UNITED STATES,
SPECIAL SUBCOMMITTEE ON RADIATION,
JOINT COMMITTEE ON ATOMIC ENERGY,
Washington, D.C.

The subcommittee met, pursuant to recess, at 10 a.m., in room P–63, the Capitol, Hon. Chet Holifield (chairman of the subcommittee) presiding.

Present: Representatives Holifield, Durham, Price, and Van Zandt; Senators Anderson (chairman of the Joint Committee), Pastore, Gore, and Bennett.

Also present: James T. Ramey, executive director; George E. Brown, Jr., and George F. Murphy, Jr., professional staff members; Richard T. Lunger, technical adviser; and Dr. Paul Tompkins, committee consultant, Joint Committee on Atomic Energy.

Representative HOLIFIELD. The committee will be in order. Our first witness this morning on the developments in radiation biology, with particular reference to the somatic effects of radiation, will be Dr. Austin Brues, of the Argonne National Laboratory, and Dr. Lloyd W. Law, of the National Cancer Institute of the Public Health Service. Will you gentlemen please come forward?

Dr. DUNHAM. Mr. Chairman, I would like to present to you and for the record a statement by the General Advisory Committee of the Atomic Energy Commission at the request of the Chairman, Mr. John A. McCone, which has to do with the AEC studies in relationship to the fallout problem.

Representative HOLIFIELD. This material is a statement by the General Advisory Committee to the United States Atomic Energy Commission. In view of the standing of this committee and the personnel involved in it, the committee will accept it, and it will be included in the record. At the time there will be questions directed to points raised in this statement. The acceptance of it, of course, by the committee does not necessarily mean that the committee approves or disapproves of the statement. It is an independent statement by the General Advisory Committee.

Thank you, Dr. Dunham.

(The statement by the General Advisory Committee follows together with an introductory letter to Chairman McCone from Warren Johnson, Chairman of the General Advisory Committee):

GENERAL ADVISORY COMMITTEE
TO THE U.S. ATOMIC ENERGY COMMISSION
Washington, D.C., May 4, 1959.

Mr. JOHN A. MCCONE,
Chairman, U.S. Atomic Energy Commission, Washington, D.C.

DEAR MR. MCCONE: In acordance with your request of March 23, 1959, the General Advisory Committee to the U.S. Atomic Energy Commission was called into special session for the purpose of reviewing (1) the problems presented by radioactive fallout, and (2) the activities of the Commission as well as those of the Department of Defense and other groups pertaining to these problems, with the objective of providing the Commission wth any advice and suggestions that subsequently might appear pertinent. The Committee met in Washington for 2½ days, April 9–11, during which period briefings and discussions were held with a number of experts in the field representing national committees, the Department of Defense, the Atomic Energy Commission, the Department of Health, Education and Welfare including the Public Health Service, and others. Fortunately, the general problem of fallout is not new to the Committee; its members have followed the problem for a number of years.

The General Advisory Committee commends the Atomic Energy Commission, and Dr. Libby in particular, for having recognized the problems pertaining to fallout and radiation hazards at an early date, and for having initiated investigations on these problems several years ago. It also commends the Department of Defense for the important contributions it has made to an understanding of these problems. The cooperation between the Department of Defense and the Atomic Energy Commission appears to the Committee to have been excellent. If studies pertaining to fallout had not been implemented when they were, we would not have the understanding of fallout that we have today. Attached is a statement which the Committee has prepared and endorses unanimously.

Sincerely yours,

WARREN C. JOHNSON, *Chairman.*

STATEMENT BY THE GENERAL ADVISORY COMMITTEE TO THE U.S. ATOMIC ENERGY COMMISSION*

(1) The General Advisory Committee has reviewed carefully the available facts and many opinions regarding the magnitude of fallout to date, and how much can be anticipated in the future from weapon tests that have already been carried out by the United States, the United Kingdom, and Soviet Russia.

(2) We find that the Atomic Energy Commission has released all significant fallout data to other agencies and to the public. Certain information as to the estimated yield of various weapons tests and certain other factors bearing on the radioactive content of the upper atmosphere have defense implications which require classification, but the significant information on actual fallout throughout the free world that the AEC has developed has been released. Furthermore, the Commission has been largely responsible for the development of equipment and procedures to measure extremely minute quantities of radioactive materials.

(3) It is now apparent that the circulation of the upper atmosphere, and particularly the stratosphere, is much more complicated and the concentration of bomb debris less uniform than had been anticipated when early estimates were made. This has resulted in nonuniform distribution of the fallout with higher concentrations in the middle latitudes of the Northern Hemisphere. Fortunately, it was just here that most of the early measurements of actual fallout were made. The principal result of later information has been to reduce somewhat the earlier estimates of future fallout of debris which has been injected into the stratosphere near the Equator by the United States and United Kingdom tests. The debris injected last autumn by U.S.S.R. tests into the stratosphere in the more northerly latitudes has been falling out quite rapidly and is largely confined to the Northern Hemisphere.

A reasonable estimate of the amount of fission products that has been injected into the stratosphere by all nuclear tests is 65 megatons (TNT equivalent) of fission energy. This corresponds to about 100 pounds of strontium 90 in the en-

*For comments on GAC statement, see vol. 3, app. D.

tire stratosphere. It is estimated that fully 50 percent (50 pounds) of this strontium 90 has already fallen out. This means that not more than half of the total strontium 90 injected into the stratosphere still remains there.

(4) The present state of knowledge does not permit a full evaluation of the biological effects of fallout. However, in order to place the hazard of fallout in proper perspective, it should be pointed out that the amount of total body external radiation resulting from fallout to date, together with future fallout in any part of the world from previous weapon tests, is—

(*a*) less than 5 percent as much as the average exposure to cosmic rays and other background radiation.

(*b*) less than 5 percent of the estimated average radiation exposure of the American public to X-rays for medical purposes.

(5) It is interesting to note that human beings have lived for many generations in parts of the world which have 5 times or more the background radiation normal to the United States, or more than 100 times the average amount of radiation from fallout in the United States.

(6) In regard to internal effects of strontium 90 due to ingestion, the amount of strontium 90 which has been found in food and water is less of a hazard than the amount of radium normally present in public drinking water supply in certain places in the United States, and in public use for many decades.

(7) Next, the Committee addresses itself to the question of the responsibilities of the Atomic Energy Commission with regard to radiation safety. Clearly, the Commission must assure itself that it is conducting its own operations in a safe manner, as is required by the Atomic Energy Acts. To meet these needs the Atomic Energy Commission has established, in addition to worldwide sampling of air, soil, foods, and water, an effective and outstanding biological and medical research program in the general field of radiation hazards and protection. The AEC needs this program in order to have scientific facts available to insure and improve the safety of its own operations. Therefore, the Committee recommends that the AEC continue its scientific studies in these areas.

As the civilian use of X-rays, radioisotopes, and nuclear reactors increases, public health authorities should actively sponsor proper public standards of radiation safety. In so doing they should continue to make use of all information available. The relation between the public health authorities and the AEC in its civilian activities should be analogous to that between the same authorities and most industry. The Public Health Service and the Food and Drug Administration should make the best use of information developed by the AEC and others and should be given whatever funds are necessary for programs to be carried out on their own initiative, whether these programs be in the realm of research, training, or dissemination of information to local authorities. It will take time for the public health authorities to develop such programs and to acquire the necessary background of knowledge and experience. In the opinion of the Committee, the public health agencies, both national and local, should gradually assume responsibilities for matters pertaining to the regulation of all radiation hazards affecting the public. At present X-rays are the most important artificial source of such hazards.

(8) It is the opinion of the committee that the level of effort the AEC has devoted to its research programs on radiation standards and protection, in their broadest sense, has been quite adequate. However, it is realized that in several areas of the programs considerable time will be required to obtain conclusive results which will provide a more comprehensive understanding of radiation and its effects.

(9) The committee feels that although the AEC is releasing information on fallout to the public promptly and completely, the statements and scientific papers presenting this information have not always been in a form readily understood by the public. As a result, the public has been confused about the status of fallout and its implications. There is a real need for clear, simple exposition of the facts of fallout in media widely available to the public. We feel that the Commission should assume this responsibility. It should be clearly explained to the public that weapons tests have been an essential part of our effort to prevent the occurrence of nuclear war.

Jesse W. Beams, Manson Benedict, James W. McRae, Eger V. Murphree, Kenneth S. Pitzer, J. C. Warner, Robert E. Wilson, Eugene P. Wigner, Consultant; Warren C. Johnson, Chairman.

Representative HOLIFIELD. Now gentlemen, we will start the program. Do you have a prepared statement, Dr. Brues?

STATEMENT OF DR. AUSTIN BRUES,[1] ARGONNE NATIONAL LABORATORY

Dr. BRUES. Thank you, sir. I do. I will summarize part of it and read part of it.

Representative HOLIFIELD. Yes.

Dr. BRUES. Mr. Chairman, I was asked to make a statement bringing into focus the progress in understanding the somatic effects of radiation since the hearings 2 years ago. I might emphasize that somatic effects of radiation in this context are the effects on the cells of the body other than the germ cells, which will be discussed by other witnesses.

At the time of the last hearings, we had much detailed knowledge of large doses of radiation on man and on the higher animals. It was brought out in those hearings that a shortening of life occurs as a result of doses of radiation that are not quite high enough to kill an animal in an acute mode of death; also, that various forms of cancer, and particularly leukemia, are brought about by such doses of radiation. The fact that human beings exposed to fairly large doses of radiation (say 100 r) have an increased chance of developing leukemia is beyond question. Similar evidence makes it clear that radium in the human skeleton has been responsible for cancer of the bone.

There has been real progress in the treatment of acute radiation sickness—the state that claimed so many thousands in Hiroshima and Nagasaki, and which has been observed in some rare individuals injured in mishaps associated with nuclear materials and reactors. It has now been found, in treating victims of the Yugoslav reactor accident, that human bone marrow will grow in an injured person and supply urgently needed blood elements, and that dangerous complications of the use of foreign tissue can be avoided.

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[1] Birth date: Apr. 25, 1906.

Education: A.B., Harvard University, 1926; M.D., Harvard Medical School, 1930.

Experience: Medical resident, Huntington Memorial Hospital, Boston, Mass., 1930–31; house officer, Massachusetts General Hospital, Boston, Mass., 1931–32; assistant in medicine, Harvard Medical School, Boston, Mass., 1932–34; graduate assistant, Massachusetts General Hospital, ibid., 1932–34; assistant physician, Huntington Memorial Hospital, ibid., 1932–37; Research Fellow in Medicine, Harvard Medical School, ibid., 1934–35; instructor and tutor in biochemistry science, Harvard College, ibid., 1934–42; Moseley Traveling Fellow, Harvard University and Royal Cancer Hospital, London, England, 1935–36; graduate assistant, Peter Bent Brigham Hospital, Boston, Mass., 1936–37; instructor in medicine, Harvard Medical School, ibid., 1936–37; associate physician, Huntington Memorial Hospital, ibid., 1937–41; associate in medicine, Harvard Medical College, ibid., 1937–42; ward visit, Boston City Hospital, ibid., 1938–39; freshman advisor, Harvard College, ibid., 1939–42; junior associate, Peter Bent Brigham Hospital, ibid., 1940–41; intern, Army Induction Center, ibid., 1940–44; associate physician, Peter Bent Brigham Hospital, ibid., 1941–42; responsible investigator, Office of Scientific Research and Development, ibid., 1941–44; assistant professor of medicine, Harvard Medical School, ibid., 1942–45; assistant physician, Massachusetts General Hospital, ibid., 1942–45; senior biologist, Metallurgical Laboratory, University of Chicago, Chicago, Ill., 1944–46; associate professor of medicine, Department of Medicine and Institute of Radiobiology and Biophysics, ibid., 1945–52; professor, 1952–——; senior biologist and director, Biological Division, Argonne National Laboratory, Lemont, Ill., 1946–50; senior biologist and director, Division of Biology and Medical Research, ibid., 1950–——.

Professional societies: American Academy Arts and Sciences; American Association of Anatomists; A.A.A.S.; American Association for Cancer Research (president, 1954–55); American Clinical and Climatological Society; American Physiological Society; American Society for Clinical Investigation; Association for Study of Internal Secretions; Association of American Physicians; Centennial Society of Clinical Research; Institute of Medicine of Chicago; Society for Cell Biology; N.Y. Academy of Sciences; Society of Experimental Biology and Medicine; Tissue Culture Association; Radiation Research Society (president, 1955–56).

Member of United Nations Scientific Committee on the Effects of Atomic Radiation; member of Advisory Committee, Atomic Bomb Casualty Commission; consultant to the Office of the Surgeon General.

Professional societies: Member of World Health Organization, Mental Health Committee; member of Subcommittee on Internal Emitters of the International Commission on Radiological Protection.

We don't know whether these individual lives were saved because we don't know what would have happened if they had not gotten radiation, but this treatment is now on a more sound basis than it was.

I would like to mention the scientific background of this form of treatment which illustrates some things about how this work goes. Just after the war we were studying the effects of radio strontium and comparing them with X-rays, and were impressed with the importance of the blood forming organs in recovery from radiation sickness. Experiments were made by Jacobson, who shielded parts of the body. Lorenz at the National Cancer Institute then got into the work and first showed the lifesaving efficacy of bone marrow. Many problems remained in making this a safe and useful procedure and these have been worked out at Argonne, Oak Ridge, and elsewhere. Now, not only is the procedure useful in accidental radiation injury, but it looks hopeful as a means of giving large enough doses of radiation to cure leukemia, and still save the patient from fatal radiation effects.

In a number of clinics this sort of work is now going on.

The interesting point here is that this discovery grew out of one of the first fallout studies that was made. This kind of work was not as popular then as it is now. In fact, Lorenz was substantially supported by the Atomic Energy Commission through a contract with the Argonne Laboratory. I have emphasized progress in the acute disease. While that is not our main interest here, it would be an enormously important problem if there were the possibility of a nuclear war.

Representative HOLIFIELD. Of course, the significance of this discovery does not mean that it would be practical to use this particular method in the case of thousands of injuries.

Dr. BRUES. We can see some enormous logistic difficulties about this. A number of us have been thinking about this.

Representative HOLIFIELD. It is something like the problem of blood transfusion only much more difficult.

Dr. BRUES. Yes, sir.

Representative HOLIFIELD. In the treatment of individual or very few cases, such treatment might become significant. But from the standpoint of saving populations that have been exposed, this would be for practical purposes of no particular importance.

Dr. BRUES. I would agree that it would be enormously difficult to tool up for this sort of an operation.

Representative HOLIFIELD. We try to keep this in the proper perspective because we might get a newspaper story, you know, something like the headlines on the New York press this morning, saying we have now discovered a way of curing all radiation by applying one specific statement in such a way that the whole population would gain some unfounded hope as to their safety from radiation in case of war. We want to keep these things in perspective.

Dr. BRUES. Thank you very much.

Representative HOLIFIELD. Although we are very thankful that we found out we can do it.

Dr. BRUES. Thank you for calling my attention to the fact that I do not want to say anything that would mitigate a thermonuclear war.

At the last hearings I think we left the impression that no matter how large a dose of radiation is given, it is possible and indeed some

persons felt likely that a proportionately low number of leukemias and cancers and a proportional shortening of the life span would occur. I have to report now that there is still no proof or disproof of this proposition, and that in fact we don't know where it might be coming from. This is because of the very small proportions of a population that would be involved at fallout levels even under the most extreme guesses that have been made.

Proponents on both sides of the argument are agreed that it would take millions and more likely billions of mice to settle the point directly. Possibly large enough human populations which were identical with respect to everything but radiation exposure might settle the point. We are looking quite seriously into this, but it is doubtful that we can count on an entirely secure answer on these levels, because of the relatively few vital statistics. The more fruitful advances come from the study of proportionality or nonproportionality, where we do have information. That is, where the numbers of injured individuals in terms of tumors may or may not be proportional to the dose of radiation which is given at high doses. Perhaps even more by studies as to the basic nature of processes in the cells and body which produce leukemia and cancer.

I may say that when we look carefully for proportionality we generally fail to find it. R. H. Mole summarized a large amount of work in the British Medical Bulletin last year: I shall supply a copy of his paper for the record (see p. 1409). Dr. A. C. Upton, speaking before the American Association for Cancer Research last month, demonstrated a number of instances in which cancer or leukemia is produced by radiation, in none of which the relation is proportional, and in which the shape of the relation differs very much from one case to another.

Representative HOLIFIELD. Will you please explain for the lay reader of this record what the phrase "in none of which the relation is proportional" means?

Dr. BRUES. Perhaps I might use the blackboard. The proportional hypothesis, as I am calling it, is one which assumes that the number of leukemias or tumors plotted against the dose of radiation makes a straight line which goes down to zero. This, I think, is the most pessimistic situation which any of us can conceive of as a reasonable possibility.

Representative HOLIFIELD. Now, are you speaking of both somatic and genetic or somatic?

Dr. BRUES. I am limiting my remarks to somatic effects. I think the later speakers will emphasize that the evidence on the genetics effects is possibly clearer, and this may indeed be the proper description of the situation. However, in the case of the somatic effects which Dr. Upton brought together in his paper, it appears that they may vary in a number of different ways. At the higher levels we must leave out those levels which are too low to examine, because the proportions of individuals would be to small to give us a significant answer. This is what I mean by the shape of the curve. I mean a deviation from the straight line or proportional hypothesis.

Our own work involving the production of bone tumors by strontium 89–90 has always shown a dropping off of the number of tumors in terms of dosage as the dosage goes down. If we look at the animals at any given time after the strontium has been given, that is. It is not

always the case in the lower doses are relatively less effective than the higher ones, according to some experimental work that has been published. Bond and his colleagues at Brookhaven found that for a particular type of rat which is susceptible to tumors, there seems to be a direct proportion between the number of tumors and X-ray dose between 25 r and 400 r. It is the opinion of some that radiation was in this case hastening the normal processes of tumor formation.

Senator PASTORE. Doctor, one of the frustrating and I suppose irritating situations or incidents with relation to asking questions of witnesses, especially in such an involved field as this where so much ground is covered, is to have a Senator drop in casually and get into something that may have been covered thoroughly. You will forgive me if I do that today. We have so many other commitments that most of us who would like to attend these hearings a little more consistently and constantly do not get the opportunity. I hope you will forgive me and shut me off if I cover a field that has been exhausted.

I remember in 1955, I visited Hiroshima. That was, of course, 10 years after the bomb was dropped in August of 1945. I noticed there at Hiroshima we had quite an elaborate research laboratory built on the side of the hill to which the people fled at the time the bomb was dropped. That and the Nagasaki incident are unique—with the exception of these accidents or incidents that have happened, or the incident we had in the Pacific, and I happened to be there too after that incident, where that population was transferred to the Island of Kwajalein—I recall we had very, very extensive laboratory facilities at Hiroshima, and the purpose was to take cultures and to examine the people. That is the only known place on the face of the earth where a 14 kiloton yield bomb was dropped, with the exception of Nagasaki.

We have been conducting these experiments. I would assume that it being a fission bomb that was dropped, you would have an incident of a tremendous amount of strontium 90 that was deposited in the atmosphere within that locality. In short, and if it has been covered, I don't want to pursue this too far, what have we actually found out with regard to Hiroshima and the amount of money and the amount of experiments that we have conducted there? After all, there were 300,000 people in that locality. It was a 14 kiloton bomb. It was a dirty bomb. There is no question at all about that in relation to this discussion of dirty and clean bombs. What was actually found out, and what is the situation today?

Dr. BRUES. Sir, I would like to answer that in two parts. First, not much is to be found out from the events at Hiroshima and Nagasaki concerning fallout because there was very little fallout. I myself collected some soil a year after the Nagasaki bomb went off in the area which was supposed to be most highly contaminated, and the contamination was relatively quite low in terms of fallout from modern bombs. I think that we have found out that the levels of fallout were sufficiently low that one would not expect to see anything. The important—this is my second part—findings that have come out of there, and that are continuing to come out of there relate to the effects of the radiation that comes over immediately with the flash of light from the bomb. In this regard we have, of course, very extensive data on the rates of leukemias.

We have information which we might perhaps get in no other way as to the fact that if one is exposed to radiation that leukemia develops for a few years, and then appears to drop off. It would be another 5 years before this is absolutely verified but this seems to be the case.

This population is also being followed from the standpoint of other diseases, and this has had to be tied in with a rather extensive study of the exact dose which people got, and which has to be worked out individual by individual. I would say we have not answered all of the questions that ought to be answered, but I think this is an irreplaceable opportunity to learn some things which we would have been very remiss had we not set up to learn. We hope that this won't repeat itself.

Senator PASTORE. Therefore, is it fair for me to assume that with relation to the precise problem that concerns us at the moment with relation to contamination that springs from testing and from fallout, the situation of Hiroshima had a very small part to play scientifically speaking, in obtaining the answers and giving us the results that we are trying to seek out with relation to the overall problem of contamination that comes as a result of testing and fallout after this debris gets into the stratosphere?

Dr. BRUES. I think that is right. The information bearing directly on fallout which we get from there is information gained indirectly through learning about the mechanism of radiation and its action on the human beings.

Senator PASTORE. Will you say that again?

Dr. BRUES. I think the information that we get there is chiefly valuable in this other area of the acute radiation effect and its latent effects on the human beings. What we are learning there that is pertinent to the fallout question is what we are learning through our becoming familiar in this study with more and more things about the effects of radiation on human beings in population. This indirectly has bearing on the fallout problem but is not sufficient in itself, by any means.

Senator PASTORE. Therefore, is it fair for me to say again that the only tangible results we may hope to obtain insofar as guiding us properly with relation to this matter of injury from fallout and what result it has on human beings—God forbid that it gets to the point that it is excessive and does deposit itself on certain people throughout the world—the only experiments thus far that provide any technical knowledge to guide us is from mice or possibly an incident that might occur in some of these reactors?

Dr. BRUES. We have, of course, additional information which we are getting and will get from the rather heavier fallout that occurred in the Marshall Islands which you mentioned. I mentioned this as an example of a human study which has direct bearing on this. This was fallout at higher levels by far than what we have or contemplate having.

Senator PASTORE. Is that the situation of which I have spoken, the people that were transferred to Kwajalein?

Dr. BRUES. Yes, sir.

Senator PASTORE. Apart from that group of people we have to rely chiefly on the experiments that have been made with animals.

Dr. BRUES. I was going to mention a little bit later that it is also possible that we can find human populations which have received different doses of radiation and which are very large populations and are comparable. These populations exist, for instance, in the areas where the radium content of the drinking water varies rather widely. Here we can compare directly persons who got a very low amount of radium deposit in the skeleton with other persons who got radium deposited in amounts which we still consider essentially negligible but which under the most pessimistic hypothesis might have an effect.

Senator PASTORE. Now in order to conclude my inquiry with regard to this phase of the problem, is it fair for me to assume that technologically speaking and scientifically speaking we have the basis at the present time which is substantial enough and has substance enough to give us warning as to what will happen and what effects it will have on human beings if we continue this business of testing and intensify the amount of debris in the atmosphere which will increase the volume of fallout?

Dr. BRUES. I doubt that we have the information and I don't know when we will get the information that would tell us precisely.

Senator PASTORE. I don't mean the information. I mean do we have the source and the basis upon which to predicate our experiments that will lead to information?

Dr. BRUES. We have ways in which to proceed, and this is what part of our research is directed to at the present time.

Senator PASTORE. In other words, the problem is a little more than academic. As a matter of fact, we are now in a situation where we have enough scientific bases and predicates and foundations in order to make a fairly good judgment as to where we are going in this particular field if this testing and fallout business continues.

Dr. BRUES. I would say, sir, that we know a considerable part of it. There are still parts of it that must be worked out and understood.

Senator PASTORE. Am I to understand that as the situation exists today no one can stand up and say that we have little or too much contamination in the world for humankind to sustain because we don't know scientifically what the situation is?

I have heard so much about people debating we have enough and we have less or we have too much. What I am trying to pin down here is this: Are we actually still in the dark? Don't we know just how much damage can really be wrought upon humankind even with the debris that is in the atmosphere at present? Do we have the technology and the scientific knowledge that puts a man in the position that today he can stand up and say we have not quite enough or we have too much?

That is what I am trying to determine in my own mind and I think that is the one thing that is disturbing the American people. You pick up the newspaper and you hear a very reputable scientist say we have nothing to worry about.

I heard that yesterday. Then I picked up the newspaper and I found out that another distinguished eminent gentleman made a speech downtown and he says there is nothing to worry about. Where does the American public stand and I would like to have somebody say it in simple English?

Dr. BRUES. I think what we have to worry about, if we have anything to worry about, at the present levels of fallout is so much less than other hazards that we receive rather innocently that in that sense we have nothing to worry about. I think it is a scientific impossibility to say that no one individual has been injured by the present fallout.

This is beyond possibility to state at the present time. I think this is not unique. I think if we were to inquire as to whether some of the chemical agents that produce cancer in the exhausts of automobiles are giving us anything to worry about or are not, scientific opinion would say no, we don't know and we don't know how to find out.

I think this is a very parallel problem. I can say, however, that if we have anything to worry about from our present levels of fallout, it is much less than we would have to worry about on the basis of going from one part of the country to another where the radiation level is doubled because of natural causes. Whereas, in the case of fallout this is not the case at present.

Senator PASTORE. Am I fair in assuming, then, Doctor, that insofar as you are concerned you have no immediate apprehension that the situation is such as of today that we ought to be unduly alarmed?

Dr. BRUES. I would say that this is right. I think there has been a tendency on the part of the public to feel either that the situation is an entirely safe one that can be forgotten about or on the other hand that if we get above a certain level there is going to be a catastrophe and lots and lots of people will die and there will be an epidemic. This is not true.

Senator PASTORE. There is more substance to it than that. It is not an abstract theory. How will a housewife in New York State feel this morning when she reads the glowing headline in the newspaper that there was more strontium in the bread that she eats than was estimated. This is serious business. Every time you take a bite of another slice of bread you worry about how much strontium 90 is in it. That is how serious it is.

I think somewhere along the line somebody has to tag this down in more than nebulous debatable words and tell the American public exactly how serious this is and where we are goin.

Dr. BRUES. I think we can do this in the context of other things. Does the housewife worry when she goes shopping and inhales the exhausts of automobiles which in some of our streets are quite loaded with polluting agents of one sort or another. I think not. I think it is an overemphasis which has been placed by some individuals on this particular hazard while we have many others which exist at levels which we have believed and believe are not of concern but nevertheless they could be.

I think if it were true of the fallout radiation that this is a real, although relatively very slight hazard at the levels that exist, we should by the same token worry about a great many more things which have not concerned us up to the present time.

Senator PASTORE. Which brings me up to the next question and I hope I am not being critical of you. As a layman I am trying to pin this down as best as I can. It is your considered opinion that even in spite of the story that appeared in reference to the contamination of

this bread as far as strontium 90 is concerned, you are not alarmed about it and it is not as serious as it might look from reading the headlines.

Can you answer that categorically on the record, please?

Dr. BRUES. I will have to be sure, sir, of your question.

Representative HOLIFIELD. Let me say to my colleague that we are going to take this up with a panel this afternoon.

Senator PASTORE. I realize that. But I have another hearing this afternoon.

Dr. BRUES. If I understand your question, am I as worried as I would be——

Senator PASTORE. In other words, will you please put in proper perspective this situation of the contamination of strontium 90 in this New York bread?

Representative HOLIFIELD. Would you like to have the headline here? I accidentally tore it so you will know what Senator Pastore is talking about.

Dr. BRUES. Thank you, sir. I saw a brief summary of this. The impression I get from this is that some levels of strontium were found in bread which were higher than certain large population levels which had been assumed previously. Possibly I am not the best person to speak to this. There are others like Karl Morgan who might do better but I think I can attempt to answer the question.

Senator PASTORE. I wish you would.

Dr. BRUES. The levels which have been set, as I understand it, are on two bases and indeed two figures appear here. One of these figures is the figure for the permissible strontium concentration in drinking water. If you are eating bread instead of drinking water, and this is on the basis of weight, this would mean that to get above the permissible level you would have to eat as much bread as you drink water adn this would mean eating about 3 pounds of bread a day.

Representative HOLIFIELD. For how long?

Dr. BRUES. And that for an indefinite period.

Representative HOLIFIELD. For a lifetime?

Dr. BRUES. Yes.

Representative HOLIFIELD. In other words, the maximum permissible concentration which is spoken of on the basis of a lifetime?

Dr. BRUES. This is right, sir.

Representative HOLIFIELD. And when it is exceeded for 1 day or in one slice of bread or one loaf of bread or a hundred loaves of bread it still is not a lifetime of bread to give the individual the maximum permissible concentration; is that right or wrong?

Dr. BRUES. That is right. At 3 pounds a day.

Representative HOLIFIELD. For 70 years?

Dr. BRUES. Yes.

Representative HOLIFIELD. For a 70-year period. That is the figure involved?

Dr. BRUES. Yes.

Representative HOLIFIELD. I say it is irresponsible reporting when a headline goes on to an article and while it may give the facts in the article, when the headline raises alarm among the general population. It is irresponsible headlining and irresponsible reporting that plays

up a scientific fact which may be accurate but which puts into disproportion this whole story.

There has been some criticism of this committee in bringing out the facts. We are not restricting anyone. We have tried to get a wide variety of governmental and nongovernmental scientists to appear before this committee and give the facts. We want to give all the facts. We don't want to whitewash anything.

I think it is up to the reporters who cover this to cover it accurately and not write headlines that scare American mothers from feeding bread and milk to their children. I am glad the Senator brought this up.

Senator PASTORE. I want to compliment and congratulate and I say thank God that we have at the head of this subcommittee a man such as Congressman Holifield, of California. I have worked with him for years and I know of no one who is more apprehensive of what the results of this contamination might be, and I appreciate fully his conscientiousness and his industry in searching out the facts.

I though in view of that story that appeared that we should have a clearing of the atmosphere with regard to these hearings as to whether this is a really a dangerous situation to frighten people whether or not is it a headline exaggeration that does not truly reflect the basic facts that are involved in the situation. I am glad it was cleared up. It needed to be cleared up. Because only today I heard that a suggestion was made that we go ahead with another plan on cessation of tests only because of this story.

These things must be placed in proper focus. If they are really dangerous the American public ought to be warned about it. But on the other hand if they are evaggerations they ought to be so told. That is the only reason I brought up the subject this morning, to clear the air and I am glad it has been cleared.

Dr. BRUES. Thank you, Mr. Chairman. I would agree with your summary.

Senator GORE. Of course, this person who might be eating bread might also be eating broccoli, might also be drinking milk, might also be subject to background radiation. Just as I think it is possible to give a disproportionate estimate of this potential hazard by taking an obtruse extrapolation of one particular item, it might also give a disproportional and unbalanced understanding of the potentiality of the hazard by calculating the effect of radiation only on one item of food which a person might consume.

As I understand your earlier answer, aside from the bread incident, it seemed to me that you were telling us that we should not worry about this because far more people would be injured in automobile wrecks. The implication was that, since there were so many other greater risks, this should not be the cause of so much worry.

Is that a fair or an unfair interpretation of what you had said earlier?

Dr. BRUES. I think, sir, this was just an attempt on the part of your witness to attempt to define some of the words that are used, like "serious", "danger", and so on. I think if we are to acknowledge that there is the least possibility that some very small proportion of individuals might be injured—and I have maintained that although I have good reason to doubt that the predictions are true, nevertheless

there is no way to rule it out—if we are to acknowledge that there is any possibility whatever, then it is necessary to look at the whole problem in the context of what people put up with in many other areas and, indeed, perhaps most importantly—at least I feel competent to speak of this when I speak of the peacetime applications of atomic energy—to look at the benefits that are to be derived and to compare them with what will usually turn out to be a very small and questionable hazard.

Senator GORE. I was not undertaking to be critical of you as a witness or of your testimony. I was only trying to find out if my interpretation was reasonably accurate. I think you confirm my interpretation. I have heard it estimated that only one out of 500,000 American citizens might receive—I am not sure whether it was a serious overdose or a fatal dose of radiation—that would be, relatively speaking, a rather small number. But I wouldn't want to be 1 of those 500,000.

Even 1 out of 500,000 is not inconsequential. The essential point it seems to me is that this is a manmade hazard. There is little we can do about the background radiation. I think we can stop the contamination, insofar as our country is concerned, of the world's atmosphere.

So long as even a few people are endangered somatically, if that is a good word, so long as even a few children of the future are damaged genetically, it is a matter of serious concern even though far more people may be killed in automobile wrecks. I think the chairman of the subcommittee is to be congratulated upon holding this hearing and this effort to put the facts into perspective.

But I think, Mr. Chairman, we must bear in mind that we are dealing with a potential hazard, the extent of which I do not understand. It seems that there is one thing upon which all scientists do agree, and that is that there is some hazard. That there is a potential hazard.

How great, like Senator Pastore, I am unable to determine. But to the extent that it is hazardous it is a manmade hazard for which this committee and officials of this Government must bear a proportional share of responsibility.

Representative DURHAM. Further elaborating on the Senator's statement I think we are all deeply concerned as to the manmade hazard. But if anybody has a solution of this thing—and the evidence that has been presented before this committee for the last 2 days on the North Temperate Zone—indicates that it is not due to the American testing of the manmade radiation hazard.

I think that is pretty evident. It has been profferred in these committee hearings. To solve that situation I don't think any of us have the answer. If anybody has a solution to it, I would like to have it. We have been at Geneva now since last October to try to resolve this question and do something about it and so far we have been able to accomplish very little, if anything; that is my own opinion.

The other thing that I would like to comment further on Senator Pastore's statement in regard to what we learn and what we have got out of the program that we have consistently supported in Naga-

saki and Hiroshima, I have consistently read those reports by the medical team who has been serving there for several years.

I think that was the first laboratory that we really had to come up with some of the answers. I think that team of medical experts have done a wonderful job. I think the investment has been well worthwhile for the future. As the doctor has already said, it is an entirely different thing from what we are discussing here.

The radiation came immediately. If in the future we did have an atomic war, I think that the background and experience and scientific data will be invaluable to the medical service of this country.

Senator PASTORE. I don't dispute that. I concur with that 100 percent. I think that is the best money we ever spent.

Representative DURHAM. I was agreeing with you.

Senator PASTORE. The only reason I brought it up was to see if we could tie the two together. The witness brought out the fact that we have an entirely different problem. I can understand that.

Senator GORE. Will the Senator yield?

Senator PASTORE. I yield.

Senator GORE. To show that this committee has long been aware of this problem and that it is as yet unresolved as it was even 2 years ago, I would like to read a brief paragraph from the summary analysis of hearings before this committee 2 years ago:

> Should a distinction be made between absolute numbers of persons affected by fallout and percentages relating these numbers to the total population of the world? Can we accept deleterious effects on a relatively small percentage of the world's population when the number of individuals affected might run into the hundreds of thousands. This question cannot be answered by considering scientific data only. Overall national policy and great moral issues are also involved. These questions must be discussed in greater detail.

It is in furtherance of that policy of the committee 2 years ago that these hearings are proceeding to discuss this matter today.

Senator PASTORE. Let me say this, I hardly think you could witness such a spectacle in Russia. This is the only country in the world that has opened this whole problem to a public hearing. We are concerned about it. We are discussing it openly. There are no such comparable hearings in Russia where they are making nuclear tests.

Naturally there is a certain element of relativity here. Security against a calculated risk and all these problems have to be weighed. We would all hope in our hearts that tomorrow we could stop all this nonsense of making bigger and better bombs, of having more tests, if we could only rely on some kind of system whereby the Russians would stop as well.

After all it is telling the soldier on the battlefield, "Throw away your pistol because it kills," even when you know an enemy is coming against him with a bazooka. You have to take this thing in proper focus. There is this to be said about America and the scientist of America and this congressional committee, at least we are putting it on the table.

We are discussing it openly. And this is the only place in the world where it is being done.

Representative HOLIFIELD. The Chair would like to say that he appreciates the comments of the members of the committee as to the objective of these hearings. As far as the Chair is concerned, such questions as he has asked have been in the nature of putting the exact

thinking of the individual witness clearly before the people in layman's language that even a noncollege graduate like the Chair could understand.

I am not carrying a torch for those people who want a bucket of blood spilled on the carpet every day in order to make headlines, nor am I carrying it for those people who say that radiation is good for you, that the sunshine units are going to make you healthy. I am not carrying a banner for either cause.

If I can be instrumental in a small way and this subcommittee can be instrumental in a small way of getting the actual beliefs of the competent people that have been brought before us—and we have people on these witness stands and on these panels that have had sharply different positions and this afternoon we will have people that have taken public position completely opposite to each other—the witnesses have been chosen by a panel of scientists occupying different views.

They were not chosen by the chairman of this subcommittee nor by the members. We called in a large panel of scientists to sit down and work out this outline, both governmental and nongovernmental, holding different viewpoints — both the threshold viewpoint and the nonthreshold viewpoint, the linear viewpoint and the nonlinear viewpoint, and the alarmist viewpoint, let us say and the apathetic viewpoint as to the mention of these figures.

So what we are trying to do is to get the best competent scientific testimony on the board. We will try at the end of the hearing to have a coldblooded analysis of the testimony giving both viewpoints on any subject which is in controversy. Wherever there is a subject in agreement between people who have disagreed in the past that will also be recorded.

We will not try to editorialize on the hearings nor to suggest that you stop bomb testing or that you continue it. This is going to be held as objectively as possible. The Chair cannot control what the witnesses say and the interpretation they put on their own numbers.

This he would not do if he could. He wants each witness to speak frankly as to what he believes because this information is important to the American people that they get the best information we have.

Another thing we know is that we cannot be specific in some field even now. We can be more specific than we were 2 years ago because we have accumulated 2 years of additional information. But there will continue to be areas of the unknown and areas that will need research and further laboratory experiments in order to add to knowledge in this new and very difficult and complicated field.

So with that short statement I hope we can proceed now, Dr. Brues; will you start with the second paragraph on page 4?

Dr. BRUES. I will be quite willing to leave part of this out, sir, in the interest of the other witnesses who are to come after me.

Representative HOLIFIELD. I would like you to take that second paragraph because I have had some correspondence in regard to Dr. Alice Stewart's work at Oxford University and I would like for you to cover that particular paragaraph completely, if you will?

Dr. BRUES. The closest approach to an observation of somatic effect at doses of radiation in the range of fallout or natural intensities is that of Dr. Alice Stewart of Oxford University. She made a study

of all cases of childhood cancer and leukemia in England and Wales over a 3-year period, thus studying a population of about 3 million at risk. These children, with cancer or leukemia, turned out to be more likely to have been X-rayed before birth, for purposes of measuring the size of the pelvis, than other children, and suggest that there is a susceptible stage in embryo life when the small diagnostic dose (2 to 4 roentgens) of X-day about doubles the chances (rather small, it is true) of developing these diseases in childhood.

Representative HOLIFIELD. May I stop you right there. You said 2 to 4 roentgens of exposure to the fetus, is that correct?

Dr. BRUES. This is the best assumed value for the radiographic pelvimetry and we believe these were.

Representative HOLIFIELD. This was a concentrated spot exposure rather than a full body exposure?

Dr. BRUES. Yes. This was given locally over the area of the pelvis and most of the embryo child and was given at one time. I think this is an important point.

Representative HOLIFIELD. I think we should at this time, for purposes merely of comparison because we are interested in the fallout and the natural background radiation, call to the attention of the record that it has been testified and agreed upon I think by almost every scientist that the total natural background radiation over a 70-year lifetime is estimated to be 7 roentgens. It has also been testified and apparently agreed upon, that seven-tenths of a roentgen, if I am right, has been the amount of background radiation which has been raised by manmade atomic tests.

Is that right? Is it one-tenth or one-seventh?

Dr. BRUES. I am sorry, sir. I can't give you that figure. That must have come from another witness.

Dr. DUNHAM. I think the seminar group will have a statement this afternoon. It is below one-tenth of a roentgen.

Representative HOLIFIELD. It is below one-tenth of a roentgen. A tenth of 7 roentgens would be 0.7 roentgen, which has occurred by the increase in background of bomb test radiation. I think I am stating it fairly clearly, and that refers to whole body radiation rather than to spot radiation. So it is less effective than if it had been concentrated on the fetus.

Senator PASTORE. If the Congressman will yield on that point, there is another practical phase to this that is quite important. It would strike me from the recitation of this paragraph that if this research has accomplished one thing it has accomplished the fact that there should be a lot more care in the employment of X-rays in relation to the human body.

At least it has brought out the fact that in many, many instances there was such a lack of knowledge and such an excessive use of these X-ray machines that more harm is being done through that source than might possibly be done by way of fallout. At least it has been an assist, let me say to the medical profession, that there has to be a great deal more care and caution in the use of X-ray machines.

The point brought out here is that these concentrated X-rays that have been employed on women in pregnancy have been more injurious than in other instances of fallout.

Representative HOLIFIELD. I want to correct, if there is an incorrect impression, that when I said that the X-ray was a spot or a concentrated dose on the fetus, I want it understood that I understand that the full power of that X-ray is whole body as far as the fetus is concerned, but it is not whole body as far as the mother is concerned.

So it is more concentrated because it is in a smaller area and because the fetus is much smaller in size than the mother. These factors have to be taken into consideration. It is very difficult in this complicated field to say something that is not misunderstood. I recognize that.

Now, will you proceed, please?

Dr. BRUES. The other point about this is that the radiographic pelvimetry is given at one sitting and there is considerable evidence that to produce leukemia it is more efficient in animals to give a dose at one time whereas if it is spread out over several months, as Dr. Upton informs me, he sees no leukemia at all in these mice that get a large amount of leukemia from a single dose.

Representative HOLIFIELD. I don't believe you read the last sentence in the previous paragraph.

Dr. BRUES. While this seems to be a real effect of the dose between two and five roentgens, we must remember that proper obstetrical diagnosis, including the use of X-rays where needed, can be a lifesaving procedure and it must not be condemned without careful weighing of the risks.

It is a safe guess that it probably saves a larger number of cases in labor than it might cost.

Representative HOLIFIELD. These are the things that have to be taken into consideration in the using of X-ray. If it is used without diagnostic reasons, it does indicate as far as these studies are concerned at least, and whether 3 million is enough to get statistical data I do not know, a flag of warning in the use of X-rays, particularly in pregnancy cases.

Dr. BRUES. I think this has resulted in the very interesting facts that now people are looking at the relative risks of these things as perhaps they used to look at the relative risks of surgery. This is a very sound thing indeed. There is no question that a routine chest plate in adult males will save more lives from unsuspected lung cancer than it is going to hurt under the worst assumptions we can make.

Representative PRICE. You just directed your remarks to the question I was going to ask. Since the results of Dr. Stewart's studies and the studies of Tulane University, has there been a lessening of the use of X-ray in pregnancy?

Dr. BRUES. On this subject I can't speak; I do know that the manufacturers of films have made their films considerably more sensitive lately, which has the same effect of giving a smaller dose of radiation from an X-ray picture. I am quite sure there has been an effect, but I cannot document it.

Representative HOLIFIELD. I can say from personal experience having four daughters in my family, all of them child bearing, that I found out on checking that several X-rays were taken of the mothers during the period of pregnancy, and in my opinion the doctor that did it, did it without proper justification.

I think it is going on all over the United States. I believe that the doctors themselves need a little information on cases like this study

of Dr. Stewart to cause them to be a little bit more cautious. It is easy when you have a nice machine and you can charge $20 to press a button.

I think there is probably a little of that going on in the United States.

Senator PASTORE. I think this, Mr. Chairman, has alerted both the profession and the laity as to the dangers involved and I think much more care is being exercised today.

Dr. BRUES. The American College of Radiologists has made a considerable effort to get information printed and in the hands of its members giving a summary of the sort of things which have been brought out here.

Representative DURHAM. Since we are relating personal experience, I am a kind of guinea pig myself because I had a lobotomy 10 or 12 years ago and I consistently have had X-rays. It made me more conscious of the fact of what might happen. Over the past 10 or 12 years I don't know how many roentgens I have taken but I still rely on that and the medical diagnosis. In the last few days I have had the same thing done again. You have to weigh the matter in two ways. You take a chance when you go out in your automobile for a ride. We have to weigh the balance and see what the benefits are. Without the X-rays it would be very difficult to diagnose cancer of the lung, in my opinion. I don't know how you can arrive at it in any other way. There are some benefits as well as danger.

Representative HOLIFIELD. You may proceed.

Dr. BRUES. I wanted to mention that life shortening of American radiologists which was spoken of at the last meeting has now had a little doubt cast upon it by further statistical examination of the information, and by the fact that the British radiologists who have records going back, I think, to 1898 appear not to have their lives shortened in the course of their practice. This is a question, still, but any life shortening that has occurred as the result of the 1,000 or so roentgens that these men have received in the course of their business is a good deal less than had been suspected before.

I wanted to say that I think that the threshold concept has been used a little bit loosely. It has been believed, I think, by most people that either there is a complete threshold or else that there is a straight line which I indicated on the blackboard here. It is my own personal feeling from what I know, and this is shared. I think, by most persons who have investigated experimentally the induction of cancer by many means, that there might be something in between.

Cancer production is a very complicated thing. It has several stages in it. Cancer is sometimes produced, and leukemia, by quite indirect methods that do not involve the direct hit on the cell which has been the basis of the straight line or proportional hypothesis. If it is just a little more complicated, if cancer requires two mutations, for instance, then the situation which the pessimistic guess would call for in hundreds of thousands of cases of leukemia with a certain amount of radiation would probably yield a hundred cases or so and if it is more complicated than that or requires a profound disturbance of tissue and there is some experimental work to bear on that, then there might indeed be something which looks like a threshold. I think that the idea that one must either have a threshold or else a proportional re-

action is one which I would not subscribe to. There are so many other possibilities.

I wrote a paper for Science last year summarizing a good deal of this information which I will submit into the record and there is a brief summary in semilayman language in the testimony which I am going to hand in.

I don't want to leave the impression that we have enough facts to consider the whole matter closed. Two years ago I hardly thought it necessary to say much about this subject because of lack of information. However, because of the great credence which seems to be given the proportional theory at the present time, almost exclusively by scientists who are less familiar with the recent great growth in our knowledge of cancer, I feel it is necessary to put a question back beside it. But I think we must take a careful and conservative attitude toward the fallout problem until the facts are more clearly outlined, especially regarding exposures of large populations. Where general contamination is envisioned there are many reasons to accept a level considerably lower than the industrial permissible level. To be on the safe side we should take into account perhaps the natural radiation which we all receive, recognizing that we have existed in this for ages past and also that it varies widely from one location to another.

For instance, the yearly natural radiation on the east coast is about one-tenth r per year and of the larger cities of Colorado it is about twice this, yet actually New York reports a higher rate of leukemia than Colorado. Persons in some areas of Illinois store about 10 times as much radium in their bones as those in Chicago, but so far we have been unable to detect any differences in the bone cancer rates. This ought to make it clear that even if small changes might occur in the region of the natural background there will be no catastrophe if it is very much exceeded.

I think as a result of some of the things which have been said in public lately a good many people are afraid that the levels set up as permissible cannot be exceeded without great personal danger. I mentioned that before. The radiation from fallout hazard increased a great deal in the past few months. This is not the subject of today's discussion but I have put in my testimony a little summary of some measurements of radiation levels on the ground in soil samples in which we have separated out the various isotopes, and they show the fact that there has been a great increase since last fall which presumably can be attributed largely to the Russian test in October.

Representative HOLIFIELD. Before we leave the former testimony, I have a communication here which says, in regard to the problem of ascertaining the effects of low-level sustained radiation, that this is essentially the problem of fallout. I thought I might mention two instances which bear very heavily on this problem.

The first is the new finding by Dr. John T. Gentry of the New York State health department regional health director at Syracuse that the rate of malformed births is 50 percent higher than the average in parts of New York where natural radioactivity is high.

It seems to me you might like to hear in detail the results of this study. It shows that where the uranium content of the soil is twice as high as the average, the number of malformed births raises to 20 per thousand instead of the statewide average of 13.2.

I do not know Dr. Gentry nor do I know the scope of his study, or the status of his capability, but I bring those figures to you for such comment as you may think is necessary.

Dr. BRUES. I had the opportunity of seeing this a few days after I had written this testimony. I had seen it earlier in tentative form. It seems that malformations are increasing in certain areas of New York. I think the author emphasized that his study is not entirely complete, being based so far on a survey of general geological types as well as a survey county by county of the important malformations. The emphasis there is that this study will be continued to make it a little more definite. My chief doubt about that is the fact that the rate of increase which they report seems to me to be more than could be expected from the increase in background, although the backgrounds have not yet been actually measured, especially if we recognize that there are a good many other things, for example, German measles during pregnancy, which are commonly accepted as causes of malformations of this sort.

I think it is an extremely important study and I think we must keep careful watch of it in order to evaluate it.

Also I suspect this may fall in the realm of genetic considerations rather than somatic, although even that is not completely certain.

I think much of the heat that has been generated in debate over the possible effects of fallout from bomb tests has been motivated by attempts to balance these against the risks of modern nuclear warfare which we all realize are vastly greater. Perhaps this testimony on somatic effects may seem to be rather optimistic. This is only in the context of what I think is an extreme pessimism with which the subject has been widely treated. I believe we must admit that in our present state of knowledge there is a possibility that real somatic damage may be done to certain rare individuals as a result of radiation from fallout levels. It seems to me the source of radiation must be considered in the light of its necessity or usefulness just as we must give thought to the other risks in life but we may safely try to calculate these risks on the basis of the worst that could happen while admitting quite likely as regards somatic damage the risk is less and much less than this. This is my opinion. That we are resonably secure can be illustrated if I go back to a calculation which I made at the previous hearing concerning the apparent hazard of cigarette smoking. I am not sure that cigarette smoking is responsible for lung cancer. It is conceivable that it may be due to pollution of the air from other agents. So this is simply a hypothetical case.

In several countries of Europe it has been noted that the number of cigarettes consumed is in direct proportion to the incidence of lung cancer. If we assume that both this and the production of leukemia by radiation follow a straight line it would appear that the present fallout levels are about as likely to produce leukemia in an individual as two cigarettes a year are to produce a lung cancer, or if we say it is the pollution of the city air, a few hours spent in the city air by a country fellow would to this extent produce lung cancer.

I point this out to put in some sort of proportion the sort of figures we have been talking about. Some very serious questions have been

raised. They will need to be considered carefully. Most of us agree that it would be fruitless to treat animals at fallout levels of radiation because of the enormous numbers required. Perhaps some direct information could be obtained from large populations of human beings. Efforts of this sort are being carried out and should be looked into elsewhere.

It is pretty clear that we are not even certain of the ground rules of the cancer-formation process. If it were as simple and straightforward as the proponents of the proportional theory have held, this ought to have been discovered long ago. Since it appears to be more complicated, it is necessary to make every effort to determine the true relation between dose and tumor expectancy on a variety of species, at levels which give measurable increases. I might say this could very well be done by some of the other cancer producing agents than radiation because, believe it or not, we know more about the shape of the curve with relation to radiation cancer than we do for all of these chemicals which have been studied for a very long time.

I could document that if necessary. Such work would require hundreds of thousands of animals probably and its importance justifies it to the extent that some of this work has been started at the present time. We ought also to look at some fission isotopes which have been neglected in previous studies because certain of them were thought of as the most fashionable to work on. This too is on the drawing boards.

Finally, I think it is important that scientific experts who are speaking on the cancer producing potency of natural fallout might become familiar with some of the large amount of careful work that has been carried out on experimental cancer in its basic aspects so that those of us who worry about these problems can speak the same language.

I am a little afraid sometimes that cancer research has been pushed into a corner and has not become as much a part of the scientific culture of the scientist as it might be.

Thank you.

Representative HOLIFIELD. Your statement will appear in the record at this point.

(Dr. Brues' formal statement together with papers submitted by him, follow:)

STATEMENT OF DR. AUSTIN M. BRUES, DIRECTOR, BIOLOGICAL AND MEDICAL RESEARCH DIVISION, ARGONNE NATIONAL LABORATORY [1]

I have been asked to make a statement that brings into focus the progress in understanding somatic effects of radiation since the hearings 2 years ago. At that time, as you will remember, we had a great deal of detailed knowledge of the effects of large doses of radiation on man and on the higher animals whose responses are similar to those of man. It was brought out in those hearings that a shortening of life occurs as a result of doses of radiation that are not quite high enough to kill an animal in an acute mode of death; also, that various forms of cancer, and particularly leukemia, are brought about by such doses of radiation. The fact that human beings exposed to fairly large doses of radiation (say 100 r) have an increased chance of developing leukemia is beyond question. Similar evidence makes it clear that radium in the human skeleton has been responsible for cancer of the bone.

[1] See professional biography attached.

First, there has been real progress in the treatment of acute radiation sickness—that fatal condition that claimed so many thousands of lives in Hiroshima and Nagasaki, and which has been observed in some of those rare individuals injured in mishaps associated with the handling of nuclear materials and reactors. It has now been found, as the result of treatment of the victims of the Yugoslav reactor accident, that human bone marrow can be inoculated from a normal person into one in a critical phase of radiation injury; that it will grow in the injured person and take over the normal function of supplying the urgently needed blood cell elements; and that dangerous complications of the use of this foreign tissue can be avoided. We are not all agreed as to whether any of these lives were saved by the treatment, because we can never know whether they would have survived without the treatment. But we can all agree that the French physicians who administered the bone marrow transplants, produced a true and striking improvement in the conditions of these critically injured individuals.

It is interesting to recall the scientific background of this form of treatment. More than 12 years ago we were studying the effects of radiostrontium and comparing them with X-rays, and were impressed with the role of the blood-forming organs in recovery from radiation sickness. Jacobson then undertook experiments that showed the effects of shielding certain parts of the body. Lorenz at the National Cancer Institute then got into the work and first showed the life-saving efficacy of bone marrow. Many problems remained in making this a safe and useful procedure and these have been worked out at Argonne, Oak Ridge, and elsewhere. Now, not only is the procedure useful in accidental radiation injury but it looks hopeful as a means of giving large enough doses of radiation to cure leukemia and still save the patient from fatal radiation effects.

Please remember that when this discovery was first made, it grew out of one of the first fallout studies. Work of this sort was not too popular then: there was not much interest in it, and in fact Lorenz was substantially supported by the Atomic Energy Commission through contractual arrangements with the Argonne Laboratory. You might also note that as with most useful discoveries, the original breakthrough was made by a scientist looking for something quite different (specifically, we were observing the spleens of animals acutely poisoned with strontium 89).

While we are mainly interested here in the effects of very low radiation doses from fallout, I have emphasized also progress in the treatment of the acute disease from doses a hundred and more times as high, for this is an enormously more important problem if there were any possibility of a thermonuclear war.

At the time of the first hearings on this subject, I believe you were left with the impression that no matter how low a dose of radiation is given, it is likely that a proportionally low number of leukemias and cancers and a proportionally small shortening of the average life span, will occur. I have to report that there is still no proof or disproof of this proposition and in fact that we cannot see where it might be coming from. This is because of the extremely small proportions of a population that would be involved at fallout levels even under the most extreme guesses. Proponents of both sides of this argument are agreed that it would take millions and more likely billions of mice to settle the point directly. It is possible that there might be two large enough human populations identical with respect to everything but radiation exposure to settle the point: we are seriously looking into this, but it seems doubtful that we could count on a secure answer at these levels owing to the variability of vital statistics as they are collected.

The more fruitful advances in understanding this question have come from a study of the proportionality or nonproportionality of the reactions to radiation at the higher doses where we do have valid information, and by studies as to the basic nature of the processes in the body and its cells, which produce leukemia and cancer.

When we look for proportionality carefully, we generally od not see it. R. H. Mole summarized a large number of such instances in the British Medical Bulletin last year, and I am supplying a copy of his paper for the record. A. C. Upton, speaking before the American Association for Cancer Research last month, demonstrated a number of instances in which cancer or leukemia is produced by radiation, in none of which the relation is proportional, and in which the shape of the relation differs very greatly from one case to another. Our own work with the production of bone tumors by strontium 89 and 90 has always shown a marked dropping off of the number of tumors per dosage as the

dosage is lowered, when we look at the animals at any time after the strontium is given.

While a disproportion is quite generally the rule with lower doses being relatively less effective than higher ones, this is not always the case. Bond and others at Brookhaven found that for a particularly tumor-susceptible rat, there seemed to be a direct proportionality between numbers of tumors and dose of X-ray between 25 and 400 r. Their experiment covered only the first year of life, and there is reason to believe that the radiation was in this case hastening a process of tumor formation that occurs naturally in these animals.

The closest approach to an observation of somatic effect at doses of radiation in the range of fallout or natural intensities is that of Dr. Alice Stewart of Oxford University. She made a study of all cases of childhood cancer and leukemia in England and Wales over a 3-year period, thus studying a population of about 3 million at risk. These children, with cancer or leukemia, turned out to be more likely to have been X-rayed before birth, for purposes of measuring the size of the pelvis, than other children, and suggest that there is a susceptible stage in embryo life when the small diagnostic dose (2 to 4 roentgens) of X-ray about doubles the chances (rather small, it is true) of developing these diseases in childhood. Another study in Louisiana, at Tulane University, has shown about the same results; some others are underway now but are incomplete and are not all yet in agreement. While this seems to be a real effect of a dose between 2 and 5 roentgens, we must remember that proper obstetrical diagnosis, including the use of X-rays where needed, can be a lifesaving procedure and that it must not be condemned without careful weighing of the risks.

It has been thought that these figures may apply to fallout radiation. I would very much doubt that it does, since the dose in diagnostic pelvimetry is given all at once, while that from fallout or the environment comes gradually over the whole life of the embryo. Upton informs me that a single exposure of mice to gamma rays produces a high incidence of leukemia, while spreading the same dose out over several months results in no perceptible increase in leukemia or decrease in life duration.

Shortening of the life of American radiologists as a result of their average occupational exposure to 1,000 or 2,000 r, was thought to be the case at the time of the previous hearings. New statistical studies, taking into account the ages at which men become designated as specialists, has thrown great doubt on this life shortening, and a British study of their own radiologists has shown that they are actually a little better off than other medical men.

One thing that has confused the issue is the use of the "threshold" concept rather loosely. Many people seem to think either that there is a threshold, and below it nothing ever happens, or else that there is a straight-line proportion between dose and number of tumors, or shortening of life. I don't think either of these alternatives has to be true. I think, actually, it is quite likely that there may not be an absolute threshold, but that the line may be a curved one which results—where the dosage is low—is something that we cannot distinguish from one.

Let me try to express this with an example. I will assume that the number of cases of leukemia or cancer increases, not as the radiation dose directly, but as the square of the dose. This would mean that before cancer is produced, two things have to happen to a cell, not just one. Most professional cancer investigations would believe that still more things must happen independently. However, looking over a large number of results of animal experiments, it seems most likely—though not proven—that the information fits this sort of curve better than the straight line which has been the basis of all the various numbers you are offered as the "expected numbers of leukemias" from fallout. This curve would predict that where it has been assumed that the natural or fallout levels might be responsible, say, for 10,000 cases, actually they might be responsible for less than 100. At best, we might at least consider that both views are possible. I am inclined to expect a much smaller number, since the great majority of our data show that the tumor susceptibility is not proportional to dose at those high levels where we can measure it, and also because of a great deal that we know, generally, about the production of cancer. I am submitting a paper which I wrote for Science last year, which summarizes a large amount of detailed knowledge about the origin of cancer which has been generally ignored by those who feel that they are able to predict numbers of leukemias and cancers which, although not capable of direct verification, might be produced by small amounts of radiation.

The theory that malignant disease is proportional to amount of radiation is based on the idea that cancer is due to a single mutation in a body cell, which gives rise to a growing colony of cancer cells. This theory was in vogue a good many years ago and has been carefully considered by cancer investigators. As in the case of many things which scientists take for granted for some length of time, eventually exceptions are discovered which lead to new theories and new discoveries. In the past 2 years basic cancer research has uncovered a number of new observations that do not fit with the mutation theory—at least in its simplest form, the one that predicts proportional responses to radiation. Several forms of cancer may now be attributed to viruses that get into the animal in embryonic or very early life. Upsetting of the immune "identifiers" of tissue cells is another view that a good deal of support. Some brilliant work of the past few years by B. S. Oppenheimer, has shown that the simple implantation of a plastic film—which is not a cancer-producing substance—produces many cancers because it interferes with the whole architecture and fluid flow in a tissue much in the way that a large dose of radiation (but not a small one) does. At the meeting of the Ciba Foundation on Carcinogenesis last summer, recently published in book form, which was attended by about 30 of the foremost cancer investigators in this field, one finds very little sympathy and much specific disagreement with the older, simple view of cancer.

I do not wish to leave the impression that we know enough facts to consider the matter closed. Two years ago I hardly thought it necessary to say much about this subject because of the lack of information on it. However, because of the great credence which seems to be given to the proportional theory at the present time, almost exclusively by scientists quite unfamiliar with the recent great growth in our knowledge of cancer, I feel it is necessary to put a large question mark beside it.

Nevertheless, I feel that we must take a careful and conservative attitude toward the problem of fallout until the facts are more clearly outlined. This applies especially to the exposure of large populations to low radiation levels. Let me therefore draw a distinction between industrial "permissible" exposures and those that might be applied to large populations. Industrial levels are based on ascertaining a level which appears not to have produced visible harm, and then adding in a large factor of safety—a factor of 10 usually. This has been the practice in industrial toxicology for many years. As to isotopes, our best information comes from radium exposures in human beings. At the present time, in New Jersey, New England, and Illinois, hundreds of exposed persons are being followed up without discovering, so far, any injuries at levels below those previously assumed. Exhaustive animal experiments have been carried out to estimate comparable levels of other isotopes.

Where general contamination is envisioned there are many reasons to accept a much lower level. My own personal view is that to be on the safest side we should take into account the natural radiation that we all receive—recognizing not only that we have existed in this for ages past, but also that it varies widely from one locality to another. For instance, the yearly natural radiation on the east coast is about one-tenth r per year and in the larger cities of Colorado is about twice this—yet actually New York reports a higher rate of leukemias than Colorado. Persons in some areas of Illinois store about 10 times as much radium in their bones as do those in Chicago, yet we have so far been unable to detect any difference in the bone cancer rates. This should serve to make it clear that even if small changes might occur in the region of the natural background, no catastrophe will occur till it is very much exceeded. I think as a result of things that have been said lately many people are afraid that the levels being set up as permissible cannot be exceeded without great personal danger.

This is an important fact to keep in mind, since the radiation from fallout has increased a great deal in the past few months. To illustrate this, I am appending to this statement one by Gustafson and Marinelli, of the Argonne Laboratory, based on measurements of the separate gamma ray activities in soil of northern Illinois. This indicates that last month the gamma ray dose rate from the ground was about three-fourths as great as that from natural sources in the ground, plus cosmic rays. The great increase since last fall, according to the testimony of

others, apparently is mainly due to the Soviet tests last October. Of course, most of the activity now is due to the short-lived fission zirconium, so that the total dose over the rest of the year will amount to very much less, and after a few months, when this isotope has decayed, we might expect to have only a fifth or a tenth as much as at present.

Other factors also tend to reduce the dose, namely those of weathering and shielding. The first of these means that in the course of time the radiating material is lost by weathering or buried by cultivation. The second means that for a large part of the time the average person is shielded from the radiation from fallout because he is in or near buildings. These factors do not affect the natural background appreciably as the natural radioelements occur throughout the depth of soil and are also incorporated in building materials. The United Nations Committee has taken an average "weathering and shielding" factor of ten. Accordingly, the dose to an average person would be still lower by such a factor as this.

Of course, I think much of the heat that has been generated in debate over the possible effects of fallout from bomb tests has been motivated by attempts to balance these against the risks of modern nuclear warfare, which we all realize are vastly greater.

On the face of it this testimony as to somatic effects may seem to be in a rather optimistic mood, but this is only in the context of what I think is an extreme pessimism with which the subject has been widely treated. I believe we must admit that in our present state of knowledge there is a possibility that real somatic damage may be done to certain rare individuals as a result of the radiation from fallout levels. It therefore seems to me that sources of radiation must be considered in the light of their necessity or usefulness just as we must give thought to all of the other risks in life; and that we may safely try to calculate these risks on the basis of the worst that could happen, while admitting that quite likely—as regards somatic damage—the risk is less, and probably very much less, than this.

That we are on a reasonably secure basis can be illustrated if I rephrase the calculation I made at the earlier hearing, concerning the apparent hazard of cigarette smoking. In several countries of Europe it has been noted that the number of cigarettes consumed is in direct proportion to the incidence of lung cancer. If the radiation hazard at low levels is proportional to that at high levels, the same is possibly true of the cigarette. Taking into account the present fallout levels it would follow that they are about as likely to produce leukemia in an individual as two cigarettes a year to produce a lung cancer, using the worst guesses in each case.

Nevertheless, some very serious questions have been raised and will need to be considered carefully. Most of us agree that it would be fruitless to treat animals at fallout levels of radiation because of the enormous numbers necessary to produce a perceptible excess of tumors. It is possible that if large populations of human beings receiving various amounts of radiation could be followed, some direct information could be obtained, in any case, efforts along this line are being made in many places and should be encouraged elsewhere.

It is fairly clear that we are not even certain of the ground rules of the cancer formation process. If it were as simple and straightforward as the proponents of the proportional theory hold, this should have been discovered long ago; since it appears to be more complicated, it is necessary that every effort be made to determine the true relation between dose and tumor expectancy, on a variety of species, at levels which give measurable increases. This will require hundreds of thousands of animals at the very least, and its importance justifies it to the extent that it has already been started underway. A new look at some fission isotopes neglected in previous studies should also be taken, and this is on the drawing boards at present. Finally, I think it is desirable that those scientists who speak with assumed authority on the cancer-producing potency of the fallout should become familiar with more of the vast amount of careful work that has been carried out on experimental cancer, so that at least all of us who worry about these problems can speak the same language.

Appendix to the Statement of Dr. Austin M. Brues

Fallout Radioactivity and the Dose Arising Therefrom at Argonne National Laboratory, Lemont, Ill.

(P. F. Gustafson and L. D. Marinelli)

Measurements of gamma-ray emitting fission products have been made periodically on soil collected at the Argonne National Laboratory site since May 1957.[1] Table I shows the activity in mc/mi^2 for Zr^{95}, Nb^{95}, Cs^{137}, Ru^{106}, Ru^{103}, Ce^{141}, and Ce^{144}. These data indicate the continuing accumulation of Cs^{137}, doubling in activity per square mile from October 1957 to September 1958, and doubling again from September 1958 to April 1959. Because of its long half life (26.6 years), Cs^{137} in soils is a reliable indicator of the total fallout accumulated at any given locality. It is also to be expected that Sr^{90} will accumulate in a similar fashion (i.e. when Cs^{137} doubles, so will the amount of Sr^{90} activity), though the activities will not be equal. Investigations of Cs^{137} at Argonne using soil samples analyzed for Sr^{90} at HASL indicate an average total Cs^{137}/total Sr^{90} activity ratio of 1.6 ± 0.3 for various types of soil (3). Accordingly, one would expect the Sr^{90} activity to be roughly 0.6 that of Cs^{137}. The wide variations in the activity of the shorter-lived fission products, Ru^{103} and Ce^{141}, reflect qualitatively detonations of nuclear weapons preceding sampling by relatively short intervals of time. Besides fission products, activated elements such as Mn^{54} and Zn^{65} were present in minor amounts; W^{185} became apparent in the September 1958 soil sample, and measures 45 mc/mi^2 in April 1959. Air filters collected on the site set the appearance of W^{185} in Chicago as being in the latter half of July 1958.

The dose arising from the activities presented in table I, obtained using the method of Dunning for radioactivity on an infinite plane (4), are given in table II. To place the external dose rate from fallout in the proper perspective it should be compared with that arising from natural radioactivity and cosmic radiation which amounts to about 11.1 μrad/hour at this site (table II).

It is worthy of note that the fallout dose rate in April 1959 was 8.41 μrad/hour, a full 75 percent of that from natural sources and that the average dose rate for the interval May 1957 to September 1958 was 1.95 μrad/hour or 20 percent of natural external background. This relatively sudden fourfold increase in dose rate seems to be due primarily to the Russian tests of September–October 1958.

The values are expressed in terms of microrad per hour and net millirads per year to emphasize that the dose over a long period will not be at all close to the dose from natural background. Calculating the 30-year gonadal dose from the cesium 137 now on the ground, including the United Nations Committee's factor of 10 for shielding and a factor of 0.71 for the decay of the isotope, gives a figure of 8 millirads as against 3,000 from natural sources. Nevertheless, the rapid increase in recent values must be noted seriously.

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[1] The measuring technique employed is that of gamma-ray spectrometry of soil samples of finite size (1–2 kg) using large NaI crystals (1). Background reduction, essential for the detection of the quantities of activity involved (approximately 10^{-10} curie Cs^{137}/kg of soil) was effected through the use of thick steel shielding (2). Soil sampling was done over a known area to a depth of 6 inches. Quantitation of fission and natural radioactivity was accomplished through the use of sources of known activity incorporated in mock soil (Na_3PO_4); the activity pertaining to each radionuclide was then found by the solution of the appropriate number of simultaneous equations.

TABLE I.—*Fission product radioactivity on the ground at Argonne National Laboratory in mc/mi*[2]

Isotope	1957			1958			April 1959
	May	July [1]	October	April	July	September	
Zr^{95}–Nb^{95} [2]	180	195	230	200	95	315	1,020
Cs^{137}	35	34	37	45	60	82	170
Ru^{106}	175	180	180	170	150	210	830
Ru^{103}	175	200	240	130	35	80	230
Ce^{141}	110	120	140	60	15	50	135
Ce^{144}	240	260	275	250	215	270	1,520
Total [3]	1,095	1,184	1,332	1,055	665	1,322	4,925

[1] Soil collected from site 40 miles from Argonne in July 1957 gave essentially same results as the Argonne samples, indicating that possible local contamination due to the reactors at Argonne is of secondary importance in this study.
[2] Assuming Zr^{95}–Nb^{95} are in equilibrium.
[3] Totals include activity due to both Zr^{95} and Nb^{95}, i.e., May 1957, 360 mc/mi[2] from both these isotopes.

TABLE II.—*Dose from fission product activity on the ground at Argonne National Laboratory in μrad/hour*

Isotope	1957			1958			April 1959
	May	July	October	April	July	September	
Zr^{95}–Nb^{95}	1.14	1.23	1.46	1.27	0.60	2.00	6.48
Cs^{137}	.09	.08	.09	.12	.15	.21	.43
Ru^{106}	.15	.15	.15	.14	.13	.18	.71
Ru^{103}	.35	.41	.49	.27	.07	.17	.48
Ce^{141}	.01	.02	.02	.01	.01	.01	.03
Ce^{144}	.04	.05	.05	.04	.04	.05	.28
Total	1.78	1.94	2.26	1.85	1.00	2.62	8.41

External background radiation at Argonne National Laboratory [1]

	μrad/hour
Thorium	2.53
Uranium	2.65
Potassium	2.77
Cosmic radiation (5)	3.17
Total	11.12

[1] Dose from thorium, uranium and K^{40} calculated from γ-ray determinations of the concentrations of these radioelements in Argonne soil.

Critique of the Linear Theory of Carcinogenesis

Present data on human leukemogenesis by radiation indicate that a nonlinear relation is more probable.

Austin M. Brues

A question which has interested investigators of cancer for many years is whether various carcinogenic agents are active even in very small amounts, or whether there are amounts or concentrations of such an agent below which no effect is produced, or below which the effectiveness drops off out of proportion to the reduction of dosage. The existence of a threshold, or at least of a marked nonlinearity of effect at low dosages, is rather taken for granted in most areas of toxicology, where the degree of general physiological impairment seems to determine whether or not an end point such as death or a persistent pathologic change is produced, and where it is reasonable to assume that essentially complete repair can occur if the insult is removed or if its intensity is low.

It has been thought that a different situation might exist where cancer is the end point. The main reason for this lies in the special nature of malignant disease, which in its natural course leads inevitably to death, yet which ordinarily arises in a small anatomical focus, perhaps in a single cell. On this basis, then, it may be conceived that cancer could arise through a single randomly determined alteration in a cell of the host, which conferred on it the capacity to reproduce itself indefinitely. By analogy to the point mutation in genetics, we can see that a theory postulating a linear relation between the amount of a carcinogenic agent and the probability of a malignant response is allowable even where the respective amounts and probabilities are vanishingly low. The purpose of this article (*1*) is to examine whether such a relation has been verified (as some seem to assume) or indeed whether it seems reasonable in the light of present information.

While a generalized hypothesis of linearity of the carcinogenic response can apply equally to many types of malignant disease and to many agents, perhaps the strongest argument in favor of this hypothesis has been presented by Lewis (*2*) on the basis of several published studies of leukemia induction in man by ionizing radiation. The ensuing discussion will deal mainly with this special case, but it will be clear that many of the considerations presented apply equally well to the more general case of cancer induction by a variety of agents.

There is no question as to whether leukemia may be a result of total-body irradiation, or of irradiation of large masses of blood-forming tissue, at high dosage levels—that is, at levels at or above 10 or 20 percent of the acute lethal dose, where widespread cellular destruction occurs in both lymphoid and myeloid tissues. This has been well established in work with animals and is obvious as well from the human data to be discussed here.

The Human Evidence

The data on leukemia incidence in the irradiated populations of Hiroshima and Nagasaki (*3*) suffer from the fact that they have been calculated on the basis of concentric circles at 500-meter intervals from the hypocenters. Within each sector defined in this way, the range of radiation dosages is very large. For the area up to 1500 meters from the hypocenter (that is, the area where dosage to unshielded or slightly shielded persons is currently estimated as 125 rad or above), the evidence for induction of leukemia is plain, and it is clear that the incidence increases the closer a person was to the hypocenter.

In the critical area, from the point of view of this argument—that from 1500 to 2000 meters from the hypocenter—the leukemia incidence has appeared likewise to be increased, although the increment was, in absolute numbers, not more than four or five cases. Such preliminary data as were available until recently (*3*) indicated, in fact, that the increment is limited to the nearest quartile of this area—that between 1500 and 1625 meters—and that those cases seen from the area beyond 1625 meters occurred with a high probability in persons who had acute radiation symptoms (*4*). This would suggest that these individuals received substantially more than the estimated dosage, or else that other factors leading to acute aplastic anemia were of importance in the subsequent development of leukemia. It may be concluded that no increased incidence of leukemia following a smaller dose than 100 rad has been demonstrated. Publication of more recent figures (*5*) shows that the incidence of leukemia within the 1500- to 2000-meter sector equals that in the control sector within one standard devi-

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ation and further emphasizes the uncertainties of present dosimetry. The present data are at least as compatible with a physiological mechanism linking leukemogenesis with initial severe hematopoietic damage, or with some other nonlinear relation, as they are with one assuming an effect that is strictly linear with dose.

The best documented evidence of increase in leukemia incidence with increase in radiation dosage is that detailed by Court-Brown and Doll (*6*). These investigators made a painstaking study of patients irradiated over the spine and elsewhere to alleviate the symptoms of ankylosing spondylitis, and the individual doses have been carefully calculated. It is noted that the calculated mean spinal marrow doses show a *curvilinear* relation to leukemia incidence. Something resembling a *linear* relation can be deduced only by discarding those cases in which extraspinal irradiation was also given. The effect of this procedure is to eliminate almost all of the cases receiving a high dosage—those which most clearly demonstrate the nonlinearity of the function. It is indicated that these cases were omitted because high doses elsewhere may have involved other marrow areas; however, since the calculated integral body doses also show a curvilinear relation to leukemia, the reason for this treatment of the data is not at all apparent. Indeed, the authors state that they cannot rule out a threshold but suggest that a linear relation without threshold constitutes a good "working hypothesis."

While the actual data indicate that this working hypothesis is a less probable one than one that assumes a threshold or a nonlinear relation, it is illuminating to make a careful study of the clinical protocols. First, of the 32 cases accepted as leukemia after careful study of pathologic material, only 22 were so certified at death, and another five cases were classed as suspicious but lacking a final positive diagnosis. Many more cases of aplastic anemia were observed within the same range of dosages. Those who have had experience with clinical leukemias will be impressed by the high proportion of equivocal cases. If one is willing to forego the statistical requirement of assigning each case to a single disease category, it is obvious that many of the cases illustrate the unfolding of a sequence of pathologic changes in which a persistent disordered or aplastic state of the marrow is a precursor rather than a consequence of leukemia. This is likewise true of other agents similarly affecting the bone marrow, notably benzol (*7*).

All of the cases of leukemia arising in this series followed doses exceeding 450 r to the spinal marrow, with one exception. In the exceptional case, the leukemia was of the lymphatic type and the patient had been more heavily irradiated in extraspinal areas.

In other series of cases where leukemia has resulted from irradiation, there is no information bearing on the question of linearity. Children receiving treatment to the thymic area have shown an incidence of about 0.5 percent (*8*). This is true in both dosage groups—above and below 200 r. There has been no further breakdown of the dosages, but it may be presumed that all irradiations were severely damaging to thymic tissue, since the purpose of treatment was to produce involution of the presumably enlarged thymus. Cancer of the thyroid is also frequently preceded by thymic irradiation above 200 r (*9*), but it is possible that complicated endocrine interrelationships may be operative in this instance.

It has been noted for some time that American radiologists are more prone to leukemia than other physicians (*10*). The average dose sustained by this group has recently been estimated as about 2000 r delivered over many years (*11*), but no particular distribution of individual dosages can be assumed, and it is certainly not justifiable to use any estimated average dose in an argument concerning linearity unless the distribution can be established. Since the average accumulated dose to radiologists seems to have been far in excess of the single midlethal dose for man, and since the increment in leukemias is much less than that observed in the most heavily exposed Hiroshima survivors, it would seem that a gradually accumulated dose is much less effective than the same dose received at one time. This is in accord with experimental data, and its significance will be discussed later.

The most impressive survey indicating that leukemia may follow rather low single doses of x-ray is that of Stewart *et al.* (*12*). In a partially complete survey of childhood leukemias in England and Wales over a 3-year period, it appears that abdominal radiography during gestation (generally radiographic pelvimetry) about doubles the probability of development of leukemia, and also of various forms of cancer, in the first 10 years of postnatal life. While the fetal radiation doses would be assumed to be small, they are quite variable even in the instances so far reported (*13*), and in a survey of an entire country, they almost certainly cover an even wider range of techniques and dosages, perhaps including a certain number of fluoroscopies. Pending the accumulation of further data (for example, regarding the variables of sex, order of birth, and domicile), it can only be said that these data reaffirm that radiation is mildly leukemogenic, but that they add little to the linear hypothesis. Certain criticisms of the study have been brought forward (*14*), particularly to the effect that radiographic procedures may be based on particular medical indications which may bear some causal relation to leukemia in children. Another recent study (*15*), while not taking into account radiographic pelvimetries, demonstrated that allergic states in the mother (including the use of antihistaminic drugs) predisposed to the development of childhood leukemias.

There has been a formidable increase in leukemia incidence in the United States population in the past few decades; it more than doubled between 1925 and 1940 in all classes (white and nonwhite males and females) (*16*) and continues to increase. The increment certainly is relatively greater than that in the average population exposure to radiation in the same period. A part of this increase is no doubt due to improved diagnosis, since, as was mentioned above, a diagnosis of leukemia is not always simple or obvious. While this steady increase has been loosely attributed to an increase in human irradiation (*17*), there seems to be no doubt that many other potentially leukemogenic agents have likewise had increasing impact on man, none of which (except perhaps benzol) has been seriously considered from the standpoint of possible leukemogenic action. The suggestion has been made (*18*) that, as a result of reduced mortality from infections, persons sustaining damage to the bone marrow are increasingly likely to survive to develop leukemia.

It would appear from the foregoing discussion that, in the only series of data where linearity from zero might be demonstrated (*6*), it has indeed not been shown. It is also apparent that a critical analysis of the data fails to establish any human leukemogenic response below about 100 r, although, on further analysis in detail, the data of Stewart (*12*) might conceivably establish such a response in the special case of the fetus receiving a single, nearly instantaneous dose. Various alternative hypotheses can be constructed which conform to the existing

information as well as, or better than, this one.

There is relatively little reliable clinical information concerning other forms of malignant disease in the same context. The American radiologists, who have shown a considerable propensity to leukemia, have not shown any increased tendency to tumors of bone, although at the voltages used in diagnostic radiography, the bone-forming cells may be expected to receive several times as much ionization as do the soft tissues. Studies of juvenile cancer of the thyroid have shown that it usually occurs in persons who have had previous irradiation of the thymus, which, as mentioned above, entails a dose of about 200 r. In Stewart's studies relating childhood leukemia to diagnostic procedures on the gravid mother, a similar relation was found with other forms of malignant disease occurring before the age of ten.

Experimental Studies

In spite of the extensive work that has been carried out in experimental carcinogenesis, there is remarkably little which suggests the possibility of a linear dose-response relation—the most obvious one to look for—while there are many instances in which the response is clearly not linear. One recent review of the subject, referring to radiation carcinogenesis, states that "none of the animal experiments have indicated a linear relationship between tumour incidence and dose" (*19*).

Among the difficulties encountered in such studies are: that there is a natural "spontaneous" incidence of all or most tumor types; that there is a "latent period" which may be a significant fraction of the life span, and which appears to increase as the dose of the carcinogen is lowered; that induced tumors may continue to develop throughout life; that the spontaneous incidence usually increases with age; and that verification of a single tumor ordinarily requires careful microscopic study of well-preserved material (this is particularly true of the leukemias).

One clear instance of a nonlinear response is that of lymphoid tumors in mice induced by total-body x-ray (*20*). Here the latent period is short (about 100 days) and the response runs its course well within the life span of the species; also, the natural incidence as a function of age is well established. Under these circumstances it has been shown that the number of additional tumors increases more than tenfold as the x-ray dose is increased by a factor of three.

Further evidence of the nonlinearity of this form of leukemogenesis is seen in a study of mice irradiated by doses ranging as low as 16 rem (*21*). While the data are shown only in graph form, it is clear that incidence of thymic lymphomas rises very steeply at the higher doses and cannot be extrapolated linearly to zero. Myeloid leukemia is relatively rare, and the data (involving, apparently, less than a dozen cases at the lower and control levels) seem to allow the possibility of a linear no-threshold function but not to prove it (*22*).

There are no studies of the dose-response relation in chemical carcinogenesis that have been extensive enough to settle the point under discussion. Careful scrutiny of the data of Bryan and Shimkin (*23*) suggests a threshold for one carcinogenic hydrocarbon but not for another, but in neither case were enough animals exposed at lower doses to establish the true nature of the function. Graffi (*24*) has presented data showing a marked threshold when dimethyl-1,2-benzanthracene is painted on mouse skin, while if croton oil is also administered, a linear relation appears at daily doses of between 10 and 100 micrograms of the carcinogen.

Induction of bone tumors by divided doses of strontium-89 was demonstrated some years ago to be markedly dose-dependent (*25*). In this instance the rate of tumor development was found to be proportional to dose and to the time after the end of a latent period. I interpreted these data to indicate that each increment of radiation confers an equal probability of tumor development that is indefinite in time, *beginning after* a latent period which becomes longer as the dose rate is reduced. In a sense, this is a linear response, but in the context of the mutation theory the latent period becomes meaningless, since a simple somatic mutation theory implies equal responsiveness of any single cell receiving a certain point mutation (necessarily basic to a linear response at low doses). If one calculates numbers of tumors at any given time after the onset of irradiation, a markedly nonlinear function is seen.

Perhaps the only recent experiment in which a linear relation is more than remotely possible is that of Bond *et al.* in which mammary tumors (benign and malignant) were induced in Sprague-Dawley rats by doses of total-body x-ray of between 25 and 400 r during the first year of life (*26*). Since these tumors are very common in females of this strain at a year or more of age, and since the numbers produced in this experiment are equivocally small, linearity cannot be proved even within those limits, and the possibility of an acceleration of a normal process has not been ruled out.

Ultraviolet light is consistently effective in the production of skin cancer in man and other animals. Blum (*27*) has analyzed the results of several time- and dose-patterns of irradiation and has failed to demonstrate any responses that fit with a linear hypothesis. The response is markedly time-dependent and requires continued exposure, and its nature indicates rather conclusively that a sequence of radiation-induced changes must take place before a tumor will appear.

For a single-event theory to hold water implies that the response must be independent of dose rate. Lack of dose-rate dependence has been one of the crucial proofs of the linearity of the genetic point mutation. No cases of strict equality of the carcinogenic response at different dose rates have apparently been reported. As a rule, the response drops off with lengthening of the total time of exposure. This is seen clearly in rats exposed to external beta irradiation in single or daily doses (*28*). In the case of lymphoid tumors in mice, the induction rate appears to pass through a maximum when irradiation is distributed over a period of a few days, falling off on either side of this optimal rate (*20*).

There are many examples of the induction of malignant disease through mechanisms which are clearly indirect—that is, where irradiation of a cell can be shown not to be the critical factor. It has been found, for example, that irradiation of the mouse ovary results in ovarian cancer only when all of the ovarian tissue has been irradiated, so that pituitary gonadotrophins are evoked and stimulate this tissue to hyperplasia and, eventually, to abnormal growth (*29*).

A striking example of the indirect mechanism is seen in induction of lymphoma in mice, which, as mentioned above, is clearly not a linear response. Mice in which lymphoma is readily induced by total-body irradiation can be almost wholly protected by shielding part of the body (*30*), or by irradiating the anterior and posterior halves of the body a few days apart (*31*). Since lymphoma can also be prevented by the administration of bone marrow following total-body irradiation (*32*), it seems likely that a prolonged depression of the

whole blood-forming system is more critically necessary to its development than irradiation of the cells. It has, in fact, been noted that while lymphoma can be prevented by thymectomy in certain strains, it may develop in an unirradiated thymic transplant in an irradiated mouse (*33*).

In addition to these and other evidences of indirect physiological mechanisms intervening between tissue irradiation and malignant change, there is a large body of evidence indicating that the malignant transformation occurs after a sequence of "precancerous" stages has taken place. The most widely observed example is in the development of skin cancer, which, in whatever way it is produced, is likely to be preceded by various types of benign atrophic or hyperplastic states; in experimental studies it most often develops in a benign papilloma. It has long been known that in rabbits treated with tar, a large number of papillomas is produced but only a very occasional one proceeds to malignant change. Glücksmann (*34*), studying precancerous tissue changes, has observed that, following local irradiation, there is a long succession of destructive and proliferative changes culminating finally in cancer; this is in contrast to induction of similar tumors by hydrocarbons, where, if the treatment is intensive, the cancer may appear almost at once. It appears to be impossible, by increasing the dose of a radioactive agent, to reduce the latent period of carcinoma or sarcoma below about 6 months (*35*). Many other lines of evidence point in the same direction: the statistical studies of Blum (*27*) on ultraviolet-induced tumors; the pathologic studies of Foulds (*36*) on spontaneous breast cancer; studies on cocarcinogens (for example, croton oil) (*37*); Tannenbaum's observations (*38*) concerning the different effects of diet in the early and late phases of carcinogenesis; and a number of recent studies on the gradual process by which a tumor develops invasive characteristics. The entire question of the complexity and apparent multistage nature of carcinogenesis has been discussed at length by others, including Huxley (*39*) and Oberling (*40*).

The Somatic Mutation Theory

It has been natural to think of the cancer cell as a mutant of the normal tissue cell. What sort of mutation or combination of mutations it may represent has, however, defied intensive and prolonged study. In the last analysis, cancer is defined by its interaction with the normal tissues—by its "invasiveness" or ability to metastasize in the original host, or to transplant into a genetically nearly identical recipient or into an immunologically inert environment such as the anterior chamber of the eye (these criteria, incidentally, become satisfied only in a relatively late stage of a morphologically identified cancer). Morphologically, it is characterized by variability of cell size and by irregular mitoses; on careful cytological analysis, by aneuploidy and the presence of supernumerary chromosomes.

The unequal mitosis was first clearly recognized by von Hansemann in 1890 (*41*), and Boveri later (*42*) emphasized that this might have significance in the etiology of tumors. The concept of the somatic mutation grew, with increasing sophistication in genetics, and was first spelled out by Whitman in 1919 (*43*). Muller, in a brilliantly concise paper in 1927 (*44*), first suggested a possible relation between the mutagenic and carcinogenic actions of x-rays. In the past 30 years, workers in cancer research have kept the somatic mutation theory under scrutiny, but no definitive test of it has been achieved.

It is important to keep in mind that the somatic mutation theory is amenable to a variety of interpretations. In the sense that it is merely a restatement of well-known facts, it has little meaning to the present argument. Only if it is defined restrictively as referring to a single point mutation or similarly unique event does it imply a linear relation between cancer incidence and the amount of a mutagenic carcinogen. In the event that particular sorts of chromosome rearrangements or a certain combination or succession of mutations are necessary, a linear relation is negated. Careful consideration of the evidence has led to even broader interpretations of the theory—namely, that the critical changes occur through interaction of enzyme-determining genes, plasmogenes, and substrates (*45*). The view that cytoplasmic changes are important is supported by the fact that the chemical carcinogens appear to be fixed to cytoplasmic rather than to nuclear constituents (*46*).

Efforts have been made to find a correlation between mutagenic and carcinogenic potency of various agents and have, in general, met with exceptions in both directions (*47*). The entire matter of the somatic mutation theory has been carefully reviewed by Burdette (*48*), who finds little evidence favoring a single cell-mutation process. The remainder of this discussion, except where otherwise indicated, will refer to the point mutation in a single cell as the carcinogenic determinant, since that concept is necessary to the linear theory.

I have previously taken occasion (*49*) to direct criticism against the somatic mutation theory on the basis of the mutation rates it implies, particularly when animal species of greatly differing size are considered. While the genetic mutation affecting an individual is clearly traceable to an event occurring in a single cell, the somatic mutation which leads to a tumor in an individual would be presumed to have occurred in any of a very large number of cells of the parent tissue. A postulated rate of human leukemia development of around 10^{-8} per year per roentgen (*2*) (since there are perhaps 10^{11} proliferating myeloid cells capable of mutating) yields an estimated somatic cell mutation rate of about 10^{-17} per year (*50*). Similarly, the "spontaneous" cell mutation rate to leukemia on these assumptions must be very low. A little computation shows that if cancer is a result of a particular mutation in a single cell, a local radiation-induced perturbation in a given molecule is extremely unlikely to produce a carcinogenic somatic mutation; or, expressed in older terminology, such a mutation must occur in an infinitesimally small target area (*51*). In more modern genetic terminology, the "penetrance" of such a mutation must be so small as to require some special interpretation.

The situation is still more difficult to rationalize when we consider the implications of the fact that different species, such as mouse and man, have roughly equivalent cancer rates (spontaneous and radiation-induced) but that the number of cells from which cancer presumably can arise differs by a factor of more than 1000. This applies if mutation rates of somatic cells are properly to be calculated per generation of the species; if they were to be calculated on the basis of time, an additional factor of about 30 would be introduced. With reference, for example, to myeloid leukemia, it is apparent that the rates per mouse and per man are the same within an order of magnitude (*52*).

One possible way out of this dilemma would be to adopt the assumption that somatic mutation rates are much lower in man than in smaller, shorter-lived animals. In view of the great structural and functional similarities between somatic cells of the various species, and since no differences exist in the genetic mutation rates (spontaneous or in-

duced), this seems highly improbable. Another possible escape would be to assume that only a certain small number of cells (say one) in a given tissue of any species was capable of undergoing a mutation to cancer. Such circumstances as those, however unlikely, might serve to explain why certain species are able to attain greater size and age span than others. I feel, however, that the burden of proof must rest on those who are attracted to the somatic point-mutation hypothesis because of its superficial simplicity, and that they are the ones to take those difficulties into account.

If we are to accept the rather apparent fact that a large number of cells are potentially capable of mutating to cancer cells, and that therefore the somatic *cell* point-mutation rates must be enormously lower than those encountered in genetic mutations at a single locus, we are led to the conclusion that cancer is a very improbable result of a single mutation and, therefore, that other events are necessary in addition: perhaps one or more additional mutations occurring in the same or nearby cells, or other, physiological determinants. In the first instance the dose-effect relation will be a curvilinear one representing a power function of dose [the data of Court-Brown and Doll (*6*) actually fit a square-of-dose relation much better than they do a linear one]. The species discrepancy might then be explained on the basis that the functions are of a different power—that is, that different numbers of "hits" are necessary to produce cancer in different species; this has indeed been suggested to account for various spontaneous cancer rates in several species (*53*).

Considerable attention has been given recently to the natural age incidence of cancer as well as to that of gross mortality. It has been known for over a century (*54*) that human mortality rates tend to increase exponentially with age, indicating a mathematical function (the Gompertz function) of what may be called the aging rate. In recent years, with advances in medical means of combating infections and other diseases of early and middle life, this function is increasingly evident in vital statistics (*20, 55*). In animals of shorter life span, the mathematical function remains the same but the time scale is shortened, so that the doubling time of the mortality rate is about ten years in man and 100 days in the mouse.

A similar age-incidence function applies to many diseases, including the various forms of cancer (*55*). In the latter case, it has been noted that there is an early peak in childhood, the height of which varies with the tumor type (this is essentially the only component in some neuroblastic tumors and in Wilms' tumor of the kidney), suggesting that there may be two basically different mechanisms of "natural" carcinogenesis. It is apparent that the cancer curves decline from the exponential slope in extreme old age; this may be an artifact of diagnosis or it may represent an effect of senility on the process; it has also been pointed out that the curves may better be fit to a power function of time, as if a multiplicity of events is necessary (*53*).

It is difficult to explain the age incidence by a single-mutation mechanism of carcinogenesis; this certainly cannot be done if we are to assume that radiations (or similarly acting chemical agents) are totally responsible, through such a mechanism, for forms of cancer in which the exponential age incidence is observed. One possible mechanism involving mutational change has been suggested by Armitage and Doll (*56*), who postulate that an initial change gives rise to a clone of exponentially growing cells which are subject to a further, carcinogenic change. This, if applied to radiation, would imply an exceptional effectiveness of widely spaced dosages, which does not appear to have been observed. Other explanations of the natural age incidence based either on an exponential or a logarithmic curve have also postulated multiple independent events (*57*).

Alternative Hypotheses

It has already been made clear in this discussion that a linear theory resting on the somatic mutation is not valid if more than one mutation, or if some nonlinear phenomenon such as a chromosome rearrangement, is necessary. This may be generalized by saying that a linear theory is valid only if a single factor in the process is linearly responsive to the carcinogen (for example, radiation), while all others (if any) are quite independent of it. Such complications may indeed exist in radiation mutagenesis in the mouse, where neither dose nor dose rate shows a clear linear relation to genetic mutation rate (*58*).

Cocarcinogens. There is much to suggest, in experimental carcinogenesis, that "latent" tumor cells are produced by one process and brought to a malignant state by a cocarcinogen. The most potent such agent is croton oil. In the mouse, this agent brings to rapid fruition many tumors that are potentially created by a chemical carcinogen (*37*). It has been found that croton oil abolishes the threshold which is observed when only the carcinogen is applied (*24*). On the other hand, repeated applications of the carcinogen have been more effective in producing malignant tumors than a single application followed by the cocarcinogen (*59*). Where croton oil treatment follows a radiation exposure, only an increase in the number of benign papillomas has been observed (*60*).

Role of cell division. It can be envisioned that a number of cell divisions may be necessary in order for a carcinogenic mutation to manifest itself; such phenomena have been observed in other fields of genetics. This could explain the rather high frequency with which malignant changes occur in tissues that are cultivated over a period of months (where the mass of tissue is small, but where more successive cell generations occur than in the achievement of the cell mass of an adult).

In this instance, it will be noted that massive destruction of tissue results in regeneration of cells and that this process is observed repeatedly during the "latent period" following carcinogenic irradiation of tissue. This is difficult to fit to a linear hypothesis. In the first place, Puck and Marcus have demonstrated that cell death is not linear but is better described by a two-hit curve (*61*); also, the number of cell generations required in repair would more or less be an exponential function of the relative amount of destruction.

Carcinogenic potential of a single cell. It may be observed that a single cancer cell is unlikely to give rise to a tumor. This is dramatically shown by the extraordinary frequency with which cancer cells are seen in relatively small samples of blood draining a human tumor—for example, in 10-milliliter samples from over 20 percent of patients, including many which are not showing metastases (*62*). In highly autonomous experimental tumors, under favorable conditions, single cells may transplant to give tumors, but under most conditions large numbers of cells must be inoculated for a "take."

Physiological environment. It has been generally believed that the tissue environment is important in determining cancer cell growth. This concept has many facets. The proliferation of single cells in tissue culture, for example, requires that they be closely confined, or

that they be explanted with a large number of other cells, or that they be in a "conditioned" medium in which other cells have been cultivated. Again, as was described many years ago by Willis (*63*), there is little evidence from detailed histologic work that would lead one to believe that cancer arises in a single distinguishable cellular focus, since it is usually multicentric in its earliest visible stages. Biochemical studies have led Warburg (*64*) to suggest that the tumor arises in some way adaptively to an environment in which oxidative processes are interfered with. Experimentally it has been found that small tumor inocula take more readily in altered tissue, such as in the liver damaged by chloroform (*65*), while experiments employing skin transplantation have shown the same thing in the converse: carcinogenically treated epidermis fails to produce tumors when it is transferred to a fresh site (*66*).

While these observations are somewhat varied and point in a number of directions (and there are many more such), they all point away from the simple concept of a single mutation operating free of other influences which depend on the carcinogen. To take radiation as an example, a viable disorder of tissue architecture and of its vascular supply is universally characteristic of those dosage levels at which cancer is an observed end result. It should not be forgotten that in a number of other situations where such disorder is the chief recognizable change—as, for example, following thermal burns—cancer frequently arises.

While the concept of a multiplicity of single mutation-like events leads necessarily to a nonlinear relation between dose and cancer incidence, the added concept of a state of tissue disorder as a requirement of tumor appearance implies that a true threshold can occur. There seems to be no direct evidence of any sort that can rule this out.

Enzyme deletion and similar mechanisms. Considerable evidence has been brought forth recently which indicates that cancer may very well not be due to a gene mutation mechanism at all, but to a special situation determined by cytoplasmic (that is, plasmagene-dependent) conditions leading to deletion of certain enzyme-forming systems (*67*). Evidence for this is found in the fact that many chemical carcinogens are fixed to cytoplasmic proteins; that the enzyme patterns in tumors are characterized mainly by various deficiencies; and that changes in enzyme patterns—in particular, development of new pathways—can owe their origin to shifts in the concentration of various substrates. This condition can occur through known positive and negative feedback systems. Perhaps the most important of these involves pathways, only beginning to be defined, by which the formation of thymidine is kept under control (*68*). A failure of the feedback mechanism which normally retards thymidine synthesis might be sufficient to give rise to cancer as it is clinically recognized and defined. Such a mechanism of carcinogenesis would explain the necessity for a particular substrate concentration, hence for a group of potential cancer cells or of a specially abnormal tissue milieu. It would likewise predict—unless such systems are as unique in each cell as are single genes—a multiplicity of "hits," involving in addition, perhaps, a competition between normal and abnormal plasmagenes for supremacy. Haddow (*69*) suggests a nuclear site for the development of enzymatic changes or deletions altering growth control, perhaps in the heterochromatin, and Green (*70*) has developed evidence for a theory that the loss may be of certain immunologic identifiers. While a single-event process might be consistent with some of these mechanisms, I feel that many of the considerations given above cast great doubt on this possibility.

The virus theory. Brief mention may be made of the theory, for which there is some experimental evidence, that there is a provirus which may, through action of a carcinogenic agent, be altered to produce a tumor agent in the cytoplasm. Such virus-like agents include the milk factor and the leukemia agent (*71*). The possibly analogous production of infective agents from lysogenic bacteria has been shown by Marcovich (*72*) to be linear with a large range of radiation dosages. Again, the great rarity of carcinogenesis as a cellular change appears to be strong evidence against accepting this as a single-hit cause of cancer; also, the type of leukemia in which this sort of agent has been demonstrated, the lymphatic leukemia of the mouse, is the most notorious instance of a nonlinear radiation response.

Discussion

It has been suggested that strontium-90 from fallout might be linearly responsible for a very low (but in absolute numbers, appreciable) incidence of leukemia. It has been further suggested that very low-level increments of carbon-14 might (due to its 5000-year half-life) result, in many thousands of years, in calculable, if not determinable, numbers of leukemias. From the foregoing discussion it is deduced that this seems most improbable. Moreover, strontium-90 can, in man, affect only a very localized part of the marrow, and at a dose rate which is extremely low.

This review has necessarily included only a small part of the literature pertinent to this subject; the evidence offered against linearity at low doses must be taken only as illustrative, while the evidence in its favor has been discussed rather completely. With present experimental evidence failing to demonstrate linearity even in genetic mutations in the mammal, it would not seem reasonable to give undue credence to linearity in the much more complex matter of cancer production. The reader is encouraged to examine some recent thoughtful reviews on the subject (*73*) before accepting a simple theory of carcinogenesis.

Summary

1) Present data on human leukemogenesis by radiation fail to indicate a linear relation between dose and effect. Because data are scanty, such a hypothesis cannot be ruled out statistically, but it is less probable than a nonlinear or threshold relation.

2) Other instances in which carcinogenic agents have been examined from the standpoint of dose and dose-rate relations show many clear instances where the relation is nonlinear and none in which linearity is unquestionably demonstrated.

3) Theoretical consideration of the probability that a single critical molecular event, such as a mutation, will give rise to cancer indicate that a malignant change must be an extraordinarily improbable result of such a perturbation. It is also very difficult to reconcile this mechanism with the rather comparable spontaneous and induced-cancer incidences in species with greatly different numbers of cells.

4) Any scheme in which multiple events caused by the carcinogen are required to produce a tumor is incompatible with a linear relation, while, if a disordered state of tissue is an important factor, a true threshold may even occur. There is much evidence, from cancer research of all sorts, indicating that one or both of these conditions is involved in the carcinogenic process.

References and Notes

1. Much of this material was originally proposed as a letter to the editor; I have elected to expand it in view of the importance of the subject and the accumulation of new documentary material. This work was performed under the auspices of the U.S. Atomic Energy Commission. I wish to emphasize that I alone am responsible for the opinions herein expressed.
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THE DEVELOPMENT OF LEUKAEMIA IN IRRADIATED ANIMALS

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1. Murine leukaemia
2. Radiation-induced leukaemia
3. The role of the thymus
4. Dose-rate and the leukaemogenic mechanism
References

The induction of leukaemia in man by penetrating radiation is now a well-established fact (Court-Brown, 1958). The major problems at the moment are the mechanism by which radiation produces leukaemia and the existence or not of a threshold dose below which leukaemia will not be produced. The two problems are closely connected, since the leukaemogenic mechanism must determine the relation between dose of radiation and incidence of leukaemia. In fact the only evidence about the leukaemogenic mechanism in man which is available at present is the dose-response relation, which has been determined very approximately in the Japanese who received a single, very short exposure of radiation from the explosion of atomic bombs, and rather more accurately in the British patients with ankylosing spondylitis who each received one or more courses of treatment consisting of repeated exposures to *x* rays (Court-Brown & Doll, 1957). Animal experiments can clearly be designed to cover a much wider range of conditions of exposure to radiation than human experience can offer and should be capable of providing critical tests of hypotheses in so far as dose-response relations can give information about mechanisms. Animal experiments, too, provide opportunities for experimental interference of a kind impossible in man. The value of animal experiments, however, may depend on how closely the animal disease mimics the human (cf. Israëls, 1954; Kaplan, 1954).

1. Murine Leukaemia

Leukaemia has been found to occur naturally in a variety of mammals but has been regularly produced by experimental irradiation only in mice. (The occurrence of leukaemia in chronically irradiated guinea-pigs has been reported only once (Lorenz & Congdon, 1955).) The kind of leukaemia most commonly found in irradiated mice is the mediastinal type, as reported in the first extensive account of experimental carcinogenesis by whole body irradiation of mice (Furth & Furth, 1936). Here the thymus may appear to the unaided eye to be the only organ involved; the superior mediastinum is occupied by a soft tumour mass which may take up as much space as all the rest of the contents of the chest put together. The clinical picture is one of dyspnoea resulting from mechanical interference with breathing and is thus totally unlike that of any human case of leukaemia.

At the other extreme there is generalized enlargement of the lymph nodes—including the paratracheal lymph nodes—and of the thymus, with infiltration of liver, spleen and bone-marrow. Thymic involvement, however, may not be detectable by the unaided eye and the clinical picture is then closely similar to some cases of human leukaemia. All grades of thymic involvement and lymph-node non-involvement may be found, and whether murine lymphoid leukosis is thought to resemble or to differ from human leukaemia depends very much on which of the two extreme pictures the observer considers most typical of the disease in the mouse. The physiological involution of the thymus with increasing physiological age is much delayed in the mouse as compared with man and, if the thymus is regarded as a normal lymphoid organ (Trowell, 1958), its involvement in murine lymphoid leukaemia might be considered natural. Myeloid leukaemia is less frequently induced by irradiation of mice just as its spontaneous occurrence is rarer than that of lymphoid leukaemia; the opposite is true in man.

An abnormally high peripheral white blood-cell count is more characteristic of human leukaemia than of mouse leukaemia but it is now generally recognized that tissue involvement, as in the bone-marrow, is a more fundamental feature. It must be recognized, however, that leukaemia is an unsuitable name for the murine disease.

Besides typical myeloid and lymphoid leukaemia many other types of proliferative disease of reticular tissue can be defined in man and in the mouse. Dunn (1954) has attempted a comprehensive histological classification into which may be fitted many of the murine cases. My experience confirms here that the kinds of disease differ in their relative frequency in genetically different mouse stocks, and also that the difficulty of classification, the indefinite merging of one type into another through a series of examples each differing only slightly from the next, varies greatly between genetically different mouse stocks. When it is important to be able to classify reasonably well each case of leukaemic disease which may occur, as must be done when determining dose-response curves to irradiation, the choice of the mouse strain to use for the experiments is of great moment. A strain with a high spontaneous incidence of some kind of leukaemia may well reduce the difficulties of classification, but this advantage is more than counterbalanced by the disadvantage of the short natural life-span and the fact that the difficulty of establishing with statistical certainty the reality of a small increase in incidence when the control incidence is of the order of 50% is greater than when it is nearly zero.

In this laboratory the CBA mouse has been used for much of the work on mammalian radiation damage. The strain was originally developed by selection for longevity so that there is a low incidence of killing disease during the first two years of life. The mean age at death in our laboratory is about 27 months for males, 29 months for females. Thus, if a mouse is exposed to radiation for a period as long as several months, there is still plenty of time for the delayed effects of radiation to manifest themselves before mortality in the controls begins to complicate the interpretation of the results.

In the CBA mouse the distinction between the different forms of murine leukaemia seems to be clearer than in the other strains used (see Table I). The clinically distinct types of lymphoid leukaemia are the mediastinal and the generalized types already described, and a third type where spreading soft subcutaneous masses are found commonly in the axillae, on

TABLE I. INCIDENCE OF LEUKAEMIA IN VARIOUS MOUSE STRAINS AFTER 6 OR 4 WEEKS *x* IRRADIATION (50 r. DAILY)

Strain	Sex	Mean survival time* (months)	Incidence of leukaemia at 18 months		
			Control	After 1,500 r.	After 1,000 r.
CBA	M	25	Nil	8/8	3/10
CBA	F	27	Nil	10/12†	14/20
F_1(CBA × A)	F	29	Nil	7/20‡	3/10
F_1(A × CBA)	F	18§	Nil	8/10	3/10
A	F	19§	1%	6/10	4/15

* Measured from 8–10 weeks of age when the course of radiation started.

† Acute radiation deaths (8) occurred in this group only. These deaths occurred near the end of the course of irradiation either just before or during the few days after the last dose, and were apparently due to severe anaemia resulting directly from bone-marrow damage caused by the irradiation.

‡ This group received 25 r. daily, not 50 r.

§ The shortened survival time is due to the high incidence of milk-factor mammary tumours.

the dorsum between the scapulae and in the neck, without involvement of the iliac lymph nodes and with a very variable degree of visceral involvement. In myeloid leukaemia the lymph nodes are not conspicuously enlarged and may show a green or brown-green tinge: the spleen also is grey-brown rather than creamy. The accurate classification of stem-cell leukaemias where the abnormal cells are not accompanied by more mature and definitely myelocytic or lymphoid cells requires methods additional to the histological examination of tissue. There is a very distinctive generalized reticulo-sarcoma of liver and spleen accompanied in middle-aged, but not aged, mice by involvement of the vagina: this seems to be the same as the histiocytoma described by Gorer (1946). The localized proliferative diseases of the mesenteric lymph node are uncommon; this is helpful since they can be difficult to classify histologically and their clinical importance is uncertain. Neither the mesenteric lymph-node diseases nor the histiocytoma of liver and spleen have been included in the class of murine leukaemia. Some cases where the cell type is that of the histiocytoma, but the lymph nodes and/or bone-marrow are involved, have been considered to be leukaemic.

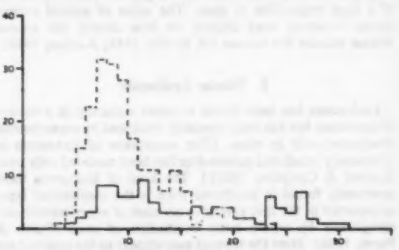
2. Radiation-Induced Leukaemia

In order to assess the leukaemogenic effect of irradiation, mice aged 8–10 weeks were given *x* rays either in single doses or in a series of equal doses five days a week, once weekly or once fortnightly, to predetermined total doses (Corp, 1957; Mole, 1955, 1956). Table I shows that a 6-week period of daily irradiation at 50 r. daily 5 days a week can produce a leukaemia incidence of 60–100% in three genetically different mouse strains, none of which has a high spontaneous incidence. This type of exposure to radiation seems therefore to be uniformly leukaemogenic in the mouse, and the results suggest that every mouse is capable of developing leukaemia if the right conditions for irradiation are chosen.

No comparable human experience exists, although the *x*-ray treatment of ankylosing spondylitics is not too dissimilar in length of over-all exposure time and in accumulated dose. The major difference is in the irradiation of the whole of the mouse body as compared with the more localized spinal irradiation of the human patients. Kaplan & Brown (1951) showed experimentally, by irradiating the two halves of the mouse's body at differing times, how necessary simultaneous or nearly simultaneous irradiation of the whole body was if a very high incidence of thymic leukaemia was to be produced. Whole body irradiation of mice with 1,000–1,500 r. over 4 to 6 weeks (Table I) results in an incidence of leukaemia a hundred times greater than in subjects of ankylosing spondylitis given the same dose to the spinal marrow.

Any estimate of the degree of leukaemogenesis by radiation depends on the base-line of incidence among controls. Before 18 months this was negligible (Table I) but, as the control mice aged beyond the 18-month point, proliferative diseases of reticular tissue began to occur. Their total frequency, however, did not exceed 15–20% in any of the strains of Table I, so that irradiation clearly did more than merely shorten the "maturation" time of a spontaneously occurring disease. Nevertheless the incidence of radiation-induced leukaemia against time after irradiation is clearly an important question, and in fig. 1 this is shown for over 300 examples occurring in irradiated female CBA mice. The leukaemias are divided into two classes, thymic leukaemia and generalized leukaemia of all other types. The class of thymic leukaemias includes a majority in which the thymus, or the superior mediastinum when thymus and paratracheal lymph nodes were not separately identifiable, was involved to no greater an extent than might have been expected in a proliferative disease affecting the whole lymphoid system. The distinction between these and the generalized lymphoid leukaemias depended on whether the thymus was or was not involved as judged by the unaided eye, and was perhaps arbitrary. In old mice the thymus is naturally so atrophic that its involvement in proliferative disease might not be expected. In some young adult mice, which had been irradiated and had died from leukaemia, the thymus had apparently not yet properly regenerated after the damage produced by the irradiation. The paratracheal lymph nodes were then always involved, and often the leukaemic cells were infiltrating the paravertebral

FIG. 1. DISTRIBUTION OF LEUKAEMIA IN FEMALE CBA MICE PLOTTED AGAINST TIME AFTER START OF IRRADIATION



Abscissae: months after start of irradiation
Ordinates: number of cases

Mice were aged 8–10 weeks at the beginning of their exposure.

Broken line: thymic leukaemia
Continuous line: other leukaemias

muscles of the thorax and the spaces around the nerve-roots and the spinal cord itself, thus causing gross paralysis of the lower trunk and hind limbs.

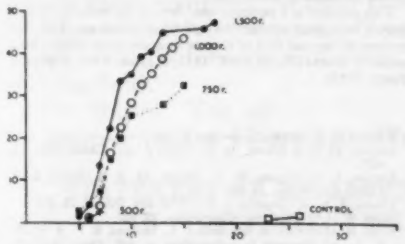
Figure 1 shows that an observational period of 19 months revealed all but one of 204 thymic leukaemias produced by irradiation of young adult female CBA mice. Generalized leukaemias, however, went on occurring for 30 months at least and did not show such a sharp peak of incidence during the first year. Furth & Upton (1954) found a similar differential incidence with age in Rfl mice given a single dose of *x* rays. Leukaemia of either kind did not appear before 21 months in controls (figs. 2, 3).

The cumulative incidences with time of the two classes of leukaemia are given separately in figs. 2 and 3 where the effects of different total doses are also shown separately. In these experiments the dose was 10 r. daily or 50 r. weekly, 25 r. daily or 125 r. weekly, or 50 r. daily (over-all exposure time 2–30 weeks). Larger individual doses reduce the over-all incidence of leukaemia (Table II and Mole, 1956, 1958). Figure 2 shows a striking threshold effect. Very little thymic leukaemia was produced by a total dose of 500 r. The larger total of 750 r. increased the incidence more than tenfold, but increasing the dose still further did not increase the yield commensurately.

Figure 3 shows a very different picture. The incidence of generalized leukaemia (all non-thymic types combined) steadily increased with time whatever the total dose. Late in life the lowest total dose, 500 r., apparently had more effect than the higher doses, but after 20 months there were very few mice left in the other groups. It must be emphasized that this group of generalized leukaemias is not homogeneous, and needs further analysis: for example, after total doses of 1,500 r., a high proportion of cases of aplastic anaemia, which after review were considered to be cases of leukaemia, occurred just as in the patients with ankylosing spondylitis (Court-Brown & Doll, 1957).

The excess of leukaemias produced by 500 r. (fig. 3) may perhaps be exaggerated by excluding from the class of leukaemia the reticulo-sarcomata of liver and spleen mentioned earlier. The incidence of this type of lesion was markedly reduced after all the radiation doses considered here. If one effect of radiation is to encourage the generalization of an otherwise focal proliferation of reticular tissue, it may not be fair, when assessing leukaemogenesis, to include the generalized cases (which may occur in the irradiated) but to exclude the focal cases (which occur in the controls). Clearly much more analysis is needed to see whether the best basis for classification is clinical picture, morbid anatomy or cytological character, and before deciding whether the leukaemic diseases are a unitary group or have diverse aetiologies.

FIG. 2. DOSE OF *x* RADIATION AND CUMULATIVE INCIDENCE OF THYMIC LEUKAEMIA IN FEMALE CBA MICE



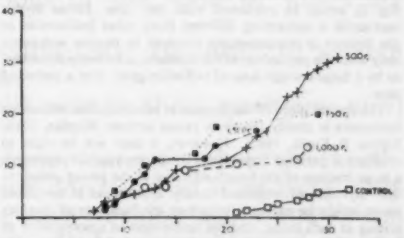
Abscissae: months after start of irradiation
Ordinates: percentage of mice with thymic leukaemia

The number of mice in a group was 72, 115, 40 and 90, for the total doses of 1,500, 1,000, 750 and 500 r. respectively.

Over-all exposure time: 2–30 weeks (see text). Each accumulated total dose is indicated by a different symbol:

●–● total dose of 1,500 r.
○–○ total dose of 1,000 r.
■–■ total dose of 750 r.
+–+ total dose of 500 r.
□–□ control animals

FIG. 3. DOSE OF *x* RADIATION AND CUMULATIVE INCIDENCE OF NON-THYMIC LEUKAEMIA IN FEMALE CBA MICE



Abscissae: months after start of irradiation
Ordinates: percentage of mice with non-thymic leukaemia

Data as for fig. 2.

3. The Role of the Thymus

Comparison of figs. 2 and 3 shows how easy it may be to reach erroneous conclusions about the dose-response curve for the induction of leukaemia by radiation, when attention is confined to thymic leukaemia or when the observational period is cut short. Unfortunately most experimental work on murine leukaemia has been confined to relatively short-term experiments on thymic leukaemia. Here the evidence that radiation causes thymic leukaemia by an indirect mechanism

TABLE II. INCIDENCE OF LEUKAEMIA IN IRRADIATED FEMALE CBA MICE AND SIZE OF INDIVIDUAL DOSE

Total dose of *x* rays	Number of mice with leukaemia/number in group					
	25 r. daily	125 r. weekly	250 r. fortnightly	50 r. daily	250 r. weekly	500 r. fortnightly
1,000 r.	10/18	12/20	3/18	14/20	9/19	4/15
1,500 r.	11/20	18/20	5/17	—	—	—

is strong (Kaplan, 1954; Kaplan, Carnes, Brown & Hirsch, 1956), and the remarkable effect on the incidence of thymic leukaemia of increasing the total dose from 500 to 750 r. (fig. 2) would be consistent with this view. Either thymic leukaemia is something different from other leukaemias or the thymus is conspicuously involved in murine leukaemia only when the physiology of the mouse is sufficiently disturbed, as by a large enough dose of radiation given over a period of time.

The central place of the thymus in recent studies on murine leukaemia is clearly shown in recent reviews (Kaplan, 1954; Upton & Furth, 1957). However, it may not be right to attribute a peculiar importance to the thymus. It represents a large fraction of the lymphoid tissue of the young mouse so that thymectomy, regarded simply as removal of lymphoid tissue, might be expected to reduce the incidence of diseases arising in such tissue, and the restoration of susceptibility to leukaemia, spontaneous or radiation-induced, by the subcutaneous grafting of thymuses into thymectomized animals has not in fact been controlled in published experiments by the grafting of equal masses of non-thymic lymphoid tissue. This is unfortunate because the demonstration that leukaemia can apparently originate in an unirradiated thymus, when this has been implanted into a thymectomized mouse after it has been irradiated (Kaplan *et al.* 1956), would seem to prove a humoral mechanism for leukaemogenesis. The proof hinges on the genetic characters of the leukaemia as determined by transplantation. Further evidence is being sought in this laboratory by a different method, chromosome markers being used to identify the origin of the leukaemia (Ford, Ilbery and Loutit, unpublished observations from this laboratory).

4. Dose-Rate and the Leukaemogenic Mechanism

Further information on the over-all incidence of leukaemia after total doses of less than 500 r. is clearly needed before the shape of the dose-response curve may be guessed at, and it is possible that other observations may allow a decision about the hypothesis that leukaemia is produced by radiation through the production of point mutations. The chromosome complement of leukaemic cells in the mouse is so commonly abnormal in several respects (Ford, Hamerton & Mole, 1956),

TABLE III. THE EFFECT OF DOSE-RATE ON THE INCIDENCE OF LEUKAEMIA IN FEMALE MICE GIVEN 1,000 r. WHOLE BODY γ IRRADIATION DURING A 4-WEEK PERIOD

Group	Irradiation schedule	Dose-rate (r./hour)	Number of mice with leukaemia/number in group after 10 months' observation	
			CBA	C57 black
A	5 days weekly	81	4/10	4/9
B	5 days weekly	21	0/10	4/9
C	5 nights weekly	3.3	0/10	0/9
D	Continuously	1.3	0/10	0/5
E	Controls	Nil	0/10	0/8

The dose in Group D was 900 r., not 1,000 r. as had been planned. (Similar results are being obtained in other groups of mice similarly irradiated but observed so far for only 5–6 months.)

that it is tempting to think that these visible abnormalities indicate that the basic change which makes cells leukaemic is not a point mutation but something grosser. If such gross changes are indeed an aetiological factor in leukaemogenesis the effect of dose-rate might be of overriding importance. This deduction is being confirmed by the preliminary results of an experiment designed to test the question, in which the same total dose of radiation spread out over the same over-all exposure time was given in daily doses at different dose-rates (Table III). Because of the short observation period any conclusions can at present apply only to thymic leukaemia, but for this form of leukaemia it is clear that somatic mutation, if it occurs, is of subsidiary importance to some factor dependent on dose-rate. Much more work is needed to extend these observations to smaller total doses spread out over longer periods of time and to other kinds of leukaemia. Nevertheless, chromosomal abnormalities in leukaemia cells were, if anything, commoner in generalized leukaemia than in purely mediastinal leukaemia.

This account is a personal one: few of the many published papers have been selected for individual reference. Excellent reviews of the vast field of murine leukaemogenesis have been made by others (Furth, 1946, 1951; Kaplan, 1954; Upton & Furth, 1957).

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Representative HOLIFIELD. I will ask our consultant, Dr. Paul Tompkins, to propound some questions to you, some of which are very technical.

Mr. TOMPKINS. Would it be possible, Mr. Holifield, to reserve these questions until Dr. Law testifies?

Representative HOLIFIELD. We will reserve those questions until later, if we may.

Representative DURHAM. I noticed your statement about the threshold concept. At the present time you don't regard it as being very firm. It is just something that you have to think about and dream about, and come to some conclusion. It has been bandied around here for the past few years as to whether or not we do have a threshold.

Dr. BRUES. I think that is a very basic point that you have made. We have to consider that possibly there are a great many different ways in which this might work. We are not just choosing between whether something will produce a thousand leukemias or no leukemias. It is possible that there might be some sort of mechanism going on that would produce 1 or 10 or some other number. A good deal of the confusion in the public mind, is the idea that the scientists have two hypotheses and they are trying to choose between them and they are not bright enough to distinguish between them. It is much more complicated than that. This is the point I meant to make.

Representative DURHAM. You believe there has to be a good deal more work before we can come up with a firmness on the threshold in this field.

Dr. BRUES. Yes, sir.

Representative HOLIFIELD. We will now hear from Dr. Law.

STATEMENT OF DR. LLOYD W. LAW,[1] NATIONAL CANCER INSTITUTE, PUBLIC HEALTH SERVICE

Dr. LAW. Mr. Chairman, members of the committee, ladies and gentlemen, I would like, in a short time, to consider the question of one somatic effect of radiation, leukemia, and direct my remarks specifically to that, and particularly the present knowledge, latest findings, attempts to use the experimental animal to answer some of the questions which have been raised this morning. I would like also to submit for the subcommittee's attention a manuscript which I have

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written on "Radiation Carcinogenesis" which attempts to bring the present knowledge fairly well up to date in this field.

Representative HOLIFIELD. It will be accepted.

(The statement referred to appears on p. 1437.)

Dr. LAW. Let me say at the outset that I recognize that some somatic effects, particularly the neoplasms, do not represent the major hazard of radiation and probably not the major somatic hazard, genetic material and reproduction function probably being much more sensitive. I would rather not, since the statement I have is rather lengthy, and the time is running on, to read it. I would rather cover a few points of the statement.

Representative HOLIFIELD. There are only 7½ pages. I think this is important. Why don't you read this, Doctor?

Dr. LAW. I have something like 17 pages in the statement.

Representative HOLIFIELD. Then you may summarize. I had a different set of testimony here.

Dr. LAW. Before I summarize, may I give a small amount of background? I would like to say first of all just a word about the concept of linearity of response and no threshold which has been referred to here and has been used by certain individuals as a concept of the effects of radiation in the induction of leukemia in man, for example.

The two sets of data from which this concept has been derived are those of the Japanese bombings, and those of Court-Brown in the treatment of ankylosing spondylitics with fairly large doses of partial-body radiation to the spine and the surrounding tissues. This is a concept which, of course, suggests that over the dosage of radiation met with in ordinary civilian life, the dose-response relationship is a simple proportional one as outlined by Dr. Brues, analogous to that for the induction of mutations. If this were finally shown to be the case, two deductions would become valid: One, a proportion of naturally occurring cases of leukemia are probably due to natural background radiation and secondly, any increase in background radiation from artificial sources will be accentuated with an increase in mortality from leukemia.

This is a working concept. I have pointed out in the statement some of the objections which have been raised to accepting this concept. I think probably one of the major objections is that the data so far presented are data in progress. As far as I know, there is not much indication that the frequency of leukemia among Japanese survivors is falling or that a plateau is being reached at the present time.

Ten years from now or after all the Japanese survivors have died, the situation concerning frequency and distance from the bomb, may be quite different than they are at the present time. There are also some very serious statistical objections to this type of retrospective study. These have been detailed by Dr. A. W. Kimball of the Oak Ridge Laboratories and should be submitted for the record.

So although this is a concept and a good concept, it should be accepted knowing the scientific limitations of it. This being the case one may then turn to the probable use or possible use of the experimental animal in attempting to answer some of the questions concerning dose-response relationships, mechanisms of carcinogenesis, means of interfering with or protecting against radiation and so forth. Such

experiments can be designed and executed in the experimental animal.

I wish to point out that the question of extrapolation or the assessment of the consequences of radiation in the mouse has been questioned as to whether the data so obtained can be extrapolated or assessed in terms of human action. This certainly has not been answered by scientific experimentation as far as the somatic effects of cancer are concerned.

Representative HOLIFIELD. Is it impossible to extrapolate accurately because of the long life span of man and the lack of time to do a study of generations of a species?

Dr. LAW. That is right. I just point out that this certainly is not a good scientific basis but one has to extrapolate.

Representative HOLIFIELD. Is it also true that the mouse being a mammal that there is a certain similarity which has been estimated about a 25-percent difference on other types of experiments?

Dr. LAW. That is correct.

Representative HOLIFIELD. A mouse has also the same response to one hundred roentgens or a man would show to 75 roentgens, or is it the other way?

Dr. LAW. I am not certain that there is scientific evidence to extrapolate even from these figures.

Representative HOLIFIELD. Within 25 percent. You would doubt that there is scientific evidence to support a 25 percent differential in effect?

Dr. LAW. I would doubt at the present time that there is much scientific evidence to allow such an extrapolation. I probably will answer that as I go along. There is a striking similarity, morphologically and biologically of the leukemias in mouse and man. If one looks at the Sixth International Revision of the causes of death as to the classification of leukemias and lymphomas, almost every neoplasm found in man is found in the mouse. Chronic and acute lymphocytic and granulocytic leukemia, Hodgkins' disease, and so on. The point I should like to make here is that there is some information from some of the recent experimental work that the leukemias and lymphomas probably represent separate diseases and should be considered as separate diseases. They certainly are in their response to radiation or in their response to therapeutic agents. Morphologically they are distinct neoplasms. They are found in man and in the mouse and probably should be considered as separate diseases. Not leukemia in general, in other words.

I think perhaps there have been some unwise extrapolations in the past in statements that such a thing happens in mouse leukemia, therefore perhaps we can extrapolate this to man without reference to the type of leukemia in mouse or man.

Representative HOLIFIELD. On the other hand, you would not disregard an experiment such as Dr. Russell made on the 5,000 mice. You certainly cannot disregard the radioactive effect upon the generation that follows. As I remember, he exposed 5,000 mice to a 100 roentgen shot, and then traced the effects of it through 100,000 descendants. Your statement does not mean that such an experiment is lacking in significance to genetic study and there is some possibility of extrapolation from those effects to man.

Dr. LAW. Absolutely not. The point I am making is that you can extrapolate, yes.

Representative HOLIFIELD. The point you make is that it is not now enough of a science to pin it down acurately in percentages, is that right?

Dr. LAW. Yes. I feel there probably is not enough information even from the experimental animal to pin this down from a mouse to a man basis. I think it could be done.

Representative HOLIFIELD. Is it true that it has a significance in certain areas? I would not say 1 percent, but there certainly should be some area of comparison which would be significant.

Dr. LAW. Yes. The point I am making is that the area is significant from the fact that these diseases occur in man and the mouse and from the fact that the radiation effects in the induction of certain neoplasms are similar in mouse and man.

Representative DUNHAM. There is tissue in rats and there is tissue in man. There is similarity of tissue which you are dealing with primarily.

Dr. LAW. That is correct, the lymph nodes, bone marrow, spleen, and other blood forming tissues. Functionally and morphologically they are very much alike, those having to do with the leukemia and lymphomas.

There have been reported numerous data in the literature concerning animal experimentation in the use of mice and rats in the study of mechanisms of leukemia induction. Most of these, however, were designed for a maximum response in the animal. That is, the response of 5 to 100 percent of neoplasms; whereas few studies have been set up or designed to answer the problems in the clinically important range, which probably is at the 0.5 point to the 1 percent range of response.

Representative HOLIFIELD. The point you are making there is that the intensity of the radiation to which these mice were exposed was 100 roentgens, and they had to make it that high in order to get meaningful statistical data in the 100,000 descendants, is that right?

Dr. LAW. That is correct, 100 roentgens or more.

Representative HOLIFIELD. This does not mean that there could not have been used a lesser dose, and you could have obtained observable damage on the descendants, does it?

Dr. LAW. I am talking about leukemia now in the individual. I am coming to that in a little while, the matter of lower dosage rate and chronic radiation. What I am saying is that the experimental approach over the years has been that not particularly designed to answer the questions which concern us at the moment. That is, the question of dose response or threshold, influence of dose rate, and so forth.

Representative HOLIFIELD. Because experiments have been carried on a higher level of radiation exposure?

Dr. LAW. Yes. They were designed for other purposes.

Representative HOLIFIELD. Than your natural radiation background exposure plus your bomb test addition?

Dr. LAW. That is correct. The evidence which may answer some of the questions posed this morning, and in the 1957 hearings, are

certainly experiments which are rather recent. I hope to detail some of the results.

Recognizing, therefore, that there are different diseases which make up what we call leukemia and lymphomas, it is evident from the newer data which are now being published that the response to radiation may be quite different for the development of myeloid leukemia in comparison with lymphoid leukemia or reticulum cell sarcoma or chronic compared with acute leukemia. I have mentioned here only a few pertinent facts which relate to this problem. These are from different experiments which are documented in the manuscript and given at some length in the statement.

For example, if radiation is fractionated or protracted, there is a difference in the induction of myeloid or lymphoid leukemia. If one fractionates, for example, the tendency is to develop lymphoid leukemia. If one does not fractionate using the same total dose, even in the same strain of mouse, the tendency is to develop myeloid leukemia.

As far as lymphocytic leukemia is concerned, and these data are obtained from several different strains of mice, one probably has to reach a fairly high level of total radiation 150 r to 500 r, fractionated or single, before there is an increased frequency in lymphocytic neoplasms. On the other hand, this total dose which one has to reach is strikingly lower if one considers the response in the development of myeloid leukemia. As a matter of fact, there is some evidence that it is less than 100 r for myeloid leukemia total body radiation and for lymphoid leukemia it is in the neighborhood of 150 or as high as 500 r before a response in the induction of lymphocytic leukemia is shown.

There is some evidence from the Oak Ridge Laboratories that radiation as low as 16 or 32 r influenced the frequency of myeloid leukemia in mice. I point this out only to show that there is quite a difference between the response to develop myeloid and the response to develop lymphoid leukemia.

In one particular neoplasm in the mouse, called a Hodgkinslike neoplasm, which resembles morphologically Hodgkins disease in man, gamma radiation was actually shown to decrease the incidence of the disease. Radiation was from an atomic detonation. The incidence of Hodgkins disease was 40 percent in the control mice and at 500 r, it was near zero percent. This is a different response to radiation from the lymphoid and myeloid leukemias.

Just one more point in regard to this. There are differences in age sensitivity. For example, if one radiates mice in the early neonatal period, one is able to produce lymphoid leukemias, whereas myeloid leukemias do not usually appear. Contrariwise, if the radiation is done late in life, at 6 months of age, myeloid leukemias appear and the frequency is increased, and there is very little effect on the induction of lymphoid leukemias.

The point I am making is that these are separate diseases. We know that there are in certain cases probably different etiologies of these diseases, and that in considering background radiation or other types of radiation as an inductive agent we ought to keep in mind the differences in the different diseases.

I would like to say just a word or two concerning the recent work relating to chronic radiation, that is, radiation which has been given over the lifetime of the animal and what this means in terms of in-

creasing frequency or of decreasing the time at which the animals die of leukemia, that is, influencing the latent period.

In the early days of the Manhattan Project, Lorenz was one of the first to use gamma radiation from a radium source, and indicated that there was some influence in the induction of leukemia with dosages as low as 0.11 r per day. That is a fairly high total dose, but given chronically.

Recently the British group at Harwell have used fast neutrons in irradiation of mice at certain levels, the lowest being .54 rad per week. In these experiments, although there was a striking influence on reproductive function at the .64 rad level per week, there was no influence on the induction of leukemia in these mice in any of the forms which I have mentioned.

The work at Argonne, which has been published recently relating to the chronic effects of strontium 90 given intravenously in mice has been reported as well as the influence of other alpha and beta emitters. Details of the work were given concerning principally strontium 90. I think it is interesting that a dosage of 8.9 microcuries per kilogram given intravenously, which is a fairly high dosage level, did not influence the frequency of leukemia in the mice, although there was some influence on the time at which the leukemias appeared. This is a dose at which no influence on life or no influence on the induction of bone sarcoma was observed. There was not observed an increased frequency of any of the different forms of leukemia. They did appear earlier and kill the animals earlier. At dosage levels below 8.9, there were two levels which were used, there was no observable influence on either the latent period or the induction of leukemia.

Representative HOLIFIELD. That is using 9 roentgens.

Dr. LAW. Nine microcuries per kilogram of mouse was the dose used. This I think turned out to be in the neighborhood of 1 microcurie retained during the lifetime figured at 600 days.

Representative DURHAM. What was the life span?

Dr. LAW. The life span was 600 days. That was the mean life span. So it was quite a long period of chronic radiation. These are the recent data which concern chronic radiation. I would like to point out that they are extremely limited. The limitations have to do with age. The radiation was begun at a certain age. They are extremely limited as far as strain of mouse is concerned. For example, strontium 90 may have indeed increased the frequency of leukemia if one had chosen another strain of mouse. There are other variables which we know something about, which were not investigated. I would like to consider only one, and this is the possibility of the action of cocarcinogens during chronic radiation.

Representative HOLIFIELD. What do you mean by that word?

Dr. LAW. I will attempt to explain it now, if I may. It has been shown in certain experimental situations in the induction of cancer that if one uses a compound which may in itself not be carcinogenic—that is, does not of itself produce cancer or leukemia—if given along with radiation or with a carcinogen like coal tar, will actually increase the frequency with which the neoplasms appear. It is said to be cocarcinogenic.

In the case of radiation there are several of these agents. For example, a compound which is called urethane is known to influence

strikingly the X-ray induction of leukemia in mice when given along with X-rays and at total levels of radiation which of themselves are only slightly carcinogenic.

If one looks now to some of the evidence which is available in the range of diagnostic X-radiation, that of a few roentgens, there are few data to indicate the effectiveness of these levels. I have mentioned that there is some indication of myeloid leukemia resulting from as little as 16 or 32 rads of radiation. I think this should be reinvestigated under the same conditions of strain, age, radiation factors, and so forth. There is some evidence in the induction of ovarian tumors of mice or mammary tumors in the rat, that dosages as low as 32 r or 25 r are carcinogenic, increasing the frequency of these diseases, and in the case of ovarian tumors, initiating an entirely new neoplasm. Although these effects are probably not direct effects of radiation, they are hormone-mediated in the case of both of these tissues which are hormone dependent, I think it is interesting that in given situations there are isolated examples of carcinogenesis or production of leukemia at low dose rates.

There is also some evidence that P^{32}—phosphorus 32—is extremely carcinogenic in certain strains of rats in the production of bone tumors and carcinomas of the skin and at doses one-half that which are used in man for therapeutic purposes.

One other point which was mentioned earlier was the work of Stewart and colleagues in England concerning the possibility that X-radiation as low as 1 to 10 rads may be effective in the production of leukemia in man. Some people have considered these results as being equivocal, and they certainly should be repeated elsewhere. I think it certainly implies that we should look closely at the response to radiation of embryonic or fetal tissues or tissues of childhood. This is especially significant in view of the fact that the Russells have shown recently that developmental abnormalities appear with as little as 50 r X-radiation, or that there has been production of possible somatic mutations in mice with 100 r, and probably lower, if irradiated at the optimal time in utero.

There has been some work done in our laboratory and some reported from Oak Ridge that following the radiation of mice of various strains in utero at particular times there has been no observable increase in the frequency of leukemia or in the decrease of the latent period. But here again there are limitations to the experiment so far and only a few strains have been used. There are limitations as to the amount and type of radiation. There are limitations as far as the time at which embryonic or fetal tissues were irradiated. It is the type of experimentation in which there are few data, and which certainly should take our time and effort.

Mr. Chairman, may I summarize what I have said so far and have listed in the statement concerning some tentative generalizations? I wish to stress that these certainly are tentative, from the very little evidence that we to date have on the subject. I think there are trends in evidence which may exist 2 to 10 years from now or may completely change, depending upon further findings. Therefore, these should be interpreted in the light of these limitations.

First of all, ionizing radiation induces leukemia. Chronic radiation is appreciably less effective than acute radiation in the induction

of these diseases. Strontium 90 deposited internally is relatively ineffective as a leukemogen but here again there are limitations as discussed in the statement. Indirect effect of radiation has been established for the induction of thymic lymphocytic leukemia in the mouse, but not for other types of leukemia. So we don't know, for example, whether there is an indirect induction mechanism for myeloid leukemia. Dose response relationship, threshold, and the results of fractionation and protraction of radiation differ among the several types of leukemia. Present data, though inadequate, are consistent with the concept that there exists a threshold dose below which leukemia will not result from radiation. This threshold dose may be quite different for the different morphologic forms of leukemia.

Again I would like to emphasize that these are trends and they are based upon relatively few data accumulated only over the last few years in most cases.

Thank you very much.

(Dr. Law's formal statement follows together with a paper by A. W. Kimball, of Oak Ridge:)

Leukemogenic Effects of Radiation by Dr. Lloyd W. Law, Head, Leukemia Studies Section, National Cancer Institute, USPHS; for presentation at the Public Hearings on Fallout from Nuclear Weapons Tests, to be held by the Joint Committee on Atomic Energy, Special Subcommittee on Radiation, May 7, 1959.

A strikingly large number of publications have appeared in the literature since the "Hearings Before the Special Subcommittee on Radiation" in 1957. The recent Report of the United Nations Scientific Committee on the Effects of Atomic Radiation makes clear two things:

(1) That our knowledge of the physical aspects of radiation exposure to which we are subjected and may continue to be subjected is far ahead of knowledge of the biological consequences of such exposure; and

(2) That the attempts of the Committee at quantitative assessment of the hazards involved do not add greatly to the other national reports.

It is not that available chemical and biological data have not been sufficiently studied and analyzed but that progress must depend upon new data and the development of biological (basic) concepts.

Although it is questionable that much new data have been published since 1957 that will lead to a better understanding of the non-genetic effects of irradiation or will help to resolve some of the practical problems facing humanity, it is my intention to fit the newer data into existing knowledge concerning leukemia principally and where pertinent to the discussion, to other neoplasms. My interpretations of our basic knowledge relating to findings in experimental animals are somewhat at variance with those of witnesses who presented data and their interpretations in 1957.

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In restricting the discussion it is realized that leukemia and other neoplasms probably do not represent the major hazard of radiation. The available evidence suggests that the effects on genetic material and on reproductive function are extremely sensitive indicators of radiation damage.

The causal relationship between leukemia in man and radiation from an external source is now an established fact as shown among ankylosing spondylitics receiving X-ray therapy and by retrospective studies of the survivors of the Japanese bombings. Two major problems, closely related, are: (a) what is the mechanism which produces leukemia? and (b) what is the relationship between dose of radiation and incidence? Does there exist a threshold below which leukemia will not be induced?

The only evidence concerning mechanisms of leukemogenesis in man is that concerning the dose-response relationships among the Japanese and more accurately among those British patients given repeated exposures of X-rays to the spine and immediately surrounding areas. Both Lewis and Court-Brown favor the concept of linearity of dose-response and lack of a threshold in interpretations of these data. Serious objections to such a concept have been voiced. In regard to the interpretation of leukemia among the Japanese survivors, some objections are as follows:

(a) The doses received are extremely uncertain and it is felt that radiation dose cannot be inferred from distance alone.

(b) It is suggested that there was an overestimation of dose which could easily convert a curvilinear response to a linear response.

(c) In the area where dosage was estimated to be 125 rad or less, the increase in absolute numbers was only a relatively few cases and these appeared to be limited to the nearest quartile of the area.

(d) Latest evidence obtained through 1957 concerning leukemia in the Hiroshima survivors gives no indication of a decline in the incidence or that a plateau in the occurrence of leukemia has been reached. Thus, the likelihood must be considered that the final results, when the entire population has died, may indeed show different relationships that the present results would indicate.

(e) There remain uncertainties regarding the population particularly at Hiroshima and of migrations in and out of the city from 1950 to the present.

The latest information published in 1958 concerning the findings among Japanese survivors lends support only to the previously reported qualitative findings of the Atomic Bomb Casualty Commission that "those survivors who received large doses of radiation, that is, who were within 1500 meters of the hypocenter - had a significantly higher incidence of leukemia than those beyond that distance, who received little or none".

Similar objections may be raised to the interpretations of Court-Brown. An adequate control group is not available. The susceptibility of this arthritic population to "spontaneous" leukemia is not known, whether it is increased or the same as the average. An increased sensitivity to radiation is a possibility.

The considerable number of serious statistical errors that may be involved, particularly in the Lewis study have been discussed in detail and at length by Kimball.

Emphasis has been placed on this aspect of the subject principally because of the fact that some scientists interpret the discussions, principally of Lewis and Court-Brown, that proof has been obtained that there is no threshold dose for the induction of leukemia in man by ionizing radiation and that therefore "it is possible to calculate - within narrow limits -

how many deaths from leukemia will result in any population from any increase in fallout or other sources of radiation". In the years 1957 and 1958, several estimates have been made, based upon the assumptions of Lewis, of the genetic and somatic effects to be expected from carbon-14 produced by the testing of atomic weapons. The predicted somatic effects, including leukemia in man are estimated to be about equal to those from the fallout fission products, including strontium-90.

Caution should be therefore exercised in attempting to interpret the Japanese data, or those from other sources involving man, to mean that the data are consistent with a linear response and no threshold.

In the consideration of background radiation possibly being an important leukemogenic agent in man and contributing to a steadily increasing mortality from leukemia the interesting study published in 1958 by Gilliam and Walter concerning the trends of mortality from leukemia in the U. S. is mentioned. These studies indicate a perceptible decline since 1940 in the rate of increase in these diseases, a trend which if it persists will eventually stabilize or result in actual decline. According to the authors "..... the trends presently evident provide no support whatsoever for a theory which postulates a sharp increase within the last 15 years in leukemogenic factors affecting the environment of Americans in general".

The design and execution of experiments capable of providing critical information concerning dose-response relationships, mechanisms of radiation carcinogenesis, means of interfering with or protecting against radiation, etc., can best be accomplished with experimental animals and some basic information has already been obtained.

The question of the justification of extrapolation from one species to another and particularly to man has not been answered by scientific experimentation. Extrapolations must be made and it appears wise to make them from a species in which meaningful data may be obtained.

The striking histologic similarity of neoplasms of the blood and blood-forming organs (leukemia) of the mouse and of man is a matter of record. Those diseases in man classified by the "Committee for Clarification of Nomenclature of Cells and Diseases of the Blood and Blood-Forming Organs" have their counterparts in the laboratory mouse, and represent chronic and acute forms of neoplasms of lymphocytes, granulocytes, monocytes, plasma cells, etc. Emphasis is placed here on the variety of such neoplasms encountered in the mouse occurring "spontaneously" or following the use of carcinogens, including radiation. Much of the early information relating to the role of radiation in these diseases refers only to "mouse leukemia" and as discussed later, many of the inferences and generalizations drawn from these data may be incorrect or misleading. It is emphasized that each type of leukemia should be considered as a specific disease. There is considerable evidence to indicate they have different etiologies.

Certain generalizations may be stated concerning the relationship of radiation to "leukemia" in experimental animals, particularly the mouse:

(a) All types of radiation so far investigated have been shown to be leukemogenic. These include X-rays, γ-rays, some α-and β-emitters and fast and slow neutrons. The initiation of the leukemogenic train of events follows an instantaneous atomic detonation or to a lesser degree long-continued chronic irradiation.

(b) Striking differences in the response of various inbred strains of mice probably are the result of genetic differences. Nearly all mice of some strains (e.g. C57BL) develop leukemia providing the proper conditions are met of total dose of irradiation, proper fractionation, sensitive age, etc. On the other hand several inbred strains of mice are strikingly resistant to irradiation e.g. the C3H strain.

(c) A common form of leukemia of the mouse is a lymphocytic neoplasm that originates in the thymus; this also is commonly encountered following acute X-ray exposures.

(1) Induction of this neoplasm by X-radiation represents an example of an indirect mechanism similar to the induction of ovarian tumors, pituitary tumors and probably certain other neoplasms of endocrine or endocrine-dependent tissues in mice. The target tissue in thymic leukemia need not be irradiated but if grafted into an animal which has received whole-body X-radiation, responds by transformation to a neoplasm.

(2) Thymic leukemias have an early latent period, making their appearance at from 50 to 100 days after irradiation.

(3) Protection by lead-shielding of a part of the hematopoietic system or the intravenous introduction of bone marrow after irradiation almost completely prevents leukemia.

(4) Information obtained from at least 3 strains of mice, RF, C57BL and CBA is consistent with the hypothesis that there exists a threshold dose from 150r to 300r depending upon strain, below which these neoplasms do not occur.

(5) By increasing the total dose of X-rays, the yield of thymic leukemia is increased. In contrast the dose-response curves for generalized lymphocytic leukemia, granulocytic leukemia and reticulum cell sarcoma are

unpredictable. The incidence of disease may increase steadily throughout life, reach a peak and decline or, there may be no influence on the incidence of the disease, depending upon the strain of mouse used. A "damping" effect probably representing doses above the optimal carcinogenic level has been observed for neoplasms of blood and blood-forming tissues other than those of thymic origin. This "damping" effect is a rather common finding for other neoplasms such as thyroid tumors and osteosarcomas.

(6) Within the past few years a concept has been introduced relating X-radiation to the "unmasking" or activation of a "latent" virus. These findings are interesting but it should be emphasized that they are preliminary and only suggest the possibility of intervention of a virus in thymic lymphocytic neoplasms of the mouse.

Rather specific details have been given concerning our present knowledge of radiation and lymphocytic neoplasms of thymic origin in the mouse principally because these data have been used by some in attempts to extrapolate to man. These neoplasms are probably special and unique to the mouse and provide a model for academic pursuits relating to etiology and pathogenesis. At the moment, however, caution should be used in attempts to extrapolate this information to practical problems in man concerned with fallout as a radiation hazard.

Other recent data are available concerning the pathologic effects of the Atomic Detonation, Operation Greenhouse. Genetically uniform LAF_1 mice were exposed at varying distances from the hypocenter of the detonation. In terms of the effects of γ-radiation there are certain findings of interest:

(a) Thymic lymphocytic neoplasms occurred earliest and frequency increased with dose, to the highest level 749 r. The total incidence was surprisingly low, less than 10%.

(b) Little, if any, effect was seen on the frequency or latent period of granulocytic leukemia.

(c) Interestingly, a striking reduction in frequency occurred, related to total dose, of a neoplastic condition normally encountered in these mice resembling Hodgkins' Granuloma of man.

(d) Many neoplasms were observed in these studies not seen previously and stated not to have been previously observed after X-radiation.

(e) The frequency of certain neoplasms was not influenced by radiation but had a shortened latent period. This may indicate a different mechanism of carcinogenesis from that observed in which an increased frequency is observed. Certain neoplasms, particularly those of lung tissue and mammary tissue (sarcoma) were less frequent following these conditions of radiation; whereas earlier studies with LAF_1 mice indicated an inducing influence of relatively low levels of chronic γ-radiation.

Recent evidence presented by Upton and colleagues points up the desirability of analyzing the inductive effects of radiation, dose-response relationships, etc., on the basis of morphologic forms of the disease and not on a classification which lumps all "leukemic" diseases together as a unitary group. In studies of X-radiation in RF mice it is clear that the factors that influence the induction of lymphoid leukemia (lymphocytic neoplasms) are quite different from those influencing the induction of myeloid leukemia (granulocytic neoplasms).

(a) The incidence of myeloid leukemia was significantly elevated by exposures to 150 r but the incidence of lymphoid leukemia was not influenced. Previous studies from the Oak Ridge National Laboratory had indicated the possibility of an increased frequency of myeloid leukemia following exposures to levels as low as 16 rem.

Dose response curves obtained from 150 r to 450 r differed considerably for the lymphoid and myeloid forms of leukemia.

(b) Fractionation of a 450 r dose significantly affected the induction of lymphoid but not myeloid leukemia.

(c) Removal of thymic tissue, prior to irradiation, prevented the development of lymphoid (thymic) but did not influence the incidence of myeloid leukemia. On the other hand splenectomy influenced the X-ray induction only of myeloid leukemia.

(d) Mice irradiated during neonatal life did not develop myeloid leukemia despite an observed susceptibility to development of thymic lymphomas. Irradiation, however, at 6 months of age revealed induction of myeloid but not lymphoid leukemia. This difference in susceptibility of RF mice to radioleukemogenesis in relation to age at the time of irradiation is borne out by additional studies by Upton. It is to be noted that the available data for man show acute lymphoid leukemia predominating in irradiated children and acute and chronic myeloid leukemia in irradiated adults.

(e) Sex differences were noted in the expression of these two X-ray induced neoplasms.

These results indicate how easy it is to reach erroneous conclusions about latent period, dose-response curves, threshold dose, etc., when attention is directed to one morphologic form of leukemia or when all forms are considered together as a unitary disease.

Some interesting data on the late effects of internally deposited radioisotopes in CF 1 mice have been published recently from the Argonne National Laboratory. The toxicologic and carcinogenic properties of six α - or β -emitters, Ra^{226}, Pu^{239}, U^{233}, Po^{210}, Sr^{90} and Ca^{45} were investigated. It is stated that all of these isotopes, injected intravenously as the soluble material, influenced the expression of neoplasms arising in

the lymph nodes, thymus, spleen and bone marrow.

Only results with Sr^{90} are given in detail. CF 1 mice normally exhibit a fairly high spontaneous incidence of diseases of the blood-forming organs (leukemia). The surprising results of these studies concerned the total incidence of leukemia which was not markedly influenced by any of the radioisotopes. However, if the leukemia rate for each dosage group (from 1.3 $\mu c/kg$ to 200 $\mu c/kg$) is plotted against days after injection, and comparisons made of these groups of the times required to reach a 20% incidence, there was observed a statistically significant decrease in latent period between those mice receiving 8.9 $\mu c/kg$ or more and the control mice. At the two lowest doses (4.5 and 1.3 $\mu c/kg$) there was stated to be a slight but statistically insignificant decrease in latent period.

A most interesting aspect of these studies was the lack of influence on longevity and on osteogenic sarcoma induction of the 8.9 $\mu c/kg$ dose, which was effective in decreasing the latent period of leukemia. These neoplasms of the blood-forming organs are therefore very sensitive indicators, by the parameter used, for response to irradiation.

Classification was made by Finkel or the morphologic forms of neoplasms of the blood-forming organs observed in these studies: lymphocytic, granulocytic, plasma cell and type A and B reticulum cell sarcomas. Type-specific mortality rates revealed a more striking decrease in latent period of Sr^{90} on lymphocytic and plasma cell neoplasms, and little or no effect on the other neoplastic cell types.

In examining these data for evidence of linearity and threshold it is observed that for three of the high dose levels used, the response is linear with dose; the two lower levels, however, below 8.9 $\mu c/kg$ are far removed

from an extension of this straight line. It was concluded that these data do not support the concept that the carcinogenic effect increases linearly with dose and has no threshold.

It should be pointed out that the parameter used was not induction of leukemia by Sr^{90}, but decrease in latent period and that a larger range of doses in the relatively untested area, below 8.9 $\mu c/kg$ should be evaluated.

The low leukemogenic potential of Sr^{90} in the Argonne studies discussed above is of interest. In contrast, X-rays, chronic γ-radiation, fast neutrons and P^{32} have resulted in increased frequency, lowering of latent period, or appearance of some types of leukemia not ordinarily observed to occur spontaneously.

A high carcinogenic potential on the other hand was observed in the Argonne studies for malignant bone tumors for most of the radioisotopes studied, plutonium -239 being the most potent.

It would appear premature from these limited data, concerning the influence of Sr^{90} on the appearance of leukemia in CF 1 mice to predict the consequences in man of the currently accepted maximum permissible levels of Sr^{90} from fallout, as has been attempted. Evidence which is documented in the literature some of which is discussed in this statement shows that profound differences in response to radiation are attributable to sex, strain, species, age, etc. It is quite probable that the choice of another strain of mouse may have led to a totally different response. The interesting observations of several investigators concerning the potentiating effects of radiation and other agents has received little attention but should be given serious consideration particularly in studies of chronic exposure to radiation given over long periods of time. The compound, urethane, for example is known to enhance strikingly the induction of generalized lymphocytic leukemia at levels of whole

body radiation which only slightly influences the frequency of this disease.

The action of radiation may be thought of in terms of ionization, of free radicals, of target theory or of genetic change. Recent attention has been directed toward the influence of dose-rate (intensity) as it influences the induction of leukemia in the hope that this information may allow a decision about the hypothesis that these diseases are induced through the production of point (gene) mutations. Certain basic criteria should be fulfilled by analogy with radiation-induced mutations in Drosophila: absence of a threshold, linearity of dose and response and independence of changes in the cellular environment such that radiation intensity, the rate at which radiation is delivered, does not influence the mutation rate. Preliminar information concerning the response of C57BL mice to dose rates of γ-radiation from 1.3 to 81 $r/_{hour}$ indicates a dependence for expression of thymic lymphomas on dose-rate. The conclusion drawn that "somatic mutation if it occurs is of subsidiary importance to some factor dependent on dose-rate" may be correct but applies only to the induction of thymic leukemia in the mouse, a neoplastic form known to result from indirect effects of radiation and for which some evidence exists of a threshold dose of radiation below which there appears to be no response. A pertinent question concerns the influence of intensity of radiation on the induction of other leukemias in experimental animals which probably do not represent a special and unique case.

The recent findings of Russell concerning mutation frequency in the mouse are of interest in this connection. A much lower specific locus mutation rate was obtained from chronic γ-radiation than from acute X-radiation when spermatogonia, in contrast to spermatozoa, were irradiated. The mechanisms responsible for the dose-rate dependence of mutations in metabolically active spermatogonia should be of extreme interest to those

investigators concerned with radiation carcinogenesis. These results reported by Russell raise the question of how meaningful dose-intensity data are when discussed in terms of somatic effects without more precise knowledge of recovery mechanisms, mutual interaction of cells, selection, cell death, and other variables in the immediate cellular environment.

Profound differences in the leukemogenic response of mice to chronic γ-radiation as opposed to acute radiation are known. A most recent comparative study in RF mice reveals that continuous γ-radiation from a cobalt source (1 rad/day) did not induce leukemia nor shorten life, whereas the same median accumulated dose ($\sim$500 rad) given in a single, brief exposure was highly leukemogenic and greatly reduced the life span.

The results of continuous exposure in a nuclear reactor of CBA mice have been described recently by investigators at the Radiobiological Research Unit at Harwell, England. The effect of 0.54 rads/$_{week}$ of fast neutrons was imperceptible. The higher dose of 6.5 rads/$_{week}$, although markedly damaging reproductive function, did not increase the frequency of leukemia, nor influence the latent period. A limited period of daily X-radiation was previously found to strikingly increase the incidence of leukemia in these mice.

A differential effect of chronic radiation from an external source on the induction of leukemia would therefore seem to be established from these and earlier studies. A similar differential effect has not been found, however, for other neoplasms which may be encountered.

The earlier report of an influence of chronic γ-radiation from a radium source on the production of leukemia in another strain of mouse with levels as low as 0.11 r/$_{day}$ indicates that genetic composition of the mouse may be an important variable.

The chronic effects of exposure of man or of other mammals to total radiation of a few roentgens or less are not known. The possible induction of myeloid leukemia in mice irradiated with doses as low as 16 and 32 rem are suggestive. Ovarian tumors in certain strains of mice are produced with dose levels as low as 50 r (and probably lower) and their induction is largely independent of dose rate. Mammary tumors in rats are accelerated with 25 r whole body X-radiation. The latter two, though resulting from hormonal imbalances, do suggest the possibility of neoplastic transformation at "low" levels of irradiation. P^{32} is highly carcinogenic in certain strains of rat, producing bone and skin neoplasms. Though the data are consistent with a sharp threshold it has been pointed out that one half of the carcinogenic dose approaches the average therapeutic dose for man of P^{32}. All of these responses are probably strain - or species - specific, and are mentioned to indicate the probability of response to low levels of radiation if certain conditions exist.

An increase in leukemia and other forms of malignant disease has been reported in children irradiated *in utero*, as a result of abdominal radiodiagnostic examination during the mother's pregnancy. The dose of radiation may have been of the order of 1 to 10 rads. Although these results are equivocal and still need to be confirmed by other groups, they do indicate that some consideration should be given to the possible increased radiosensitivity of embryonic and fetal tissues. A measurable incidence of developmental abnormalities has been observed following 50 r X-radiation of the mouse and presumed somatic mutations of coat color genes have been reported following irradiation of 10½ day embryos; interestingly, it was stated that the somatic mutation rate was of the same order of magnitude

as the germinal rate. Irradiation of embryos of two inbred strains of mice, RF and C57BL, has not resulted in the induction of leukemia, however. These studies are in progress in our laboratory and by Dr. Arthur Upton of the Oak Ridge National Laboratory. Both of these strains are known to have the potentiality to develop lymphocytic or granulocytic neoplasms following X-radiation in post-natal life.

More attention is being given to methods of modification of acute and chronic effects of radiation. Chemical substances, unfortunately with a low therapeutic ratio, that is, the ratio between the lethal dose and the minimal dose affecting radiation damage, are known to reduce the killing power of large doses of radiation. Their precise action is unknown. Preliminary evidence from several sources indicates their ineffectiveness, however, in modifying the induction of leukemia in mice. Reduction of acute radiation mortality in the experimental animal by lead shielding of a part of the bone marrow, or spleen, or by injections after radiation of bone marrow, or spleen cells, is now a familiar and fascinating story. It should be pointed out that the results achieved in the experimental animal were demonstrated under the best possible conditions and over a narrow range of variables. Nevertheless, the effectiveness of the introduction of bone marrow into heavily irradiated human subjects, those suffering from leukemia or those involved in radiation accidents is being studied. Preliminary results are indeed encouraging and provide a ray of hope for modification in man of radiation damage.

In summary it may be stated that our present knowledge concerning radiation and leukemia in man and the experimental animal is probably inadequate to resolve the major problems set forth in the Joint Committee's Summary-Analysis of the 1957 hearings.

Nevertheless, certain tentative generalizations of interest may be made from laboratory studies:

1. Ionizing radiations produce leukemia.

2. Chronic radiation is appreciably less effective than acute radiation in the induction of these diseases.

3. Strontium-90 deposited internally is relatively ineffective as a leukemogen, but relatively few data have been reported.

4. An indirect effect of radiation has been established for the induction of thymic leukemia but not for other types of leukemia.

5. Dose-response relationships, threshold, age sensitivity and response to fractionation and protraction of radiation differ among the several types of leukemia.

6. A dose-rate dependency is characteristic of the induction of thymic leukemia.

7. Present data, though inadequate, are consistent with the concept that there exists a threshold dose below which leukemia will not result from radiation.

There exist certain largely unexplored areas where little information is available. These include the influence of radiation on fetal and embryonic tissues, the role of co-carcinogens and environmental hazards in potentiating radiation effects, chemical and biologic modification of radiation carcinogenesis and other somatic damage, the relative biologic effectiveness of types of irradiation and dose-response relationships. These require our immediate efforts.

RADIATION CARCINOGENESIS

Dr. Lloyd W. Law

I. Introduction

Two of the most important and well-established effects of radiation in animals are the induction of neoplasia and the production of changes in genetic materials. It is the purpose of this report to discuss, principally, the knowledge acquired to date, and particularly in the last few years, concerning radiation carcinogenesis. Where it applies to an understanding of carcinogenesis, knowledge and concepts will be discussed concerning radiation and genetic changes. No attempt will be made to cover the field in historical perspective, nor in detail. This has been done admirably in recent years by Brues (1951; 1954); Furth and Lorenz (1954), and others. It is proposed to discuss those findings which relate specifically to gaps in our knowledge, to point out some possible deficiencies in interpretation, and suggest what investigations may be feasible and worthwhile.

Certain basic questions need be asked concerning radiation carcinogenesis which attempt to define the nature of radiation effects, of the neoplastic process itself and for the purpose of establishing permissible levels of exposure to radiation:

1) What are the radiation doses necessary to initiate carcinogenesis, for example, in skin, bone and certain susceptible tissues? There appears to be an association between extremely high radiation doses and neoplasia for man. Do these doses really represent the doses necessary for initiating neoplasia?

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2) What is the role of dose rate and of fractionation and protraction of radiation in the induction of neoplasia?

3) What is the carcinogenic potentiality for doses of radiation in the range of, and under conditions analogous to, radiodiagnosis?

4) What is the evidence for the direct and indirect carcinogenic effects of radiation?

5) The latent period in radiation-induced neoplasia, especially in man, appears extremely long. What is the significance of this?

6) In some instances, responses to radiation appear similar for many species of animal, in contrast to the species-specific response to chemical carcinogens and viruses. What is the significance of this?

7) What is the evidence for a linear and non-threshold relationship between radiation dose and incidence of neoplasms?

8) Is there evidence of modification of the response to radiation through other carcinogens, cocarcinogens and other factors?

II. Radiation Carcinogenesis in Man

1. Skin Neoplasms

Although the histologic features of skin cancers are the same whether they occur spontaneously or result from radiation, there is a striking difference in the pathologic response. The preponderance of spontaneous carcinomas are of the basal cell type, whereas, those arising from a previously normal skin, receiving irradiation, are of the squamous-cell type. This difference, perhaps, is not of significance when it is considered that the radiation-induced carcinomas which do appear on the face and head are morphologically similar to the spontaneously occurring basal cell type, whereas the majority of X-ray neoplasms of the skin occur principally on chest, arms and hands. Spontaneous squamous cell carcinoma is more common in these regions. Skin sarcoma is said to be a rare sequel to radiation. Of interest is the indication of a shorter latent period to carcinoma formation in cases of individuals with dermatoses given X-ray therapy.

Though dermatitis, following chronic irradiation or erythema, desquamation and scars with ulceration following acute radiation, have been emphasized as changes preceding skin neoplasia, it is not known what, if any histologic or functional changes in skin predispose to carcinoma. It would appear extremely dangerous to assume that certain precancerous changes precede the development of malignancy, particularly at present with the reduced dosages employed in therapeutic procedures giving little direct evidence of the hazards of such procedures.

Extremely high doses of X-rays, probably in the range of 1000 r per year, have been recorded as being received by persons who later presented skin carcinomas. The question of a threshold dose has not been resolved for man. It is emphasized repeatedly, especially by Brues (1949) and by Evans (1951), that from human experience, total dosages in the order of 1000 to 10,000 rep for α-radiation and of the same order for X-radiation and β-radiation are necessary to induce low incidences of neoplasms, probably 10%, after prolonged latent periods of 15 years or more. The question of prolonged latency in adults receiving massive doses of radiation requires further study. Comparable situations are, in all probability, found in the latent carcinomas of the prostate, gastrointestinal tract and lung in man and in dormancy of neoplasms, as seen in long-delayed recurrences and metastatic growths after apparently complete removal or destruction of such growths.

The skin is relatively more resistant to the induction of neoplasia by irradiation than other tissues in man and in experimental animals. Radiation, which induces some neoplasms in short periods of time and following a single exposure, as discussed later, appears harmless to the skin.

Many of the studies of radiation carcinogenesis of the skin done in experimental animals relating to dosage rates, fractionation, induction time and other variables, have been at extremely high dosage levels (See

Glücksmann, 1958), and the histogenesis of such neoplasms has been discussed in terms of injury, healing, regeneration and such sequellae. Yet, there are findings to suggest that relatively low levels of radiation are carcinogenic to the skin under certain conditions, such as peculiar susceptibility of strain, modification by cocarcinogens, etc. Some of the findings will be discussed later.

2. Neoplasms of Bone

Data concerning the relationship of radiation to induction in man of bone tumors, chiefly osteogenic sarcomas, have been obtained from three sources: radium dial painters, patients who have been treated with radium salts for therapeutic purposes and patients receiving X-radiation of bones, particularly those with benign or inflammatory lesions. These data are unique in several respects. The radiation conditions were known and at least among the radium dial painters, the total population at risk was determined. Some quantitative determinations have been made but are incomplete. Radium burden has been determined in most of the subjects, but only recently has the relationship of the radium burden to time been taken into consideration and estimates made of the accumulated radiation dose (See Looney, 1958). Obviously, many deficiencies appear in the data and it is questionable how much reliance may be placed upon them to draw other than general conclusions. Recent autoradiographic studies made by Hindmarsh and Vaughan (1957) have shown areas of concentration in bone reaching 16 times the mean value. The changing radiation doses in tissues resulting from excretion and redistribution are unknown. The role is undetermined of the isotope mesothorium as a carcinogenic agent. The groups under study have been very heterogeneous as to age, sex, radium burden, etc. Certain findings of interest have been reported, and the most

recent by Looney (1958). Sarcomas of the bone have been found in 11 of the 78 patients evaluated in the recent Boston-Chicago studies; 7 sarcomas in 50 patients receiving radium and 4 sarcomas among the 28 dial painters. One patient with a low terminal body burden of 0.8 micrograms developed a bone neoplasm. Radium was given medically 24 years previously and it is estimated that the total accumulated dose from mesothorium was 50 rads and from radium 1100 rads, a level at which some major skeletal damage in other patients was observed. Again, latent periods are long and although it is suggested that bone sarcomas arise from tissues which suffer severe disorganization from localized or systemic radiation damage, it is not known precisely that they arise only in such areas. The probability must be considered that lower levels of radium concentration may induce neoplasms in the absence of roentgenographic change or major skeletal damage. The radiation doses calculated in radium dial painters and in radium-treated patients do not necessarily represent the doses necessary to initiate neoplasia. Furthermore, as pointed out by Looney, no information is available of the effects of radium in man retained over a lifetime, nor are there data available concerning the effects of radium introduced in childhood. The appearance of osteochondromas in irradiated infants (Simpson and Hempelman, 1957) for example, suggests the likelihood of an increased susceptibility in the lower age groups.

Many isolated cases, since the first report in 1922, have been reported of the appearance of bone tumors, following irradiation from an external source. In the collection of cases given adequate study, it is clear that the malignant condition has arisen pricipally in patients irradiated for benign conditions of the bone, such as ossifying fibrosis, bone cyst, and in cases where inflammatory processes may be expected. There is similar evidence from experimental animals, particularly in the induction of sarcomata, that the dose of radiation necessary for induction may be reduced by the presence of inflammation or infection in the irradiated tissue. In the most recent report on this subject, 17 patients presented bone sarcoma, 16 of these following irradiation for benign conditions (Sabanos, et al, 1956). It is possible that in certain cases, the malignant process was merely hastened by irradiation. The role of osteoblastic activity at the time of irradiation needs further attention. An interesting and convincing group of cases has been presented by Hatcher (1945) in which osteosarcomas were observed in patients given radiation for reasons other than benign conditions of bone.

3. Neoplasms of the Hemopoietic and Lymphoid Tissues

Ample evidence is available from several sources that whole-body irradiation, or of irradiation of large volumes of blood-forming tissues alone, at fairly high dosage levels, leads to leukemia in man. Clinical evidence obtained from survivors of atomic explosions, from individuals irradiated for diagnostic or therapeutic purposes, and from a study of radiologists, strongly suggests an increase in the frequency of these neoplasms arising as a result of, or a possible result of radiation exposures. Although an attempt has been made recently by Lewis (1957) to develop quantitative estimates of the somatic effects of ionizing radiations in the induction or acceleration of leukemia in man, chiefly from data obtained from the above sources, it is clear, as shown in the following discussion, that the assumptions and extrapolations made, represent one way of treating the data, and probably the most pessimistic way. It is apparent, also, that certain investigators may be too optimistic, on the basis of present evidence, of the possible hazards to the individual of enviromental contamination and other radiation sources.

American radiologists are said to be more prone to develop leukemia than other physicians, or other males of the same age group in the population. Yet, the conditions under which radiologists develop leukemia are most likely non-leukemogenic in experimental animals. Exposure of only

small parts of the body has occurred, non-uniformly, at relatively low, average-tissue doses. A mean dose of 2000 r, received over many years, has been calculated for radiologists (Evans, 1950). Here non-uniformity of exposure, and as pointed out by Brues (1958), the lack of data concerning distribution of individual doses make it unwise to use an estimated mean dose in discussions of linearity of dose response. However, the response among radiologists to radiation may be similar to the response observed in CF_1 mice to bone tumors, following introduction of Sr^{89}. Here small, divided doses of radiation appear to increase the response (Brues, 1949).

The detailed and comprehensive studies of Court-Brown (1958) and Court-Brown & Doll (1956, 1957) reveal, without a doubt, that X-irradiation in man is leukemogenic, particularly for the acute forms of disease and for chronic myeloid leukemia. These studies show further that in patients suffering from ankylosing spondylitis, the leukemia rate increases with dose of radiation within the limits of radiation employed. It is questionable that the data are sufficient to determine whether or not a threshold exists below which leukemia will not be induced since the mean doses to individuals have all been relatively high.

In treating these data, retrospectively, to arrive at some relationship between radiation dose and induction of leukemia, Court-Brown and

Doll and Court-Brown have made certain assumptions. It was assumed that the probability of developing leukemia among radiated spondylitics was the same as for the general population of the same age and sex. Though Abbat & Lea (1956), found, in a limited group, no striking evidence of an increase in leukemia of non-irradiated spondylitic patients, this does not answer the question of differential susceptibilities of normals versus spondylitics to the leukemogenic effects of X-irradiation. It was also assumed that fractionation and protraction of irradiation did not influence leukemogenesis in these patients. Though, as mentioned previously, the dose-response curve is true for spondylitics, it is questionable, as discussed by several authors, that the data are compatible with a linear relationship and with a no-threshold effect. As pointed out by Lamerton (1958), the tentative conclusions reached by these authors, as a working hypothesis, concerning the dose-response relationship is to a great extent implicit in their method of treating the data. A serious deficiency of data obtained in such a study is the lack of information on patients treated with lower doses of radiation.

Several reports of the studies of leukemia among survivors of the atomic bombings of Hiroshima and Nagasaki indicate clearly the relationship between ionizing radiations and these diseases (Folley, et al. 1952; Lange, et al. 1954; Maloney & Kastenbaum, 1955). It is indicated that the

morphologic forms encountered are those found spontaneously and occur among the radiated populations in the same proportions. It appears safe to conclude, from the present available information, that those survivors who were within 1500 meters of the hypocenter, an area in which the mean dose has been estimated at 125 rad or above, had a significantly higher incidence of leukemia than those survivors beyond this distance. Attempts to establish a quantitative relationship between dose of radiation and the probability of developing leukemia among survivors are beset with many difficulties. If it is assumed that the data, to the present, are truly representative and may be corrected for migrations of the population, the dosimetry program now being conducted by the Atomic Bomb Casualty Commission, in which attempts are made to assign each survivor a dose of radiation, for neutrons and/or gamma rays, may be extremely valuable in estimating quantitative relations. These data will provide, also, a sufficient sample of the population at risk to determine incidence rates.

There are many reasons to consider as premature any attempt to conclude linearity of dose-response, the non-existence of a threshold dose, and to define estimates of the probability of developing leukemia from the Japanese data: 1) there are many uncertainties concerning the estimation of dose (See Mole, 1958), 2) there has occurred only a small number of leukemias in the 1500-2000 meter zone, 3) the question of adequacy of

control data based upon national mortality, 4) and the incompleteness of the data. For example, there is no indication of a plateau being reached or of a diminishing incidence of leukemia among the survivors (See Wald, 1958). The incidences of leukemia prevailing at present may be misleading and indeed be quite different after all members of the population have died.

The possibility of induction of leukemia at dosage levels in the diagnostic range has been introduced by Stewart and co-workers (1956). It was found in a survey of childhood leukemias conducted in England and Wales for the years 1953 through 1955, that the proportion of mothers receiving pre-natal abdominal radiographic examinations, principally pelvimitries, was greater in the case of leukemic children than in a group of non-leukemic children. The probability of developing leukemia and of other neoplasms, was approximately doubled in the first 10 years of life in those children whose mothers received diagnostic X-rays. Again, such retrospective studies raise many objections. The question of why radiography was accomplished in gravid mothers is not answered, and may bear some causal relationship to leukemia in children. Are factors or circumstances other than radiation, for example the allergic status of the mother, likely to be responsible for similar findings if these factors or circumstances, rather than radiation, are considered as the variable in a study of such data? There is much than can be learned from studies

of similar neoplasms in experimental animals, but there appears to be little or no information relating to leukemogenesis in foetal and embryonic tissues of extremely low doses of radiation.

There is little information concerning possible differences in susceptibility of adult versus tissues of children to radiation. The increased incidence of thyroid neoplasms and adenomas and apparently of leukemias in children treated by X-rays for thymic enlargement are suggestive (Duffy & Fitzgerald, 1950; Simpson & Hempelman, 1957). As little as 200 rads of X-rays to thyroid tissue is stated to be carcinogenic under these conditions. More recently, Wilson and co-workers (1958) have observed thyroid carcinomas in 6 patients who received radiation by radium or X-rays in infancy or childhood. The treatment was for cutaneous lesions and as low a dose as 130 r was used. Adults, on the other hand, treated for hyperthyroidism with I^{131} at dosages of 10,000-15,000 rads have not, as yet, experienced thyroid neoplasms.* Further, as pointed out by Doniach (1958), aside from the production of malignancy of thyroid tissue in children, following relatively small doses of radiation there appears to be no evidence of a TSH promoting action. This indeed is in contrast to the situation encountered in experimental animals where most radiation-induced neoplasms, following low doses of radiation, appear to depend upon hormonal influences. The striking increase of

* The absence in the literature of cases of thyroid carcinoma attributable to previous irradiation may be entirely misleading, as pointed out by Goolden (1958), since apparently a large-scale survey has not been done. Recently, however, there have appeared reports of 4 neoplasms of thyroid tissue in individuals treated for thyrotoxicosis, exophthalmic goitre, or tuberculous lymphadenitis (See Goolden, 1958).

osteochondromas in infants irradiated very early in life, in contrast to a difference in response of adults radiated for thyrotoxicosis, for example, points to a probable striking difference in response of adults and children to radiation.

It is unfortunate that the Japanese data do not lend themselves to a study of the influence of radiation on the probability of developing leukemia in those individuals under 5 years of age, since these data, of all considered, could conceivably provide some evidence of the inducing capacity of low doses of radiation.

Some of the recognized sources of error have been pointed out in the data available to date among the bombed Japanese populations. Recently, Kimball (1958), has reemphasized additional errors in interpretation in his discussion of the statistical reliability of existing data, particularly the Japanese data, relating ionizing radiation to human leukemia.

The confidence of Lewis in his interpretations of the leukemogenic properties of radiation is more or less balanced by the confidence of others in assuming that the present data may be interpreted to show the lack of linearity in dose-response relationship and the existence of a threshold; when, in fact it would appear that the data are insufficient to justify any hypothesis concerning dose-response relations of any kind.

The role of radiostrontiums in the induction of neoplasms of reticular tissues in man is unknown. Reports of the leukemogenic capacities of

Sr^{89} and Sr^{90} in mice and rats have been given in preliminary fashion, but the results are equivocal (Watanabe, 1955; Casarett, et al, 1958).

In discussions of the relationship between spontaneous leukemia and natural background radiation, prompted in part by the suggested "formidable increase in leukemia in the United States in the past few decades," it is well to keep in mind the recent survey of Gilliam and Walter (1958). Their data concerning mortailty from leukemia in the United States, indicate an actual decline in the rate of increase; with indications of stabilization, in all age groups, except for those beyond 75 years of age the decline is particularly evident in the lower age groups. It is stressed by Burnett (1958), that radiation is probably acquitted of any prominent role in the increase of leukemia in man, although he accepts present data as indicating that radiation is leukemogenic and that the dose-effect relation is linear and without threshold. The peak of incidence between 3 and 4 years for whites (Cooke, 1954; Hewitt, 1955; Walter & Gilliam, 1956), suggests to him the likelihood of a mutagenic stimulus active about the time of birth. He has recommended that a search be directed towards something associated with pregnancy or neonatal care which has become "progressively more available in the U. S., Australia and England since 1942.....and seems to be conspicuously absent in the American Negro's way of life." However, more attention also should be

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directed toward the question of whether the striking difference in the peak incidence between whites and non-whites is, in fact an artifact. Further, is the peak incidence at 3 and 4 years a new development or have better population data been available since 1940, the first date at which a reliable accentuation of this peak incidence is seen?

Retrospective studies of samples of the human population receiving radiation appear to be one method of arriving at some of the pertinent answers required. Unless the errors involved in such studies are recognized and stringent attempts are made to avoid bias in prejudging the issue in choice, treatment or analyses of the data, these results will always be equivocal. It is clear that much may be learned from controlled studies using the experimental animal.

4. Other Neoplasms in Man

Reports of radiation-induced neoplasms at other sites in man, apart from those already discussed, are indeed rare and not too convincing. A characteristic feature of all such neoplasms, said to be related to radiation of various types, is the exceedingly long latent period. Recently, of interest, are the observations of hemangioendotheliomas of the liver in those individuals receiving thorotrast, confirming the original report (Tesluk & Nordin, 1955; DaSilva Hurta, 1957).

Lung cancer, chiefly squamous cell carcinoma, was recognized during the latter part of the last century as one of the major causes of death among the uranium miners of Schneeberg and Joachimstal. The annual mortality rates for the two populations was approximately 10 per 1,000. Comparable populations of Vienna males showed a crude cancer mortality of 0.34 per 1,000 in comparison. A nearly equal number of the miners died from silicosis and/or tuberculosis (Peller, 1939; Sikl, 1950). When it was recognized that a heavy load of radioactivity from radon and its daughter products was characteristic of the atmosphere in the mines of both locations, this was considered a possible cause of the lung neoplasms. Some idea of the population at risk in these communities, along with determinations of radon concentrations made this a most interesting model for study. From the numerous studies made of the radon content of the mine air, if 3×10^{-9} curies per liter is accepted, the total radiation dose on the

upper bronchial epithelium is estimated to be 600 rep, assuming 17 years as the average time to appearance of neoplasm. This provides no idea, however, of the effective dose for tumor induction if radon is to be implicated as the carcinogenic agent. A cause and effect relationship is by no means clear. The presence of arsenic, cobalt and other metals in the contaminating dust of those mines, leading one to suspect the possibility of cocarcinogenesis, and a 50% incidence of silicosis and/or tuberculosis of the mine workers are variables which must be considered. The negative findings in populations where a high radon content of the atmosphere is expected (Lorenz, 1944), make difficult a true assessment of the role of radon and its daughter products in the induction of carcinoma of the lung.

III. Neoplasms in Experimental Animals Following Radiation

Much has been learned from animal experimentation concerning mechanisms of radiation carcinogenesis. Some of the meaningful information concerns profound species and even of strain differences in response to radiation, particularly where it is obvious that the response is of an indirect nature, the influence of ion density, of dose-response relations and of dosage rates for certain neoplasms, the influence of single versus divided doses, some indication of the relative biologic effectiveness (RBE) for various radiations, particularly for bone neoplasms and malignant lymphomas of mice, apparent differences in mechanisms involved as indicated by the contrasts in response of skin to ionizing and ultraviolet radiation and between chemical carcinogens and ionizing radiation.

As pointed out, certain aspects of interpretations made from the best available data obtained from limited human populations, those with a high body burden of radium, survivors of the Hiroshima and Nagasaki bombings and those patients with ankylosing spondylitis treated with deep X-ray therapy, are equivocal. Despite the feeling of some investigators that the quantitative data obtained on animals cannot be directly applied to man and that the types of neoplasms in mice are not strictly comparable to those occurring in man, it is clear that the type of information needed is obtainable only from animal experimentation.

If it is desired to obtain a dose-response curve, for example, for the induction of neoplasms, three types of information are needed: the number exposed to irradiation, the number developing neoplasms, and the dose of irradiation. A large number of doses are needed before the relationship may be established. Further, the response of neonatal tissues may be quite different from the response of tissues of adult animals, and it is reasonable to expect that other variables such as sex, strain and cocarcinogenic materials will provide clues to mechanisms. In the clinically important range described by Mole (1958), to be in the 0.05 to 1% range of effect for carcinogenesis, in contrast to the 5 to 100% range of effect provided by most experimental information, it is doubtful that information is yet at hand to be quantitatively usable. Even in the most extensive experiments reported to date on the production of bone neoplasms using different dose levels of bone-seeking radioisotopes, and particularly Sr^{89}, it is pointed out (Mole, 1958), that the shape of the dose-response curve may be determined where the incidence of bone neoplasms is 10% or more, but below this level the data are probably insufficient. At low dose rates the excess in neoplasms or other biologic measurements in the treated group over the controls would be small and extremely large numbers of animals would be desirable, depending upon the doses being studied and the incidence of neoplasms under consideration (Neary, et al, 1957). It is

obvious that controls must be stringent in such experiments and that a high degree of genetic homogeneity must exist among the population of animals being used. Selection of a strain of animals should be made which is vigorous and long-lived and in which there is relatively little natural variability in life span and in which the background level of spontaneous neoplasms is not a complicating factor. Some of the difficulties inherent in such experiments and interpretations of results will be discussed.

As will be seen from a discussion of some of the data from experimental animals, it is doubtful that for a given neoplasm in a given strain of animal the question of threshold, below which a neoplastic response occurs, or of the carcinogenic effects of dosage levels of the order of a few roentgens have been established beyond doubt.

Much of the early work on radiation carcinogenesis in experimental animals attempted to duplicate and reproduce the conditions of the early radiologists and their patients. Repeated, small doses of radiation localized to various parts of the body were given over long periods of time. Following the first report of carcinogenesis by such procedures in 1910 by Marie, et al, there appeared a series of descriptive reports of carcinogenesis in several species of animals. Later it was shown that a single, relatively small dose of radiation (in the order of 1000 r) could produce neoplasms in inflamed tissues. This work was extended by Lacassagne

(1933), Lacassagne & Ruddi (1943), Lacassagne & Vinzent (1929), and by Burrows, et al, (1937), but unfortunately little has been reported since on this interesting phase of radiation carcinogenesis.

Whole-body exposure to radiation was observed by Krebs and associates (1930) to induce lymphocytic leukemia in mice and this was followed by the reports of Furth & Furth (1936) and Furth & Butterworth (1936), in which the influence of whole-body X-radiation was studied in relation to the appearance of leukemia, ovarian and lung neoplasms. There followed well designed experiments, using acute and chronic whole-body radiation, neutrons, nuclear detonations, β-radiation from external sources, instillation of various α and β-emitters by several routes. Some of these results will be discussed as they influence neoplasms of various organs.

1. Neoplasms of the Skin

Carcinogenesis of the skin, as a result of radiation, appears to be a rather complex process in which there is an extended latent period and the productivity of neoplasms is low. This is particularly true when radiation is localized and ulceration is produced by a severe dose of radiation (Glücksmann, 1958). Here, neoplasms arise from regenerating tissue which replaces that destroyed by direct radiation similar to carcinoma formation in man. The yield of skin carcinomas in rats given a single dose of 12,000 rads from an electron beam was 23 $\pm$ 8.5% after 26 months, whereas a single dose of 2,300 rads failed to induce skin neoplasms. These findings indicate that with the strain of rat employed in this type of severely localized radiation, in an area of 2.5 cm diameter, a certain amount of tissue destruction is necessary to initiate carcinogenesis. The question of the possibility of carcinogenesis of skin occurring in localized areas, through the use of small non-damaging doses, appears not to have been thoroughly investigated.

In contrast to the results obtained with high levels of radiation directed to a narrow tissue field, several investigators have reported skin cancers in rats following whole-body exposures to X-ray at ranges well below the exposures from the electron beam. In some cases protection of rats (Brecher, et al, 1953; Finerty, et al, 1953), to approximate LD-100

levels of 660 to 1,000 r was achieved by using parabiosis or protective chemical agents. In the experiments of Koletsky and Gustafson (1955), in which the largest yield of basal-cell carcinomas and skin sarcomas was achieved, unprotected survivors receiving a single exposure to 660 r X-radiation were studied. So-called Wistar "white" rats were used by these latter investigators and the likelihood should be further studied that this particular stock is extremely sensitive to radiation. Earlier studies by Koletsky and Christie (1954) revealed the induction of osteogenic sarcomas and carcinomas of the face in these rats with relatively low levels of p32.

Shallabarger, et al (1957), interestingly, found no cancerigenesis of the skin, but a strikingly early appearance of mammary neoplasms in Sprague-Dawley rats given sublethal X- or γ-radiation and localized β-radiation from a p32 source. The β-radiation which was delivered to 3 separate areas of the skin, each area receiving either 1320 or 2640 rep, appeared not to influence the induction of neoplasms.

Henshaw and colleagues (1949); (1951) have reported investigations of the effects of periodic and single surface exposures of fast and slow neutrons, γ-rays and β-rays from P^{32} in Sprague-Dawley rats and CF_1 mice. Skin neoplasms were not observed from exposures to the penetrating radiations. Response to sublethal doses of β-radiations led to skin neoplasms

of various histologic forms (although not specified) even in areas where no prior skin lesions existed. In these experiments the LD100 for Sprague-Dawley rats was stated to be $\sim$ 7,000 rep and for CF_1 mice $\sim$ 4500 rep. Skin neoplasms appeared early, beginning at 6 months in rats, following a single dose of 4500 rep and this appeared to be a much more efficient method of induction than following chronic exposures. Repeated daily doses of 0.5 and 5.0 rep appeared to be non-carcinogenic, but 50 rep daily, induced multiple neoplasms in all rats.

Although some epilation and suppuration of the skin with open lesions appeared following β-radiation in the sublethal range, a stable recovery state was reached over a period of weeks, long before the appearance of the first neoplasm. This indicates that transformation to neoplasia (carcinoma) may not always be dependent upon ulceration and subsequent regenerative conditions which seem important in cutaneous neoplasms in man and in experimental animals receiving radiation from an electron beam directed to a small localized area.*

* Although in some publications, Glücksmann and colleagues have used the term β-rays, it has been suggested that cathode rays or fast electrons be used for the radiation delivered by an electron beam accelerated in a Van der Graaf generator. The differences in response of experimental animals observed by Glücksmann and colleagues, in contrast to the results reported by Henshaw and his group, using β-rays from a ^{P} 32 source, may indeed result from differences in properties of the radiation. (See Boag & Glücksmann, 1956)

Relatively few data are available concerning dependence of yield of cutaneous neoplasms on dosage, fractionation of radiation and on dose rate. The data of Schubert, et al, (1956) are insufficient to establish, as claimed, a quantitative relationship between dose and induction of skin neoplasms.

Little effort appears to have been directed toward the interesting possibility of the influence of cocarcinogens. The report of Shubik and co-workers (1953), indicates this as a possibility. These investigators found that croton oil administration to the skin of mice after β-radiation from thallium^{204} led to papillomata formation (20% in 3.5 months), followed by malignant transformation at 8 months. The dosage delivered, 800 rep, was stated to be below the level suggested as being the threshold. Similar results have been recorded by Boag and Glücksmann, (1956). Here a chemical carcinogen 1:2:5:6:-dibenzanthracene, itself non-carcinogenic at the levels employed, when applied after radiation from a cathode ray source, at non-carcinogenic levels, resulted in sarcomas as well as carcinomas in Hooded rats.

2. Neoplasms of Hematopoietic and Lymphoid Tissues

Much attention has been given to a morphologic description of neoplasms of the skin, ovary and other tissues, arising following exposure to radiation; but unfortunately, neoplasms arising from the hematopoietic and lymphoid tissues have been lumped together in many publications as "mouse leukemia." Generalizations that have been made concerning radiation and "mouse leukemia" may be therefore entirely misleading, as will be discussed. A morphologic and biologic classification of the various neoplasms arising in these tissues has been given by Dunn (1954).* Clearly, more emphasis must be placed on classification of the neoplasms of reticular tissue induced in experimental animals, particularly the mouse. As shown by Dunn and others, the leukemic diseases as in man, are not a unitary group and much evidence is accumulating to show that they have diverse etiologies.

Kaplan and colleagues (See Kaplan, 1954), have published extensive observations on the response of C57BL/Ka strain mice to radiation. It is clear that certain generalizations may be made concerning this strain:

* The comprehensive term "neoplasms of reticular tissue," as defined by Dunn (1954), will be used here when possible. The terminology used by the investigator is retained, for the most part, however, when references are made to specific observations reported in the literature.

1. There exists a threshold, in the region of 200-300 r below which lymphocytic neoplasms of thymic origin will not appear. These neoplasms are called "malignant lymphomas" by Kaplan. The peak incidence of thymic lymphomas is reached within 12 months and some thymic lymphomas make their appearance by 100 days.

2. Extra-thymic lymphocytic neoplasms are not induced by whole-body irradiation in this strain in contrast to other strains of mice. Thymectomy nearly completely inhibits the induction of lymphocytic neoplasms.

3. Whole-body irradiation is necessary. Shielding of a small part of hematopoietic tissues, or introduction intravenously of isologous, but not homologous bone-marrow, prevents the induction of these lymphocytic neoplasms (Kaplan, et al, 1958).

4. Repeated exposures of X-rays are more leukemogenic than single exposures.

5. An indirect effect of radiation is suggested. Lymphocytic neoplasms originate from non-irradiated thymic implants grafted into thymectomized, irradiated C57BL, (C57BL x C3H)F_1 and (C57BL x A)F_1 mice. (See Kaplan & Brown, 1954; Kaplan, et al, 1956; Law & Potter, 1956; 1958).

The response of C57BL mice to radiation may indeed represent a special and unique case and there is some evidence to be discussed, which indicates this. The recent studies of Krönig and Sigmund (1955) who

observed the production of 46% lymphomas in C57BL mice following massive, localized X-radiation to a dorsal skin fold, may represent a quite different mechanism in the induction of neoplasms of reticular tissues. A comparison should be made of the biology and morphology of the neoplasms encountered under these conditions of radiation with the typical thymic lymphocytic neoplasms appearing following whole-body irradiation.

Induction of neoplasms of reticular tissue in LAF_1 hybrids and in RF strain mice have been observed by Furth and co-workers (Furth & Upton, 1949; Upton, et al, 1954; Furth, et al, 1954; Upton, et al, 1956), following exposure to fast and slow neutrons of the atomic reactor, to whole-body X-radiation and to thermal neutrons with contaminating γ-rays following a nuclear detonation.

Lymphocytic neoplasms of thymic origin in LAF_1 mice increased in proportion to the dose when exposed to a nuclear detonation. A greater frequency was seen among males, particularly above 700 r, and a threshold dose of 500 r for males, but much lower, perhaps below 192 r for females, was observed; however, the incidence of reticular neoplasms (morphological form unspecified) is higher among the non-irradiated controls and the frequency of induced neoplasms among both sexes inthe 367-424 r group is probably not significantly different from controls. It is stated that the incidence of all non-thymic reticular neoplasms which includes generalized

lymphocytic and granulocytic (rarely) neoplasms and reticulum cell sarcomas were not influenced by this radiation with dose ranges extending from LD_0 to LD_{100}. The LD_{50} (30 days) was approximately 755 r. The incidence of non-thymic leukemia in this strain, and also as reported elsewhere for RF mice, actually decreased with increasing dose. This may merely indicate a difference in expression rather than induction of lymphocytic neoplasms in irradiated mice. The earlier appearing neoplasms may be confined to thymic tissue.

More precise data have been published in extension of this work in a comparison of the RBE of fast neutrons and X-rays on the induction of neoplasms of reticular tissue in RF mice, (Upton, et al, 1956). The incidence of lymphocytic neoplasms increased and mean latent period decreased with the dose of radiation and with fractionation of both neutrons and X-rays. Myeloid leukemia developed later in life, was more common in males, and appeared in mice receiving relatively lower doses of radiation (110 rads of neutrons and 119 rads of X-rays). In an earlier publication (Furth & Upton, 1949), it appeared from the data, given only in graph form, that dose levels as low as 16 r and 32 r, X-radiation or thermal neutrons, induced myeloid leukemia in females of the RF strain. Fractionation did not influence the frequency nor the latent period of granulocytic neoplasms (myeloid leukemia). Although it is concluded that the frequency of myeloid leukemia increased

with dosage, this is not borne out by the data. There are some indications from these data, and from an earlier report of Henshaw, et al, (1947), of a lower frequency of lymphoid neoplasms in neutron-radiated, compared with X-rayed mice. The significance attached to a greater frequency of myeloid leukemia in neutron-radiated mice may be the result of sensitive mice not succumbing early to thymic leukemia.

A more detailed investigation of some of the variables influencing induction of lymphocytic and granulocytic neoplasms in RF mice following X-radiation has been reported recently (Upton, et al, 1958). Resort to thymectomy which eliminates thymic lymphocytic leukemia, and the use of adjusted incidences to correct for mortality from causes other than leukemia, have made clearer the differences in this strain of mouse, in the induction of these two different morphologic forms of neoplasm. Frequency of myeloid leukemia was greatly increased at lower dose levels (150 r) in contrast to the frequency of thymic lymphomas. This suggests that the threshold dose is well below 100 r, but exposures below 150 r were not accomplished. Fractionation of dose did not influence the frequency or latent period of myeloid leukemia, nor did thymectomy.

The dose-response curve for myeloid leukemia, in contrast to that for thymic lymphomas, leveled off above 150 r indicating a "saturation dose" may be approached above this level. Myeloid leukemia predominated in the

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males, thymic lymphomas in females, and these sex differences persisted in castrated mice. In RF mice, but not all strains, irradiation in the early neonatal period resulted in an increased frequency of thymic lymphomas; there was no indication of an effect on myeloid leukemia, however.

The significance of these results in RF mice pertaining to the induction of granulocytic neoplasms are apparent since single exposure to ionizing radiation in man appear to be highly carcinogenic in inducing chiefly myeloid leukemias. There is also some evidence from this work of Upton and colleagues that shielding of the pelvis and lower extremities, during whole-body irradiation, is less effective in reducing myeloid leukemia induction than lymphoid leukemia induction. It would be of interest to determine the influence of localized spinal irradiation at the accumulated dose and over-all exposure time similar to that given ankylosing spondylitics.

The interesting experimental design described by Mole (1956) needs to be extended to other strains of mice and other experimental animals. It is quite likely to provide answers to the question of direct versus indirect modes of carcinogenesis by ionizing radiation, of threshold dose and the maximum permissible dose rate. In CBA mice, the incidence of leukemia was found to be little affected by fractionation and protraction with a total dose of 1000 r of X-rays. Thymic and non-thymic lymphocytic neoplasms predominate in irradiated CBA mice. With total doses above 1000 r the

leukemia incidence did not increase. Interestingly, large single doses superimposed upon fractionation, appeared to exert a therapeutic effect. These dose-response relationships have been investigated further to levels as low as 500 r, total dose (Mole, 1958). Interesting findings, many contrasting with the results of Kaplan, and of Furth and co-workers, were as follows: 1) 500 r was ineffective in inducing thymic lymphocytic neoplasms (lymphomas), whereas increasing the total dose to 750 r, apparently, regardless of fractionation and protraction, increased the yield tenfold. Increasing the total dose beyond 750 r did not strikingly increase thymic lymphomas; 2) the incidence of generalized leukemia (lymphocytic neoplasms) steadily increased with time throughout 30 months whatever the total dose. The peak incidence of thymic leukemias, in contrast, occurred within a 12-month period; 3) the incidence of reticulum cell sarcomas was decreased following all schedules of radiation given.

The question of classification of the non-thymic, generalized leukemias discussed by Mole must be resolved. Do they really represent a homogeneous group of neoplasms as indicated? Further information in CBA mice, at total doses below 500 r, may indeed be very interesting and illuminating.

Brues and Sacher (1952) have investigated the response in a time-morbidity study of CF_1 mice exposed to whole-body, fractionated X-radiation over 54 days, at total doses of 400, 800 and 1200 r, respectively. The

lymphoma incidence was found to rise to a peak approximately 150 days after beginning treatment and then declined to the normal rate. The responses to the different dose levels had the same pattern and differed only in amplitude. In contrast, in Sr^{89}-treated CF_1 mice, where dosage was maintained, the mortality rates from bone neoplasms increased constantly with time, as is characteristic of the development rate of many other radiation-induced neoplasms. These authors suggest that lymphoma induction, following whole-body, X-radiation is governed by a "fundamental injury process." It is unfortunate, in the light of differences in response of different strains of mice, that the forms of neoplasm termed "lymphoma" were not more fully characterized. The time limitation on the appearance of these neoplasms suggests the development of thymic lymphocytic neoplasms in CF_1 mice similar to the response of C57BL mice.

A series of investigations by Lorenz, et al, (1954) were reported on the biological effects of chronic γ-radiation from a radium source (Lorenz, 1950; Lorenz, et al, 1947). The pertinent results to be discussed here concern LAF_1 mice (C57L x A) and C3H/He, which were continuously exposed, beginning at one month of age to 0.11, 1.1, 2.2, 4.4 and 8.8 r per day. At these levels, the highest total dose being 5900 r, no striking peripheral blood nor bone marrow changes were observed and there was no predominant syndrome for radiation deaths. Among LAF_1 mice, there

was observed an increased incidence, in certain groups, of leukemia, principally of the lymphocytic form and of lung, ovarian and breast neoplasms, the latter not being observed among the non-irradiated controls. A striking increase in the incidence and decrease in mean age at death (latent period) from leukemia in females was recorded in the 8.8 r group and a decrease in mean age at death among leukemic members irradiated at 4.4 r per day. In males, carcinogenicity was seen in the 2.2, 4.4 and 8.8 r groups. The overall incidence and latent period was similar for other irradiated groups and controls. Lymphocytic neoplasms occurred among 36% of 91 C3Hf/He female mice irradiated at 8.8 r per 8-hour day. These occurred over a period of 17 months at a mean tumor age of 13.5 months. None was said to have been observed among 160 virgin females. Interestingly, an acute exposure of C3Hf/He mice to 400 r X-rays at birth was non-leukemogenic. Neoplasms of reticular tissues were studied in C3Hf/He mice, following single exposure of whole-body X-radiation at birth (400 r) in 3-month old mice (400 r and 900 r) and in radiated mice in which radiation effects were modified by spleen-protection or by a cysteine-anoxia procedure. Evidence of induction of neoplasms of reticular tissues (lymphocytic and granulocytic leukemias and reticulum cell sarcomas) in two groups, the 400 r group, unprotected, and in the 900 r group, treated with cysteine, in both of which there was observed a decreased life span. A 3-fold increase

in frequency was noted. A striking increase in ovarian neoplasms and in medullary, as well as cortical adrenal tumors was observed in all radiated groups (Hollcroft, et al, 1957).

A study of the effects of fast neutron radiation on several biologic end points was reported recently by Neary, Munson and Mole (1957). CBA mice were irradiated nearly continuously at 3 levels, a maximum which was approximately 1 rad per day and at two lower levels approximating 1/10th and 1/30th the maximum level. There was obtained no evidence of an increase in incidence of neoplasms of reticular tissues nor of decreased latent period. The neoplasms encountered in CBA mice, among both the irradiated and control groups, were thymic and non-thymic lymphocytic neoplasms and reticulum cell neoplasms of type A and B as described by Dunn (1954). The mean age at death (survival) in the maximally irradiated group was less than the mean age at death from neoplasms of reticular tissues among all other groups and may account for the absence of evidence of induction in this group. It has been found in contrast, however, that among LAF_1 mice given fractionated X-rays, a shortened latent period for lymphocytic neoplasms was associated with maximum life-span shortening. Nevertheless, as discussed above, CBA mice, given X-radiation for limited periods, show a striking increase in such neoplasms, particularly of the lymphocytic variety. Mammary tumors and malignant neoplasms of the nasopharynx, only, were increased in frequency in this study of Neary, et al.

In earlier studies by Henshaw, et al, (1946), of the effect of fast and slow neutrons on the induction of "leukemia" in CF_1, ABC and C58 mice, it was shown that dose levels of fast neutrons from 50 to 90 n increased the incidence from 14 percent* in the controls to approximately 30 percent. A single dose of 500 r or 700 r γ rays increased the incidence to 65 percent. Details of these data were not given, however. It was stated that the incidence of "leukemia" was not altered in ABC mice and there occurred a pronounced reduction of the neoplasms in high-leukemic C58 strain mice.

Application of the estimates of the probability of radiation-induced leukemia in man is discussed by Lewis (1957), in relation to exposure from Sr^{89} and Sr^{90}. Although many experimental studies on the carcinogenicity of radioisotopes obtained from nuclear fission have been reported, little evidence is at hand to suggest that these materials are leukemogenic, aside from that observed for P^{32} (Brues, et al, 1945). P^{32}, whether given as a single dose, or in monthly divided doses, was comparable as a leukemogenic stimulus to total-body X-rays in CF_1 female mice. The evidence with P^{32}, as with X-rays, was consistent with the lack of a threshold dose below which early neoplasms were not induced. These results with P^{32} may indeed be expected to approximate the effects of whole-body radiation because of its

* The varying incidences reported for CF_1 mice among different laboratories, suggests a lack of genetic homogeneity among these mice. Indeed, no information is given concerning inbreeding of these mice.

tissue distribution. Completed data are not yet available to assess the probable leukemogenic stimulus reported for Sr^{90} in Long-Evans rats (Casarett, et al, 1958), and for Sr^{89} in mice (Watanabe, 1955).

A recent report has been given by Finkel (1958), of a long-term study of the effects of Sr^{90}, using CF_1 female mice. 70-day old CF_1 mice were given a single intravenous injection of Sr^{90}-Y^{90} from levels of 1.3 to 9330 μc/kg. The mean body burdens at 600 days were estimated to be 0.14 to 1026 μc/kg. An association between dose of Sr^{90} with osteogenic sarcomas and hemangioendotheliomas of bone marrow was observed and evidence of a threshold dose somewhat below 200 μc/kg was indicated. Reticular tissue neoplasms, arising principally in lymph nodes, thymus, spleen and bone marrow (the morphologic forms were not specified) presented a different response. The total incidence of this group of neoplasms, approximately 20 percent in treated and controls, was not influenced by dose, but there was found an association between mean age at death and dose; a dose as low as 8.9 μc/kg significantly influenced the mean age at death. It is doubtful that these data of Finkel demonstrate the existence of a threshold for reticular tissue neoplasms. Although a linear relationship exists among the values obtained for 8.9, 44 and 88 μc/kg levels, the two lowest dosage levels below 8.9 μc/kg obviously fall far off this curve. It is emphasized that the dosage level of 8.9 μc/kg represents 70 and 700 times, respectively, the

currently accepted maximum permissible level for occupational and non-occupational exposures in man. It would appear unwise, however, to conclude, as Finkel has, on the basis of this evidence that the present level of contamination with Sr^{90} from fallout, "is extremely unlikely to induce one bone tumor or one case of leukemia."

The evidence which has been discussed previously shows that profound differences in response to radiation, may result from sex, strain and species differences. Furthermore, the likelihood of an increased sensitivity in response of neonatal tissues, the influence of fractionation of exposure, the possibility of increases in susceptibility of tissues to cocarcinogens, and possible synergistic response with other leukemogens should all be considered as capable of modifying responses to radiation. It is quite probable that the choice of another strain of mouse may have led to a totally different response.

This wide range of estimates given by Finkel representing one extreme, and Lewis (1957), the other, is seen also in the published calculations of the genetic hazards of radiation, a field in which considerably more precise basic knowledge is at hand (See Russell, 1952).

Little attention has been given dose-rate as it influences the frequency or latent period of neoplasms of reticular tissues. Preliminary data are given by Mole (1958) of the effects of the same total dose of γ - radiation

of 1000 r spread out over the same overall exposure time in daily doses at different dose-rates, from 1.3 to 81 r/hour. Dose-rate appears to influence the induction of thymic leukemias in mice of the CBA and C57BL strains. This might be expected for thymic lymphocytic leukemias which, as discussed earlier, probably represent a special and unique case of response to whole-body irradiation. This experimental design should be extended to observations of other morphologic forms of reticular neoplasms and to other total dose levels, particularly at lower levels.

The several types of irradiation used by Furth and co-workers did not influence the frequency of monocytic and reticulum cell neoplasms (Type A & B neoplasms of Dunn) in RF and LAF_1 mice (Upton, et al, 1954); yet a striking increased frequency (α 50%) of these neoplasms called reticuloendotheliomas, were observed by Kaplan, et al, (1956) in C57BL thymectomized mice given fractionated, whole-body X-radiation.

In several inbred strains of mice, BALB/c, DBA/2, C57BL, CBA and BC, whole-body, X-radiation below 300 r is relatively ineffective in inducing thymic lymphosarcomas or generalized lymphocytic leukemia. At these levels of irradiation, if estrogens or carcinogenic hydrocarbons (e.g. methylcholanthrene) are given, also at levels which are non-leukemogenic, an apparent synergistic response is noted. (See Furth & Lorenz, 1954; Gardner, 1953). Both frequency and latent period are influenced, particularly for thymic lymphomas. These studies have been extended, in only one instance, to the influence of estrogens in mice given partial body X-radiation.

Here in BALB/c strain mice where neither leukemogen was effective at the dose levels employed, a precocious appearance and greater yield was obtained of thymic lymphosarcomas (Kirschbaum, et al, 1953). More recently (Kawamato, et al, 1958), a possible cocarcinogenic effect of urethan (ethyl carbamate) has been reported for X-ray-induced, as well as carcinogen- and estrogen-induced lymphocytic leukemia in several strains of mice. There has been obtained no demonstrable leukemogenic activity of this compound alone. These findings and those of Shubik, et al, (1953), relating to the influence of croton oil on the carcinogenic process in β-irradiated cutaneous tissues and the numerous reports of possible co-carcinogenic action reflected in different tissues subjected to different forms of ionizing radiation should provide a series of models for a better understanding of the carcinogenic process.

A causal relation has been established between several kinds of radiation and thymic lymphomas, generalized lymphocytic neoplasms, granulocytic neoplasms (myeloid leukemia), and reticulum cell sarcomas in mice.

It has been emphasized repeatedly by several investigators that rats, unlike mice, do not develop neoplasms of reticular tissues following exposure to radiation. Two recent reports suggest, however, that induction is possible providing the proper strains are used and the proper conditions of radiation are met.

A striking increase in plasma cell neoplasms in rats has been reported by Maisin, et al, (1956), following partial-body X-radiation. The likelihood of induction of plasma cell neoplasms (myeloma) and plasma cell leukemia should be investigated in strains of mice having the potentiality of developing these neoplasms spontaneously, the C3H strain for example, or in those strains developing these neoplasms to other insults, such as carcinogenic hydrocarbons (Rask-Nielsen & Gormsen, 1951; 1956). A second report describes the induction of 30 percent lymphocytic neoplasms in Holtzman rats given whole-body X-radiation in the lethal range but protected by a parabiotic twin (See Binhammer, et al, 1957).

3. Modifications of Irradiation Effects

The findings to be discussed of modifying the response to X-radiation and of reversing the chain of events leading to neoplasia, relate principally to lymphocytic neoplasms, thymic in origin. Lack of precise information concerning the part played by the thymus, for example in other experimental animals and in man, does not detract from these observations. The model employed provides one method of determining precise mechanisms involved in carcinogenesis.

Thymic tissue is a sine qua non for the development of spontaneous, carcinogen-induced or X-ray-induced lymphocytic neoplasms in certain strains of mice. Total removal of thymic tissue effectively reduces the incidence of the disease whether or not the thymus is the apparent site of origin of the neoplasm. It is becoming clear, however, that in some strains, following irradiation, there are exceptions, particularly as regards forms of leukemia other than thymic lymphomas (See Law, 1957).

It has been found that grafted thymic tissue, through a physiologic or biochemical mechanism, acts as a "sphere of induction" of lymphocytic neoplasms. In certain situations:

1) Parental thymic tissue (AKR), grafted into a compatible, but genetically different host, (C3H x AKR)F_1 becomes repopulated by host lymphocytes. The neoplasms which arise in the thymic graft are of host,

not graft, origin as determined by their transplantation (histocompatibility) patterns (Law, 1952).

2) In contrast, neoplasms do not arise from the compatible C3H (low-leukemic) grafted thymic tissue indicating that the "sphere of induction" is genetically expressed as an intrinsic property of thymic tissue. Similar findings have been reported for C3H as opposed to C57BL thymic tissue transferred to irradiated (C57BL x C3H)F_1 hybrids (Kaplan, et al, 19

3) In irradiation experiments, employment of the genetically susceptible C57BL thymic graft in refractory (C57BL x C3H)F_1 mice, resulting from thymectomy, has established the fact that lymphomas arise, in a majority of cases from the grafted C57BL tissue which has not received irradiation. This has been confirmed in (C57BL x A)F_1 mice; however, a certain number, particularly the neoplasms arising later in life, were found to be of host origin. Nevertheless, these data further establish the existence of an indirect mechanism in the X-ray induction of lymphocytic neoplasms of thymic origin (Law & Potter, 1956). Of interest is the finding in our series of several neoplasms which were not lymphocytic, but yet of thymic origin.

4) Grafting of C57BL thymic tissue into thymectomized (C57BL x A)F mice as late as 28 days, following the last fractionated X-ray exposure, leads to the development of lymphocytic neoplasms, some of non-exposed C57BL tissue origin, at the site of the grafted tissue (Law & Potter, 1958).

Preliminary results from this laboratory, using the millipore diffusion chamber of Algire, transplanted intraperitoneally, indicate that C57BL thymic tissue though able to grow and maintain itself for a long period of time (120 days) within the chamber does not transform to leukemic tissue when so isolated; whereas such tissue grafted under the kidney capsule does so. The possibility of tissue interaction between host and donor tissues must be considered in the induction process.

5) Susceptibility of lymphoid tissues, particularly in irradiated DBA/2 mice, is not limited to the thymus. In situ lymphocytic neoplasms arise at other sites, principally the lymph nodes. Definitive evidence is not yet at hand, to determine the precise role played by such tissues in the inductive process described above. The difference in origin of the epithelial elements of thymus and lymph nodes may account for the striking sensitivity of the thymus.

The prevention of lymphocytic neoplasms, thymic in origin, in sublethally irradiated mice by isologous (Kaplan, et al, 1953) except with marrow from high-leukemic donors (Lorenz, Law and Congdon, 1954), but not by homologous bone marrow (Kaplan, et al, 1958) has been reported. The most plausible explanation for these results, as well as in the shielding experiments (Kaplan & Brown, 1951; Lorenz, et al, 1953) is that total repopulation by the isologous non-irradiated marrow has occurred. The lack of

protection by isologous embryonic hematopoietic tissues in C57BL mice and the evidence for the persistence of homologous bone marrow cells (which do not prevent induction) in the marrow and lymphoid organs would argue against this interpretation (See Law, 1959).

A study is in progress in this laboratory of lethally irradiated mice of several inbred strains and hybrid combinations which receive homologous intact bone marrow cells. In those mice which survived the "secondary phase" of the irradiation syndrome, numerous neoplasms of reticular tissue have been observed (Uphoff & Law, 1958). As determined by their histocompatibility and antigenic patterns, the majority of these neoplasms were of the donor bone-marrow type, representing radiation chimeras. Thymic lymphomas were not observed in any of the lethally irradiated mice receiving isologous marrow. Interestingly, those lethally irradiated mice receiving bone marrow from the high-leukemic AKR and C58 strains presented thymic lymphomas.

The phenomenon of inhibition of lymphocytic leukemia by post-irradiation bone marrow is not restricted to C57BL mice. Our previous results showed the inhibitory influence of C3H marrow in (C3H x AKR)F_1 irradiated mice. Recently these observations have been extended to DBA/2 mice in which many of the induced neoplasms are non-thymic in origin. Transfer of isologous bone marrow to DBA/2 mice receiving four weekly

courses of 125 r X-radiation resulted in a decrease of the incidence of lymphocytic neoplasms from 52 to 16 percent. The 16 percent lymphocytic neoplasms appeared early and were non-thymic in origin. The possibility of a difference in influence of intact isologous bone marrow cells on the induction of reticular neoplasms of lymph node versus thymic origin is thus indicated, (Law, Potter & Uphoff, 1959).

The reversibility of the X-ray induction of thymic lymphomas has been shown for intact isologous bone marrow cells. It is known that this reversal must be early; that is, bone marrow must be introduced in the matter of a day or two after irradiation (Kaplan, et al, 1955). In contrast, cortisone administered as late as 100 days after irradiation, strikingly suppresses the formation of thymic lymphomas (Nagareda & Kaplan, 1958). The role of shielding of hematopoietic tissues during X-radiation or of introduction, intravenously, of intact bone marrow cells, is not known precisely for other neoplasms of reticular tissues, except perhaps for reticulum cell sarcomas in C57BL mice following whole-body irradiation (See Kaplan, et al, 1956a). Nor has this influence been demonstrated for other than induction by X-radiation.

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4. Neoplasms of Bone

A most comprehensive experimental study of radiation-induced bone neoplasms has been carried on by Brues and co-workers, using a number of bone-seeking radioisotopes of both α- and β-emitting character. Studies of comparative carcinogenicities, dose-response relationships, species differences and pathogenesis of neoplasms have been reported (Brues, 1949, 1951; Brues, et al. 1947; Finkel, 1953, 1958; Lisco, et al. 1947).

Bone neoplasms have been induced following the introduction of radium-226, strontium-89, strontium-90, yttrium-91, cerium-144/-praseodymium-144, plutonium-239, calcium-45, polonium-210, thorium-228, uranium-232 and uranium-233. The carcinogenic pattern for bone-tumor response, particularly following introduction of Sr^{89} and Sr^{90} in CF_1 mice appears to be as follows:

1) There is a sharp latent period of approximately 180 days, following which bone neoplasms, mostly osteogenic sarcomas, develop rapidly, and in many cases as multiple neoplasms. The latent period cannot be shortened by increasing the dose.

2) The probability of bone tumor formation is without limit in time, once the latent period is reached. This is true for single-dose, as well as multiple-dose regimens.

3) There has not been obtained a simple linear relation between dose and response.

4) There is stated to be a threshold below which osteogenic sarcomas are not induced or are not detectable. For Sr^{90} this level lies somewhere below 200 μc/kg of injected dose and for Sr^{89}, doses between 0.001 and 0.005 μc/gram did not appear to alter the incidence of neoplasms.

5) ABC mice were less responsive to the carcinogen than CF_1 mice and there appeared some evidence of the existence of recovery factors in that repeated doses at lower levels were more efficient than repeated doses at higher levels, compared with the same total dose given in a single injection.

Attempts have been made to determine mean dose levels in bone of radioisotopes by Glücksmann, et al. (1957), and by Lamerton (1957). It is clear that the mean levels estimated, for example at one year, represent levels of probably thousands of rads, except possibly for plutonium, which has a far greater capacity for bone tumor induction than other radioisotopes. Plutonium (Pu^{239}) in contrast to other isotopes produced mostly neoplasms in the spine (62% compared, for example, with Sr^{89} 22%). An explanation for this difference in localization is related to the greater affinity of Pu^{239} for collagenous tissue than for bone proper. Hamilton (1948) has reported deposition at or near bone surfaces so that dosage rates may be expected to be high at sites of bone growth.

The relative biologic effectiveness (RBE) of radium and certain other α-emitting isotopes, have been studied by Finkel (1953), in CF_1 mice. The

deficiencies of such comparisons have been recognized since differences in biological half-life, their distribution and retention in tissues and differences in susceptibilities of tissues are unknown factors. At near tolerance levels, an estimate of the RBE was found to be 7(Pu^{239}): 3(Po^{210}): 2(Ur^{232}): 1(Ur^{233}): 1(Ra^{226}).

Of particular interest is the appearance of other neoplasms in experimental animals following localization in tissues of certain of the isotopes discussed above. Malignant fibrosarcomas have been observed following introduction under the skin of 0.1 μgm of plutonium or 1 μc of YPO_4. The occurrence of carcinoma of the colon has been reported in rats fed Y^{91} by stomach intubation with as little as 1.0 to 6.0 millicurries (Lisco, et al. 1947a, 1947b). Bronchogenic carcinomas were found in rats following tracheal intubation of radioactive cerium oxide (Lisco & Finkel, 1949). These neoplasms were found in areas of severe radiation damage to the lung.

The carcinogenic response of certain radioisotopes has been studied in larger species including the cat and dog. The data from such studies are extremely limited, however, due to the small numbers used and the genetic heterogeneity of the animals.

The extensive data presented by Brues and co-workers concerning dose-response relationships (osteogenic sarcomata induction) are not

inconsistent with a substantial threshold dose. Extensions of the dose-response curve to low levels of plutonium would be interesting. It is argued (See Mole, 1958 and Lamerton, 1957) that serious limitations imposed by the short life of the mouse confront the investigator in such studies since it is possible at low dosage levels of radiation, that the latent period for tumor induction may indeed exceed the life span of the mouse. If this is so, it would be wise to study the response of other strains of mice, particularly those with potentialities of developing osteogenic sarcomata spontaneously, such as Pybus-Miller (PM) mice (Pybus & Miller, 1938), or of strains having a potentiality to develop benign osteomas, such as the AKR strain (Furth, 1946). In most of the work with bone-seeking radioisotopes, adult mice have been used. The recent work of Owen, et al. (1957), indicates a possible greater susceptibility to these isotopes in younger animals and attempts at modification of the response, with age, should be pursued.

In contrast to the response of CF_1 and ABC mice, rats at least of one strain (?) , the so-called Wistar White rat, are strikingly more sensitive to the radioisotope P^{32} In the most recent report (Koletsky, 1954) low, single doses of P^{32} in the range of 1.0 to 3.0 μc/gram produced osteogenic sarcomas in 32 percent of the animals. This is 1/200th of the injected dose in μc/gram of Sr^{90} which induced osteogenic sarcomas in CF_1 female mice. Levels of 0.25 to 0.5 μc/gram were said to be ineffective. The LD_{50} dose

was in the neighborhood of 4.5 μc/gram body weight. In addition to osteogenic sarcomas, there appeared squamous-cell carcinomas, probably originating in the nasopharynx. The sensitivity of this strain of Wistar rat to whole-body irradiation was mentioned previously and the difference in the response of CF_1 mice to P^{32} may reflect species differences or more likely, chance selection of sensitive rats and resistant mice. The induction of osteogenic sarcomas in mice, following P^{32}, appears not to have been reported.

In the experiments of Koletsky, the initial radiation dose to bone has been calculated as 100 r/day (Glücksmann, et al. 1957), and according to Furth and Tullis (1956), one-half of this strongly carcinogenic dose in rats approaches the average therapeutic dose in man.

In mice, P^{32} has been reported to induce osteogenic sarcomas in ST/Eh mice at levels of 0.2 μc/gram (single dose). (Moller, 1954). An extremely low induction rate was observed, 3 of 38 mice, and unless a complete skeletal study was accomplished in both irradiated and control Street mice, these results may merely reflect sampling differences.

Although studies of the distribution in bone and of histogenesis of bone neoplasms following administration of radioisotopes have been reported, the radiation doses have been extremely high, in the order of thousands of rads or higher. Gross tissue damage reflected in aseptic

necrosis of bone and fibrosis of bone marrow, disorganization of hemopoietic structures, resorbtion of bone by osteoclasts and atypical bone formation may represent incidental responses and have no relationship to the actual transformation of a normal to a neoplastic cell. The question has not been answered whether such neoplasms always arise from abnormal bone or at sites of maximal radiation damage.

5. Pulmonary Tumors

It is unfortunate that the study of radiation carcinogenesis of pulmonary tissues in experimental animals has been relatively neglected. In mice, the role of genetic factors and of site of action of genes has been well delineated. (Heston, 1940; Heston & Deringer, 1947, 1949). Furthermore, precise quantitative measures of response, particularly to carcinogenic hydrocarbons, have been established. In the mouse, it is believed that pulmonary growths arise from alveolar lining cells (Grady & Stewart, 1940), while the common pulmonary tumors in man, in contrast, appear to have their origin from bronchial epithelium. Some inbred strains of mice, particularly the A strain, have a frequency of pulmonary adenomas of 90-100% and these occur late in life. These tumors persist for long periods of time without greatly affecting the health of the animal. The tumors are not encapsulated, are said to metastasize on occasion (Wells, et al. 1941), and grow progressively upon transplantation into mice of the appropriate genotype. Under certain experimental conditions it is possible to reproduce in mice neoplasms of the histologic types observed in man (Smith, 1952).

Furth & Furth (1936) presented evidence that massive doses of whole-body X-radiation, given singly or in multiple doses, influenced the expression of pulmonary adenomas in 3 strains of mice, all having the genetic potentialities to develop these tumors spontaneously. The increase in frequency

was not of statistical significance but an earlier appearance of the neoplasms was seen in the X-rayed group. The neoplasms were morphologically similar in both the irradiated and control groups.

Dohnert (1938) and Hereck (1939) attempted studies of induction of lung tumors in mice exposed in the Schneeberg mines. The experimental design in each study was not sound. The report of Rajewski, et al. (1943) of induction of pulmonary adenomas and adenocarcinomas arising from bronchial epithelium in mice given exposures to various dose levels of radon, [18] not convincing nor is there evidence of corroboration of these findings.

Lorenz and co-workers (1946) reported that A strain mice irradiated from a radium sulphate source at 8.8 r per 8-hour day, for 287 days, had a significantly higher incidence of pulmonary adenomas than control litter mates. These experiments were initiated as a result of suggestive findings in LAF_1 mice, given chronic gamma irradiation (Lorenz, et al. 1954). The total dose of gamma rays was 2500 r. At 10.5 to 11.5 months of age, the tumor incidence in the experimental group was found to be 76.7% and in controls, 47.3%. There was no striking increase of multiple neoplasms of the X-rayed group over their litter mate controls. For comparison, carcinogenic hydrocarbons induce 100% pulmonary adenomas in A strain mice as early as 6 weeks with the appearance of multiple neoplasms in the

lung. Although it was stated that the evidence points to a direct effect of irradiation of lung tissue leading to neoplasia (see Lorenz, 1950), no data were given to support such a claim. Apparently LAF_1 mice with thorax shielded, did not show an increase in lung tumors following acute, fractionated X-radiation (75 r x 10 days), whereas those mice receiving total-body X-radiation showed an increased frequency. It is of interest to note, however, that in a later experiment employing the same strain of mice (A strain), 900 r whole-body X-radiation to spleen-shielded animals did not influence the expression of lung adenomas (Heston, et al. 1953).

Radiation from a nuclear detonation (Furth, et al. 1954) and thermal neutrons (Upton, et al. 1954), were ineffective in LAF_1 mice in significantly altering the frequency or latent period of pulmonary tumors despite the carcinogenic effectiveness of these radiations for other tissues. Fast neutron irradiation, as reported by Neary, et al, (1957), proved also to be non-carcinogenic for lung tissues of CBA mice. (See earlier discussion for dosages employed). This proved to be the experience also of Henshaw & co-workers using CF_1 mice (Henshaw, et al. 1947).

Of interest is the finding of Lisco & Finkel (1949) of the induction of bronchiogenic carcinomas in rats by inhalation of Ce^{144}. Severe lung damage was encountered with the dose levels used. There appear to be no similar studies reported on the response of mice to radioactive isotopes.

The evidence to date in mice, indicates that a high radiation dose given to genetically susceptible animals results in all probability, only to an earlier appearance of these tumors, alveolar adenomas, which ordinarily will occur in the absence of radiation. The effects observed, however, following administration of carcinogenic hydrocarbons or nitrogen mustard congenera are striking in comparison, but nevertheless, appear to act similarly by accelerating a process of transformation which is to occur under normal circumstances. For a rough comparison, the total energy absorbed in the above 2500 r experiment of Lorenz and colleagues was determined to be in the neighborhood of 2×10^5 ergs/gram of lung tissue while approximately 1×10^3 ergs/gram was found among the miners of Schneeberg & Joachimstal. As mentioned earlier, the chronic gamma-ray exposure studies of Lorenz and co-workers, employing LAF_1 mice, gave suggestive evidence of an influence on expression of pulmonary adenomas of levels as low as 1.1 r/8 hour day. These, apparently, are the only data relating to dose-response. In this connection, it is of interest to point out the dose-response studies of Heston & Schneiderman (1953) in relation to pulmonary tumor induction in A strain mice, using the carcinogenic hydrocarbon 1:2:5:6-dibenzanthracine. These authors found the tumor-dose relationship to be linear, subject to certain defined limitations, and there appeared to be no indication of a threshold dose.

6. Neoplasms of Endocrine and Endocrine-Controlled Tissues

Previously, evidence was presented of an indirect effect of whole-body irradiation on the induction of lymphocytic neoplasms of thymic origin. In contrast, generalized lymphocytic neoplasms, myeloid leukemia and reticulum cell sarcomas in the mouse appear not to fall into this category, although information is scanty concerning these latter neoplasms. An abscopal effect (Mole 1953), in the sense that irradiation produces changes in tissues at a distance from the volume irradiated, may be better understood in a study of neoplasms arising from endocrine imbalances, apparently as a result of radiation.

a. Ovarian Neoplasms

The early work of Furth and colleagues (Furth & Furth, 1936; Furth & Boon, 1943; Furth & Butterworth, 1936) pointed out the striking sensitivity of ovarian tissues to whole-body irradiation. X-radiation in single or fractionated doses or chronic γ-radiation has been carcinogenic in most strains and in F_1 hybrids tested, although the number of strains have in fact been limited and the role of genetic susceptibility or of genotype, as it influences morphologic types of neoplasm, has received little attention. A single dose of fast neutrons, as low as 26 n, was said to increase the frequency of ovarian tumors in CF1 mice (Henshaw, et al., 1947).

In CBA mice, on the other hand, given fast neutron exposures at 3 different dose rates, with a maximum exposure of 1 rad/day, no evidence of an increased frequency of ovarian neoplasms was noted (Neary, et al., 1957).

Ovarian neoplasms have been observed in LAF_1 mice following total body X-radiation with doses as low as 50 r (Deringer, et al., 1955), but not 25 r, and also following chronic exposure to 0.11 r per day of γ-rays to a total dose of 110 r (Lorenz, et al., 1947). In the former experiments, shielding of the ovaries by exteriorization, during X-radiation to levels as high as 900 r prevented the appearance of neoplasms. X-radiation of ovaries alone, with the body shielded, yielded 28% bilateral ovarian tumors

with a single dose of 50 r. The role of circulating estrogen in these X-ray-induced neoplasms is shown by the experiments of Lick, et al. (1949), in which irradiation of only 1 of 2 ovaries in BALB/c mice did not lead to tumors and by Gardner (1950) who showed prevention of ovarian tumors in estradiol-treated mice given 280-380 r whole-body X-radiation. Further, intramuscular implants of irradiated ovaries into ovariectomized females showed a high percentage of tumors, while similar implants into intact hosts remained non-tumorous (Kaplan, 1950). Some direct evidence that deficiency of circulating estrogens leads to increased pituitary gonadotrophins has been published (Miller & Pfeiffer, 1950; Muhlbock, 1953).

Although it is generally accepted that the total accumulated dose of radiation is the deciding factor in the induction of ovarian neoplasms there are few data to suggest the influence of dose-rate or of fractionation and protraction. If an anovular stage must be reached prior to the development of tumors one might expect some influence of dose-rate or fractionation.

Recently, a comparison has been made of tumorigenesis in the ovaries of X-rayed mice, of ovaries implanted into the spleen of castrate hosts and in the sterile ovaries of C57BL-W^VW^V mice (Russell & Fekete, 1958). In the latter situation, the W^V gene, a pleiotropic gene, influences the growth and development of ovarian follicles through the primordial germ cells. Very few ovarian follicles are present at any time and they cease to grow and develop after 2 months; little circulating estrogen is present in these mice.

Nearly all C57BL-W^VW^V females present bilateral tubular adenomas by 7 months of age. It is of interest to note that the C57BL strain, following total body irradiation, shows a predominance of tubular adenomas and luteomas. This is in contrast to the findings of others employing different strains in which there is a preponderance of the granulosa-cell type of tumor.

The chain of events appears to be the same in all 3 situations, however: downgrowth of germinal epithelium, absence or a low level of circulating estrogens due to the lack of developing ovarian follicles or in the case of ectopic ovaries placed in the hepatic-portal system, the estrogen is metabolized in the liver, and finally ovarian tumors.

It is likely in the case of the W^V gene effect that the extremely early absence of germ cells and estrogen deprivation determine the morphologic type of neoplasm encountered. If so, X-radiation of susceptible strains at different ages may throw some light on the pathogenesis of the different neoplasms.

The role that the definitive germ cells play in the early appearance of luteomas in C57BL-W^VW^V mice suggests that the direct effect of irradiation, which appears necessary to induce ovarian tumors, may merely eliminate the irreplaceable supply of definitive germ cells.

Although a significant role is, in all probability, played by gonadotrophins in the induction of ovarian neoplasms in the mouse, and in other species direct irradiation of the ovaries is also necessary in the radiation-induced tumors which have not been observed however in other species.

b. Thyroid Neoplasms

Thyroid carcinomas, some with metastases, have been induced in rats following administration of I^{131} (Goldberg & Chaikoff, 1952). If I^{131} or X-rays are given, along with a goitrogen, these tumors are induced more readily and a lower dose of irradiation is effective (Doniach, 1958). Thyroid response is similar to the response of ovarian tissues in that induction of tumors occurs following the use of several techniques apparently through creation of hormonal imbalances. Diets deficient in iodine, goitrogen administration of a carcinogen, 2-acetamidofluorene with partial thyroidectomy, use of carcinogens alone, etc., lead to tumors of the rat thyroid in the observed sequence of events of rapid goitrogenesis, adenoma formation and finally carcinoma formation.

Benign and malignant tumors observed by Goldberg and Chaikoff appeared 1.5 to 2 years after a single injection of 400 μc I^{131}. It is estimated that the mean thyroid irradiation was in the neighborhood of 150,000 rads or more (Feller, et al. 1949). Several investigators later found this dosage level actually inhibited thyroid tumor formation reflecting probably strain differences in the rats used, and that the optimal range was much lower and probably in the range of 10 to 30 μc I^{131}. Interestingly, as shown by Doniach, similar quantitative carcinogenic effects were obtained with 30 μc of I^{131} given over a few days, and 100 rads of X-rays given in a few minutes. The

addition, in each case, of a growth proliferative stimulus, methylthiouracil, led to carcinoma formation. Thus, it is postulated that the initial radiation effect "initiates" neoplastic cells which are later "promoted" to tumor formation by the goitrogen-induced TSH (thyroid stimulating hormone) stimulation. Recent work by Dr. Seymour Wollman (personal communication) of this institute has shown, however, that thiouracil given alone, without resort to carcinogen or radiation, provides sufficient stimulus to carcinoma formation. Profound strain differences were found among three inbred strains of rats used in these studies: A x C, Marshall (M-520) and Fischer. The latter inbred strain was especially sensitive to the goitrogen. Similarly, I^{131} administered without goitrogen resulted chiefly in adenoma formation but also a striking increase in carcinomas.

There exist some data concerning dose-response relationships of tumors of the thyroid following administration of I^{131} (Lindsay, Potter and Chaikoff, 1957). These data show, as stated previously, that at high levels of radiation, above 25 μc I^{131} there is a decrease in tumor induction with increasing dose so that with 400 μc of I^{131} no tumor induction was observed, presumably because of irreversible damage to the thyroid elements. Unfortunately, the range below 10 μc was not investigated in these studies and the question, asked previously, whether damage to the tissue is a necessary prerequisite to tumor formation is not answered. Further, it

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should be determined whether TSH is a necessary requisite to carcinoma induction of the thyroid. The carcinogenic effect of small doses of X-radiation in infants radiated for thymic enlargement and in those receiving radiation for cutaneous lesions, would suggest some other "promoting" agency if indeed this is necessary. An explanation is given that the normal growth of thyroid tissue from infancy to early adult life (a stage at which total dependence upon the pituitary is not proved) simulates the results of long-continued goitrogen administration and of itself is the "promoting" factor (See Doniach, 1958); yet, it was pointed out previously that a substantial increased sensitivity is suspected in the response to radiation of those tissues of the infant or embryo, which are not "dependent" on endocrine secretions.

c. Pituitary Tumors

Recent work on the role of radiation, particularly of X-radiation and I^{131}, as it relates to the induction of neoplasms of the pituitary gland in mice, has been reviewed by Furth (1955, 1957). As has been pointed out, these neoplasms offer unique research possibilities, chiefly because, a) they may be characterized biologically into hormone-secreting types, and b) they offer the possibility of studying the mechanism of transformation of the functionally differing cell types from dependent to fully autonomous neoplasms.

Pituitary tumors in mice and rats may be produced by several mechanisms including ablation of the thyroid gland by radiothyroidectomy, castration, increasing the level of estrogenic hormone or by whole-body X-radiation. There appear to be distinct morphologic and functional differences among the tumors arising as a result of different treatments.

Gorbman, in 1949, induced pituitary tumors in mice following the introduction of 40-50 μc per gram of body weight of I^{131}. Later, Furth & Burnet (1951) induced pituitary tumors in C57BL strain mice after injection of 200-400 μc of I^{131}. All mouse strains tested to date have proved sensitive. Complete destruction of thyroid tissue appears necessary; transplantation of functional thyroid tissue or administration of thyroid hormone will inhibit tumorgenesis in athyroid hosts.

Similar tumors may be induced by surgical thyroidectomy (Dent, et al. 1956) or through blocking thyroid hormonal synthesis with propylthiouracil (Moore, et al. 1953). These tumors, as well as those resulting from radiothyroidectomy have been found to be functionally of the thyrotropic type. All have a common denominator: a marked and sustained depression of the level of thyroid hormone, which in all probability, culminates in tumors resulting from a sustained stimulation of thyrotrops. Furth (1957) has stressed that failure of a feed-back mechanism is decisive, whereas, Bielschowski (1955) has emphasized that the attempt to compensate for thyroxid deficiency is important in the pathogenesis of these tumors. The thyrotropic tumors have been found by Furth and colleagues to be dependent tumors, transplantable only to mice whose thyroid function is markedly depressed. In time, however, through many transfers in isologous hosts, they may become autonomous.

In contrast to results obtained with I^{131}, Furth, (1955) observed the induction in mice of quite different neoplasms, functionally and morphologically, following total body irradiation from an atomic detonation (Furth, et al 1954). These were classed as mammotropic and adrenotropic tumors. The morphology and functional character of these new types have been described by Furth. In contrast to thyrotropic tumors induced in I^{131}-treated mice, these neoplasms were autonomous, growing progressively in all normal mice

of the strain of origin. It has been suggested that in the induction of thyrotropic pituitary tumors in mice, total-body irradiation (or non-specific stress) is effective if given following I^{131} (Gorbman, 1952; 1956). This is doubted by Furth (1957) who feels that X-radiation does not play a role in the induction of the thyrotropic types. There was some evidence from these studies of a correlation between irradiation dose and frequency of tumors if corrections were made for reduced longevity in the higher dose ranges. The optimal dose range appeared to be between 400 and 600 r; however, 192 r, the lowest dose used, was quite effective.

In more recent studies in progress, 500 r whole-body X-radiation of strain L, strain A and LAF_1 hybrids resulted in mammotropic and adrenotropic pituitary neoplasms. This same dose of irradiation of the head region alone, but not abdomen alone, was as effective as total-body irradiation. Adrenalectomy did not enhance the tumor induction by X-rays.

Apparently all, or most strains of mice respond to I^{131} and the result is thyrotropic pituitary tumors, the induction of which is not influenced by superimposed X-radiation. The induction of the autonomous mammotropic and adrenotropic neoplasms has been recorded chiefly in LAF_1 mice and it is to be noted that some of these neoplasms occur spontaneously in one of the parental strains, the L strain (See Furth, 1955). Although chromophobic adenomas associated with mammotropes may develop in some strains of

mice (Gardner & Strong, 1940) or rats if high levels of estrogen prevail, these tumors are "dependent" or "conditional" and are transplantable only to estrogen-treated hosts. It would appear therefore, that certain pituitary neoplasms, induced by whole-body X-radiation, are indeed quite distinct in their functional and biologic characteristics from morphologically similar neoplasms induced by other methods.

d. Mammary Tumors

There are reports in the literature concerning the appearance of mammary tumors, following ionizing radiation, in several species of animals. The most complete and pertinent information has been obtained from mice and rats. The most recent surveys of these findings are found in the reports of Lorenz, et al. (1951) and Furth & Lorenz (1954).

The appearance of mammary neoplasms, principally adenocarcinomas, in the mouse has been shown to be influenced by genetic factors, hormonal factors, particularly estrogens, and a virus-like agent which is quite specific for mammary gland tissue. Normally, there is a striking interdependence among these factors and the expression of a neoplasm depends greatly upon the genotype of the mice being used. Consequently, attempts to assess the role of irradiation in this complicated situation is indeed difficult. Slight metabolic disturbances may also drastically influence the frequency of such neoplasms. Calorie restriction (Tannenbaum, 1940), or feeding a low cystine diet (White & White, 1944) diminishes the frequency of these neoplasms. Although caloric restriction has been considered to result in hormone deficiency, this appears not to be the case, since inhibition results from a degree of caloric restriction which does not seriously affect mating and fertility.

The chronic γ-radiation studies of Lorenz and co-workers (1951), reveal some interesting findings. The frequency of breast neoplasms in

C3H/He mice, a subline carrying the breast-tumor virus, was not influenced following exposures to 4.4 r, 1.1 r, 0.11 r and 0.44 r daily for 10 to 15 months (Lorenz & Heston, 1954); whereas, in C3Hf/He mice, a subline freed of virus, and in LAF_1 hybrids, there occurred an increased frequency following chronic radiation. Among the latter, the typical morphologic form, adenocarcinoma, was increased and, in addition, 25% sarcomas were found where nonewere observed among controls. Increasing daily dose rates did not significantly influence the incidence or age at appearance of the carcinomas, and 2.2 r daily was found to be optimal in contrast to the response of ovarian and pulmonary neoplasms among this same irradiated group. The appearance of the unusual sarcomas of the breast was observed also in C3Hf/He mice receiving 8.8 r daily if these mice were maintained as virgins. These histologic dissimilarities may reflect etiologic differences and demand further exploitation.

Earlier, Furth & Furth (1936) and Furth & Butterworth (1936) found that the overall incidence of mammary tumors in acutely irradiated mice actually decreased, although there was a suggestion that the frequency was high among those irradiated mice later developing granulosa-cell tumors of the ovary.

The findings obtained in chronically irradiated C3Hf/He mice (freed of virus) require further amplification. Non-irradiated C3Hf/He mice maintained as virgins have a mammary carcinoma incidence of 4 percent;

if these are bred intensely, the frequency is 38 percent in the particular subline used in this study. Among the chronically irradiated virgin C3H mice, free of virus, mammary sarcomas predominated, the incidence being 30 percent at 17.1 months. Twenty percent of these females had mammary carcinomas. The mean accumulated dose of γ-radiation was 3570 r and there occurred a 36 percent reduction in mean survival time. Previous studies had shown female mice of this subline to be sterile after 500 r total dose, independent of the dose-rate (Lorenz & Heston, 1954). It is of interest to note that irradiation of the same subline of C3H mice, those carrying the mammary virus (Lorenz & Heston, 1954) with daily doses as high as 4.4 r per day revealed no change in the frequency of these neoplasms.

Although Furth & Lorenz (1954) suggest an association between the presence of hormone-secreting ovarian tumors, those of the granulosa-cell type, and the increased frequency of neoplasms of the breast, particularly sarcomas, this relationship is not clear. In the group of LAF_1 mice receiving 0.11 r daily at the lowest dosage level used, 25 percent sarcomas were observed after an accumulated dose of only 80 r to 150 r. At the highest dosage level, 8.8 r per day, no sarcomas, but granulosa-cell tumors were observed. Further, in chronically irradiated C3Hf/He mice, the mean age at appearance of breast neoplasms is approximately 16.5 months, while in intensely bred females, the mean age is 20.3 months.

Hollcroft, et al. (1957), observed little or no change in frequency of mammary carcinomas, but a striking increase of ovarian neoplasms in the same subline of C3Hf mice, free of the mammary virus, acutely irradiated with X-rays. Excessive estrogen stimulation in mice bearing granulosa-cell ovarian neoplasms would therefore not appear to account for the early appearance of the neoplasms. It is known too that DBA/2 mice, treated with methylcholanthrene, develop mammary neoplasms morphologically similar to those observed in irradiated mice, yet ovarian neoplasms are not associated with these induced neoplasms (Andervont & Dunn, 1950). Biologic tests for the presence of the mammary tumor virus in neoplasms arising in the irradiated C3Hf mice, discussed above, failed to reveal the presence of the mammary tumor virus.

Furth, et al. (1954) noted an increased frequency of mammary neoplasm in LAF_1 mice receiving acute ionizing radiation from an experimental nuclear detonation. It was stated that sarcomas, in contrast to adenomas and adenocarcinomas, appeared later in life. No specific histologic classifications were given nor information of the relationship of hyperestrogenization (coexistence of various tumors) to morphologic type of mammary neoplasm. Doses from 192 r to 687 r appeared effective as carcinogens.

Chronic exposure of CBA strain to fast neutrons, as reported by Neary, et al. (1957), resulted in an increased frequency and early occurrence of mammary tumors at the maximal radiation dose of 6.4 rads per week.

No sarcomas, but a few mixed carcinoma-sarcomas, along with typical adenocarcinomas were observed. Interestingly, other neoplasms, except tumors of the nasopharynx, which normally occur earlier in this strain, were not increased in frequency by the fast neutron irradiation.

The limited, though interesting, reports concerning the relationship of radiation to induction of neoplasms of mammary tissue in mice, indicate striking strain differences in response, particularly to chronic irradiation. The dissimilarity in response, depending upon the presence or absence of the mammary tumor virus in mice of the same genotype (C3Hf/He) requires further exploration, as does the genesis of the rare sarcomas appearing after relatively low total doses of radiation.

Generally, the common tumor of the mammary gland of the rat is the fibroadenoma and estrogens are known to play a role in the induction of these neoplasms. Unfortunately, non-homogeneous rats have been used in radiation studies, and difficulties in interpreting the effects of radiation may be expected. The varying incidences of spontaneous neoplasms occurring in Sprague-Dawley rats, for example, housed in different laboratories should be noted.

An increased frequency of mammary tumors has been reported following exposure to chronic whole-body X-radiation (Metcalf & Inda, 1951), to neutrons from the cyclotron (Leitch, 1945), to direct β-radiation (Passonneau, et al. 1953), to supralethal whole-body X-radiation in protected

rats (Finerty, et al. 1953; Brecher, et al. 1953; Binhammer, et al. 1957), and in Sprague-Dawley rats sublethally irradiated by X-rays (Shellabarger, et al. 1957), and by administered Astatine^{211}, an X-ray emitter.

Among 48 Sprague-Dawley rats receiving either 200 r or 400 r of γ -radiation or 400 r of X-radiation, 76 mammary tumors were observed within an 11-month period, the earliest appearing 85 days after whole-body exposure, (Shellabarger, et al. 1957). More than half were adenofibromas or fibroadenomas and many animals had multiple tumors. Similarly, Sprague-Dawley female rats given At^{211}, at a level of 0.5 μc per gram of body weight, presented mammary tumors in 73% of the animals in a 12-month observation period. (Durbin, et al. 1958). Chromophobe adenomas of the anterior pituitary and adenomas of the adrenal cortex were observed in the At^{211}-treated rats and were apparently not found in those receiving γ - and X-radiation. Although Davis, et al. reported a frequency of 57 percent spontaneous mammary tumors in Sprague-Dawley rats, a strikingly higher number of malignant neoplasms were stated to have appeared among the At^{211} treated rats. Mammary tumor induction by whole-body X-radiation in Sprague-Dawley rats was reported by Cronkite, et al. (1957) to be intimately associated with endocrine disturbances. Few rats, ovariectomized before irradiation, developed mammary tumors, while shielding the ovaries during radiation, or implantation of unexposed ovaries resulted in frequencies similar to that encountered in the intact, irradiated animal.

In contrast to the above results, Koletsky and Gustafson (1955) reported many benign and malignant tumors at various sites, except the mammary gland, in those lethally X-radiated, Wistar white rats that survived the radiation syndrome. Similarly, mammary tumors were uncommon in an inbred strain of rats receiving whole-body X-radiation in the lethal range, but protected by various methods (Maisin, et al. 1955).

7. Other Neoplasms

Gastric adenocarcinoma is rare in animals. Some attempts have been made to induce such neoplasms with carcinogenic agents and with ionizing radiations (Moore, et al. 1953), without much success. A recent, interesting finding is the appearance of 5 percent adenocarcinomas and squamous cell carcinomas in LAF_1 mice receiving an acute exposure to fast neutrons in the sublethal range of 270-520 rep. (Nowell, et al. 1958). In one-third of these mice hyperplastic mucosal lesions were observed with progressive invasion of the submucosa. In addition, mucoid carcinoma distant to the stomach in the cecum and small intestine occurred with a greater frequency. In contrast, severe occlusive arteriosclerosis was associated with a lower frequency of mucosal lesions in LAF_1 mice receiving whole-body X-rays (800 r) and protected with isologous splenic cells. Only a single adenocarcinoma of the glandular stomach was observed. However, in this laboratory, the most common neoplasm found in mice of various genotypes given lethal whole-body X-radiation (700 r to 900 r) and protected with either isologous or homologous bone marrow cells, was that involving some portion of the gastrointestinal tract. The pathologic diagnosis of these neoplasms is not yet firmly established. (Uphoff, personal communication).

In Holtzman rats X-radiation in the supralethal range (900-1100 r) resulted in a group of neoplasms predominantly of intestinal origin (Brecher,

et al. 1953), whereas lower levels, 700 r, resulted in neoplasms of non-intestinal origin. The hypothesis has been advanced that intestinal injury from radiation is a prerequisite to neoplasia (Binnhammer, et al. 1957). The induction of carcinoma of the colon in rats following injection of radioactive yttrium was mentioned earlier. These neoplasms were associated with polypoid hyperplasia and ulcerative colitis (Lisco, et al. 1947).

Although adrenal cortical tumors are fairly common in some strains of mice and appear after castration in others or following irradiation (Upton, et al. 1954), few spontaneous tumors of the adrenal medulla have been reported. In two strains, however, their appearance, following irradiation with whole-body X-rays, seems established. Pheochromocytomas in the BRS strain (Smith, et al. 1949) and adrenomedullary neoplasms in C3Hf mice receiving 400-900 r have been reported. (Hollcroft, et al. 1957).

Another neoplasm, adenocarcinoma of the Harderian gland, which is extremely rare in mice, was reported by Furth and colleagues (1954) in greatest frequency in both sexes of LAF_1 mice receiving radiation from high-energy γ-rays and fast and slow neutrons from an experimental nuclear detonation. These appeared also with a lesser frequency in RF mice (Upton, et al. 1954), receiving thermal neutron irradiation. The induction frequency was stated not to be dose-dependent in the LAF_1 series and made their appearance in the lower dose ranges. The probability of finding

neoplasms among mice receiving high total doses was not great because of the long latent period of 15 to 17 months.

Various neoplasms of many different sites, including liver, uterus, pylorus, cecum, kidney, etc., appear in experimental animals following radiation. Usually these are not numerous and the relationship to radiation has not been clearly established.

IV. Conclusions and Summary

In this attempt to review some of the pertinent information available relating to carcinogenesis of ionizing radiation, it is clear that neoplasms result from whole-body irradiation given in short exposures or protracted over long periods of time. Most of the literature concerning the use of localized external radiation has involved extremely high doses. The carcinogenic effects of certain types of non-uniform irradiation has been investigated experimentally with a large number of radioactive materials but particularly with the bone-seeking radioactive isotopes where the dose is confined chiefly to the bone and immediately adjoining tissues. Differential effects have been noted and particularly the work with P^{32}, Au^{198} and thorotrast, the latter two non-bone-seeking isotopes, needs to be extended.

A large variety of morphologic forms of tumors (and neoplasms) have been induced and it is clear that in many instances new neoplasms, those which have not ordinarily been observed in the particular strain used, have been recorded following irradiation (See particularly Hollcroft, et al. 1957; Furth & Lorenz, 1954; Koletsky & Gustafson, 1955). Clearly, in other instances, for example, using Sr^{89} or Sr^{90} in CF_1 mice, the incidence of certain neoplasms, in this case neoplasms of reticular tissues, appears not to have been influenced. However, the latent period is noticeably shortened. These instances probably represent different mechanisms of carcinogenesis.

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Diverse and complex events are recorded in studies of the pathogenesis of ovarian, pituitary (thyrotropic as well as adrenotropic and mammotropic), mammary and thyroid tumors, those representing the abscopal effects of radiation. Indirect effects of radiation appear to be responsible also for the appearance, at least in certain inbred strains of mice, of lymphocytic neoplasms of thymic origin. It is to be emphasized again, however, that evidence is accumulated to indicate that the mechanisms concerned with radiation induction of other neoplasms of reticular tissues such as generalized lymphocytic neoplasms, granulocytic leukemia, the reticulum cell sarcomas, plasma cell neoplasms, etc., are quite different. The scopal effects of radiation, resulting in neoplasms arising in the target area, are represented by osteosarcomas and other bone neoplasms and neoplasms of the cutaneous tissues. Among the latter, however, there is some evidence to indicate that neoplasms arise, not in the irradiated tissue, but in adjacent tissues (See Glücksmann, et al. 1957; Marchant & Orr, 1955). These observations were recorded when massive insults were given, however.

Most of the experimental work deals with inbred strains of mice and with rats, the latter, unfortunately, were in general not genetically homogeneous groups. Nevertheless, striking species and strain differences have been revealed. It is likely, however, that species differences will become less pronounced when a greater experience is obtained with more and varied inbred strains of rats. Until recently, for example, it was

presumed that rats were strikingly resistant to the leukemogenic properties of X-rays (See Mole, 1956), yet the more recent findings, discussed above, show this not to be true.

In considering possible carcinogenic effects of small doses of radiation, 25 r or below, those for example used during certain diagnostic procedures, it is clear that little or no immediate histologic or functional changes may be observed in the irradiated tissues. Even in the most radiosensitive mammalian tissues, it has been suggested that 25 to 50 r are required to produce observable changes. The rapid rate of repair, however, in such tissues as the blood-forming or gastrointestinal tissues may obscure actual histologic changes resulting from radiation. Further, as stated earlier, it is not known that histologic changes necessarily precede carcinogenesis.

There is little clinical evidence on the subject, however the peak incidence at 3 to 4 years of acute lymphocytic leukemia in children, the suggestive evidence of increased sensitivity to radiation of foetal tissues and of the tissues of children demand that particular attention be given to the effects of low doses of radiation of foetal, embryonic and of early neonatal tissues. Experimentally, the Russells (Russel & Russell, 1952, 1954) have studied the effects of X-radiation of various dose levels, 200 r and below, on the incidence of abnormalities in inbred strains of mice. The type of damage observed and its severity depended critically on the state of

development at which radiation was accomplished. These studies, nevertheless, suggest that observable effects are to be expected at levels as low as 25 r. More recently, Russell and Major, (1957) have reported the induction of coat color mosaics following total-body irradiation of 10.25-day-old embryonic tissues heterozygous for four coat color genes. The method adopted by these workers allows one to distinguish the genetic effects of irradiation from the developmental effects. Recessive mutations in somatic tissues appear to be the most likely explanation for the results obtained, using 100 r X-radiation to the pregnant mother. Interestingly, the calculated somatic mutation rate was found to be strikingly similar to the germinal rate induced in spermatogonia for the same four loci.

There is evidence to indicate carcinogenic effects of low total doses of irradiation in experimental animals. Bilateral ovarian neoplasms in C3H mice following 50 r and probably 32 r, but not 25 r total-body X-radiation, myeloid leukemia in RF mice receiving 16 rem of fast neutrons, mammary tumors in Sprague-Dawley rats given 25 r whole-body X-rays are examples. In this laboratory (Uphoff & Law, unpublished) preliminary evidence has been obtained to indicate the effect of single whole-body exposures of 25 r X-rays on the appearance of neoplasms of reticular tissues (except thymic lymphomas). The latent period is shortened if irradiation is accomplished within the first 24 hours of the postnatal period.

This informatinn and the evidence indicating that 16 to 32 rem are leukemogenic for myeloid leukemia definitely rule out the possibility that a prolonged depression of blood-forming tissues is necessary for the development of neoplasms of reticular tissue. Prolonged depression of blood-forming tissues or specifically a disturbance in the growth equilibrium of thymic tissues may be necessary, however, for the induction of thymic lymphomas.

Much has been written concerning the dose-response relationship in radiation carcinogenesis, particularly as it pertains to radiation-induced leukemias in man. The pitfalls of such retrospective studies were discussed earlier (See also Brues, 1958). In discussions of the possible mechanisms of radiation carcinogenesis, analogy has been drawn between the production of point mutations in germ cells and the frequency of certain neoplasms as it relates to threshold, linearity of response and dose and intensity of radiation (dose rate) (Mole, 1958; Brues, 1958). It is well to bear in mind that there are a variety of possible genetic mechanisms which may account for changes in phenotype such as that expressed by the neoplastic cell: quantitative changes in the balance of chromosome parts or whole chromosomes, resulting from non-disjunction, deletions, polyploidy or chromosome rearrangement; changes in gene constitution resulting from somatic crossing over or point mutations, virus infection leading to changes in phenotype of

the host cell and transduction or similar phenomena of cell to cell transfer of genetic material. The arguments advanced for and against the somatic mutation hypothesis refer principally to the point mutation. By analogy with radiation-induced mutations in Drosophila spermatozoa, certain basic criteria should be fulfilled: the absence of a threshold, linearity of dose and response and independence of changes in cellular environment such that radiation intensity (dose rate) does not influence the mutation rate; that is, a direct randomly determined alteration in the genetic material should be expected. The danger in attempting to draw such an anology seems not to be fully realized. Analysis of point mutations in spermatozoa or oöcytes following irradiation is a relatively simple matter accomplished by classical genetic studies and these effects are determined within a relatively short time after irradiation. On the other hand, there is no method available at present to determine in irradiated somatic ti ssues the role played by selection or of the influence of the physiological state of the surrounding tissue, either as a direct or indirect result of irradiation; further, the only means of detecting the change is through expression of the neoplasms. Thus, the difficulty of determining whether the "somatic mutation" explanation of radiation carcinogenesis for a given neoplasm is reasonable or not lies not so much with the presently inadequate data, as discussed earlier, but with the inability to analyze genetically, changes in somatic cells (See Schultz, 1958

Some changes in our ideas about radiation-induced point mutations seem in order following the work of Russell, et al. (1958). Contrary to the expectations of the equal mutability of all germ cells and the independence of dose rate, these investigators report differences in the mutation rates of spermatogonia of the mouse obtained with chronic γ-radiation as opposed to X-rays. Specific locus mutation rates for spermatozoa on the other hand were not influenced by the intensity of irradiation. The findings of a lower mutation rate following chronic irradiation in metabolically active spermatogonia may indeed signify the possibility that a threshold exists even for induced point mutations. An analysis of the factors concerned with these differences in response, is of immediate interest to those working in radiation carcinogenesis. It was pointed out earlier that one of the arguments advanced against the possibility of somatic mutation being of importance in radiation-induced thymic lymphomas in mice was the dependence for expression of the neoplasms on dose-rate (Mole, 1958). Thus the arguments advanced against the somatic mutation possibility in radiation carcinogenesis do not necessarily hold. It is to be expected that radiation effects in somatic tissues will parallel the findings in metabolically active spermatogonia rather than in spermatocytes.

The existence of a long latent period before the appearance of certain neoplasms following irradiation has been discussed. The experimental results of Brues and co-workers especially concerning bone neoplasms

show this to be in the order of 5 to 8 months for the mouse and the same phenomenon has been observed in other species. It has been impossible to shorten the period significantly under most intense stimulation. It is not known, however, whether the genotypic change occurred early and the phenotypic manifestation required a certain time for expression, or whether the genotypic change occurred relatively late. Histologic studies would appear inadequate to resolve the dilemma since these would provide little information concerning the possible "progression" of neoplasms (See Foulds, 1954). An interesting and reasonable theory which considers the many factors concerned, particularly in skin carcinogenesis, and adequately explains the latent period, has been proposed by Failla (1958). It is difficult, however, to see how this can be put to experimental test.

Little can be said about the relative biological efficiency (RBE) of different types of radiation as applied to radiation carcinogenesis. It is doubtful that the biologic and physical criteria demanded for such comparisons have been met in any single carcinogenesis experiment (See Mole, 1958). In recent years the RBE of several types of radiation have been determined for a number of biological responses in mammals (Storer, et al. 1957) and for chromosomal aberrations in Tradescantia (Conger, et al. 1958). In these carefully designed and controlled studies, it is clear that the RBE for two different radiations vary with the type of biologic response studied,

being high for one response and unity or less for another response. Some attempt has been made to determine the RBE of comparable α- and β-radiations by deposited isotopes using bone tumor formation in mice (Brues, 1955). Here Sr^{89} was compared with radium and the RBE for bone tumor induction (α/β) was of the order of 1, whereas between the α-emitter Pu^{239} and Sr^{89} it was of the order of 20. The difference between other α-emitters and radium is probably the result of differences in the localization in bone.

In summary, it may be stated that only a beginning has been made in the attempt to solve some of the basic problems in cancer biology. The carcinogenic hazards from irradiations of various types and sources present serious problems. It is likely that more serious problems may be faced as our experience in man and laboratory investigations are extended. Some guiding principles may be drawn and some investigators have attempted to do so, but it is clear that these are not yet known to be applicable to radiation carcinogenesis in general. The facts indicate a multiplicity of mechanisms by which neoplasia is induced by irradiation.

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Evaluation of Data Relating Human Leukemia and Ionizing Radiation [1]

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Summary

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The central argument of this paper is a discussion of the statistical reliability of existing data relating human leukemia and ionizing radiation. Conclusions reached by some observers, partly on the basis of statistical techniques having questionable applicability, reveal a confidence in the accuracy of estimated leukemia rates that is difficult to justify. Several sources of error are outlined in detail, and their effect on the use of certain standard statistical methods is illustrated. The conclusion is reached that available data are insufficient to support any conclusion about the shape of the dose-response curve, particularly in the low-dose region.

In the years that have passed since World War II, increasing attention has been given by medical and biological scientists to the effects of ionizing radiation on living organisms. For obvious reasons, the greatest accumulation of knowledge in this field has come from experiments with animals and microorganisms. In spite of the great advances that have been made, we still know relatively little about the effects of radiation on human beings, partly because data of this kind are scarce but mainly because humans are not ordinarily used as experimental subjects when personal risk is involved. Many important questions remain unanswered because scientists are unable to design and execute the necessary experiments. With controlled experimentation virtually eliminated as a source of information, investigators have been forced to rely on retrospective studies. The retrospective study has proved to be one of the most useful tools of medical and public health scientists. Briefly, it consists of selecting from a population individuals having certain characteristics, often some disease, and then conducting a thorough examination of the past experience of each individual in order to determine what factors might be associated with the characteristic. Some of the recent studies on smoking and lung cancer, for example, have been retrospective. Although such studies are

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useful when properly evaluated, they can also be misleading when interpreted without regard for pitfalls that are frequently present.

This paper is concerned primarily with the reliability of data relating human leukemia and dose of ionizing radiation. Recently the available data on this subject were assembled and evaluated by E. B. Lewis (*1*). Included in Lewis' survey were individuals from 4 different groups: 1) survivors of the 1945 nuclear explosions in Japan, 2) patients irradiated therapeutically for ankylosing spondylitis, 3) children irradiated therapeutically for thymic enlargement, and 4) radiologists. Strictly speaking, the study of these 4 groups is not retrospective in the sense that individuals were selected because they had leukemia. On the other hand, they were selected retrospectively in the sense that they possessed some special characteristic that led to their irradiation.

It is easy to see how data obtained in this manner might differ from data obtained by first selecting individuals at random from a population and then irradiating them. For example, if one is studying leukemia incidence and it should turn out that the population selected in retrospect is more susceptible to leukemia than the general population, any estimate of incidence obtained from the retrospective study would be biased relative to the general population. The point to be emphasized here is that retrospective studies can be, and frequently are, subject to greater errors than those associated with controlled laboratory experiments.

In some scientific quarters, but by no means in all, Lewis' paper has been interpreted as proof that there is no threshold dose for the induction of leukemia by ionizing radiation. Among the published comments is the editorial entitled "Loaded Dice" by Graham DuShane (*2*), Editor of *Science*, who says in part, "E. B. Lewis (p. 965) shows that there is a direct linear relation between the dose of radiation and the occurrence of leukemia . . .," and, "Thanks to Lewis, it is now possible to calculate—within narrow limits—how many deaths from leukemia will result in any population from any increase in fallout or other source of radiation." Others, while not disputing the widely accepted fact that ionizing radiation does induce leukemia, feel that the question of a threshold dose is far from settled. Without intending to detract from Lewis' otherwise penetrating discussion and summary of existing data on this subject, the present paper purports to show that our knowledge of the correct limits of error for leukemia rates in irradiated human populations is insufficient to support definite conclusions. In this connection, it is relevant that a former member of the Atomic Bomb Casualty Commission, closely connected with the leukemia study in Japan, has expressed similar sentiments (*3*) with respect to the presently available Japanese data.

Basis for Statistical Methods in Research

Statistics is a relatively new discipline among the fields of science, and its use by the research worker has grown rapidly during the past 25 years. This growth has been accompanied by a substantial increase in our knowledge of the mathematical foundations of statistical theory, but in

spite of these advances many kinds of data cannot be examined statistically unless the scientist makes certain assumptions about the conditions under which the data were obtained. Only in rare instances can such assumptions be proved, mainly because experimental subjects seldom behave exactly as if they were black and white balls in an urn. Nevertheless, statistics, when properly applied, has an important role in the evaluation of experimental data, and a careful consideration of the necessary assumptions is essential.

In practice, the statistician is willing to accept some departures from the rigorous framework underlying his methods, but not before he attempts to assess the relative magnitudes of the departures and their possible effect on his ultimate analysis. Generally speaking he prefers to be conservative, and in most cases this means that if he knows that one of two procedures is correct but cannot identify which it is, he will choose the one that yields the larger estimate of error. This assures him that if his analysis leads him to state, "The probability is 0.95 that the interval (A, B) includes the true mean, μ," the correct probability will be equal to or greater than 0.95. Despite his willingness to allow some leeway in the assumptions, every statistician is eventually confronted with data that are subject to a large number of errors about which he has little or no quantitative information. Under these conditions, statistical analysis is of questionable value. There is every indication that the presently available data relating leukemia death rates and radiation dose fall into this category.

The real point at issue is the extent to which Lewis' analysis has advanced our knowledge of the relationship between radiation dose and leukemia incidence, particularly with respect to the existence of a threshold dose. In this connection there is at least a reasonable doubt that the statistical assumptions inherent in his analysis can be justified, and the following sections of this article are intended to establish such doubt. The Japanese data are mentioned most often because they are the only data cited that could conceivably provide direct evidence about low doses, *i.e.*, doses in the range in which a threshold is of most interest.

Recognizable Sources of Error

In the previous section it was stated that the assumptions underlying the use of statistical methods can seldom be proved for any particular experiment. Occasionally it is possible to demonstrate mathematically that certain assumptions, such as homogeneity of errors, are not satisfied, but more often any particular assumption is as hard to disprove as it is to prove. In the majority of cases then, the user of statistical methods must assemble all past and present information that might be pertinent to data of the type in his experiment and that might assist in evaluating the validity of the methods he proposes to use. For mature research workers, most of this information will be the general aggregate of their own past experience. For younger investigators or in unusual experimental situations, it is often not safe to proceed without a careful con-

sideration of the conditions under which the data were obtained, particularly as these conditions relate to the statistical assumptions. The following illustration shows how easily errors in assumptions can arise.

In dominant lethal mutation studies with *Drosophila*, mutation rates are estimated from counts of the numbers of unhatched eggs out of the total number laid. In order to obtain sufficient data, it is usually necessary to make counts for several vials. Because of environmental and other differences, the vial-to-vial variability in percentage unhatched is usually much greater than one would expect for relative frequency data, that is, greater than binomial variability. It is not unusual, however, to find limits of error for data of this type computed on the basis of the binomial distribution, simply because the data take the form of relative frequencies. Such limits, of course, are too narrow, and Cochran (*4*) has given correct methods of analysis for this case. The relevance of this particular illustration should become apparent later.

Let us turn now to the human data relating leukemia incidence and dose of ionizing radiation. The first step in determining what statistical assumptions are reasonable for these data should be a consideration of the errors that may have been involved in their collection. Proper identification of errors is necessary whenever statistical methods are to be used, as the previous example clearly shows. Broadly speaking, errors inherent in experimental data can be classified as gross errors, biases, or random errors. Gross errors result from poor arithmetic, faulty transcription of numbers, etc., and we are not concerned with these in this paper. Biases result from many different causes, the net effect of which is to yield estimates that are consistently too large or too small. Random errors are defined in a succeeding paragraph. Frequently in repeated experiments, errors that ordinarily would be attributed to bias vary from one experiment to another, that is, the bias is not constant. This occurs, for example, when factors capable of affecting the outcome of an experiment, such as age or weight of subjects, are not held at constant levels from one experiment to the next. For convenience, and to distinguish them from truly random errors, these have been called extraneous errors.

Any of the aforementioned types of error may be present in the leukemia data. It would be futile to attempt to cite every possibility, but the specific sources listed below should be sufficient to demonstrate the complexity of the situation for readers unfamiliar with this kind of data.

(*a*) *Dosimetry errors.*—The measurement of radiation dose is not exact even in the best-equipped laboratory. Obviously, in the case of the Japanese exposures, no direct measurements on the ground were possible. Furthermore, the two detonations were qualitatively and quantitatively dissimilar, and the two cities were topographically very different, thus providing quite different shielding effects. Lewis' dose-distance curves were constructed from data presented by Neel and Schull (*5*). These authors give neutron and *gamma* component curves separately for each city. The *gamma* curves are comparable but the neutron component is considerably greater in Hiroshima. The assumption that neutrons are

1.7 times more effective in producing leukemia than *gamma* rays yields remarkably similar dose-distance relationships for the two cities, whereupon the conclusion is made that it is legitimate to pool the leukemia cases from both cities. It is indeed hard to believe that effects in the two cities due to differences in the detonations and differences in shielding have balanced one another in such a way as to yield the curves presented. Certainly Neel and Schull (*5*), who have been very closely associated with this study, give no indication that they were able to reach such a conclusion.

Dosimetry for some of the other sets of leukemia data is possibly somewhat better than the presently available dosimetry in Japan, but these other data present a new problem. Little or nothing is known about the etiology of leukemia. If a threshold dose for the induction of leukemia exists, its magnitude might easily depend on the manner in which radiation is administered, that is, whether in one large dose, in a series of small doses, or chronically. The data include examples of several types of administration, and comparisons among them are certainly not precise enough to support with any confidence the conclusion that acute and chronic radiation exposures yield similar leukemia rates. Thus, if one considers all aspects of the dosimetry problem, it should be clear that the apparent agreement among the calculated leukemia rates per individual per year per rad for the four sets of data may be highly fortuitous. The truncation errors discussed below add further strength to this observation.

(*b*) *Sampling bias.*—In studies on human populations, the question of sampling bias is always foremost. Experts in the field of sample surveys have long been plagued with this problem, several examples of which have occurred in connection with predictions of presidential elections. When sampling bias is present, group differences which appear to be statistically significant with respect to the factor being studied may in fact be due to extraneous factors that were not equally represented in both groups. Efforts are always made to control known sources of bias before samples are taken, but experience shows that these efforts are seldom successful in eliminating all causes of bias. Three sets of the leukemia data deal with very special groups, two that were ill when first irradiated and the third, a group of radiologists. Apparently there is no evidence that would suggest any correlation, other than that due to radiation, between the characteristics of these groups and the incidence of leukemia. On the other hand, with so little really known about leukemia, it would be difficult to state categorically that no such correlations exist. If they do exist, conclusions based on these data could not be applied legitimately to the general population.

Of the four sets of data, the Japanese data are perhaps best from the point of view of sampling bias, in the sense that the exposed population is more representative of the entire population. Here, however, the investigators were forced to rely almost completely on the patient's recollection (several years after exposure) of his position at the time of exposure for the purpose of assigning him to a dose or distance category.

Although great care was exercised to ascertain the accuracy of such recollections, some errors have undoubtedly been made.

(*c*) *Truncation errors.*—From animal studies, it is well known that the distribution of ages at death in irradiated populations is different from that in unirradiated populations. Irradiated populations tend to die earlier, and to some extent certain diseases become relatively more important. Thus, when one compares two such populations with regard to a specific disease incidence before all members of both populations have died, the distributions of ages at death are truncated at different percentage points in the distributions. Clearly, in the Japanese situation, if leukemia victims in the irradiated group tend to die earlier than those in the control group, the incidences that will prevail in these populations after all members have died may be considerably different from the ones Lewis has calculated. In particular, the control group incidence will be relatively greater, and the dose-distance relationship, if linear now, will certainly not be linear when complete data become available. Wald's more recent tabulation (*3*) suggests exactly this, so that truncation errors of this kind are a definite possibility.

(*d*) *Random errors.*—In addition to errors (*a*) to (*c*) and others like them, some or all of which may be negligible in any one experiment, there are the random errors which form the basis of statistical theory. They occur when a population is sampled because the sample is virtually never an exact replica of the population. Only when one's sample includes the whole population do the random errors vanish. In the leukemia data, the random errors are binomial, but the incidences are small enough so that the binomial distribution may be approximated adequately by the Poisson distribution, if desired.

Application of Statistics to the Leukemia Data

With the background provided by the previous sections, it must be clear that before attempting a statistical analysis of the leukemia data, one must decide what assumptions about error are reasonable. Random errors are most certainly present and from statistical theory we know that they would be binomial if the sample sizes were accurately known. In the Japanese data this is not true. The sizes of the exposed populations, *i.e.*, the sample sizes, were estimated by methods described by Neel and Schull (*5*). Thus, appropriate random errors for the Japanese data will be greater than binomial (or Poisson) by an amount related to the precision of the estimated population sizes. Probably more important, however, is the matter of extraneous errors. Neither the existence nor the absence of these errors can be proved, but workers in the field of radiation biology will recognize that some of the errors mentioned in the preceding section and others like them are most certainly present in all of the leukemia data. Our difficulty is that data of this kind are so sparse that we have no way of accurately assessing the magnitude of extraneous errors, and no way of imposing an upper limit on them. Under conditions in which accurate information about errors is lacking, any statis-

tical analysis based on the assumption that only binomial errors are present must be interpreted with reservation.

In view of this, it is appropriate to examine the statistical methods used by Lewis and to see how they are related to the threshold problem. His methods include the construction of confidence intervals based on the Poisson distribution and use of the standard chi-square technique for testing the difference between two binomial populations. The distribution theory that underlies these methods makes no allowance for any errors other than binomial or Poisson errors. To see explicitly how this assumption affects the statistics, consider the chi-square test on page 970 of Lewis' article. Here he is testing the difference between the leukemia incidences among survivors of the Japanese explosions in zone C (p_1) and zone D (p_2), representing average doses of 50 rem and 5 rem, respectively. The formula for χ^2 may be expressed as

$$\chi^2=\left[\frac{p_1-p_2}{\sqrt{p\,(1-p)\left(\frac{1}{N_1}+\frac{1}{N_2}\right)}}\right]^2$$

where N_1 and N_2 are the numbers of exposed individuals and where p is the combined incidence for both groups. It may be seen above that the chi-square test is actually a t test with the variance of (p_1-p_2) assumed to be $p(1-p)\ (N_1^{-1}+N_2^{-1})$; that is, binomial error has been assumed. The data yield a χ^2 of 5.6 which corresponds to a significance level of about 2 percent, as stated. At this point in the discussion it is inferred that because the difference is significant, there can be no threshold dose, at any rate, none above 50 rem. Suppose now that extra-binomial error contributes twice as much to the total variance as does binomial error, not too unusual for some biological data. In this event, the χ^2 would be reduced to 1.9. This corresponds to a significance level of about 24 percent, and one would hesitate to conclude that the observed difference between zones C and D is a real one. The numbers themselves are unimportant, but the possible effect of extra-binomial errors on the tests of significance is of great importance, in view of the conclusions reached by some observers. Kendall (*6*) discusses the statistical theory of both extra-binomial and extra-Poisson variation and gives an actual example. He also indicates how difficult it is "to find material compiled outside the laboratory" that conforms to binomial or Poisson distributions.

In various places throughout the article the Poisson distribution has been used to construct confidence limits. It should be pointed out that an error has been made in computing these limits.[2] More important, however, is the fact that the correct Poisson limits are too narrow, perhaps by a large factor, unless all extraneous variation is negligible.

On page 969, 17 observed deaths from leukemia are compared with an "expected" number of 3.4 deaths. Based on the Poisson distribution, a

[2] The limits referred to as 95 percent limits are actually 90 percent limits.

test is made of significance of the difference between the 2 numbers. The test is valid only if there is no extra-Poisson error and if 3.4 is a known constant. In fact, the number 3.4 is not without error and any statistical test would have to take this into account. Apparently the number does represent some sort of arbitrary upper limit on the true expected value, but one cannot make probability statements on this basis.

Conclusions

The most obvious conclusion to be drawn from the foregoing discussion is that statements attributing great accuracy to radiation-induced leukemia rates in human populations are open to serious question, particularly those which suggest a strictly linear dose-response relationship. Lewis did not claim to have established linearity between leukemia incidence and radiation dose, although he implied that linearity is a strong possibility. From the purely methodological point of view, nothing has been done statistically that would either support or disprove the hypothesis of linearity. The linearity hypothesis precludes *per se* the concept of a threshold dose. About the latter, Lewis says, "Thus these data provide no evidence for a threshold dose for the induction of leukemia." and later, "This finding also fails to support the concept of a threshold dose below which leukemia will not develop." One has to admit that Lewis has avoided stating the definite conclusion that there is no threshold dose, although the mode of expression clearly implies this conclusion. The wording is unfortunate because, from a strictly statistical point of view, both statements could equally well have been replaced by similar ones stating the failure of the evidence to support the hypothesis of *no* threshold dose. In other words, the evidence is insufficient to support either hypothesis, and this is about the only conclusion one can safely reach from the data presented. It should also be apparent that the quotation referring to the prediction of radiation-induced leukemia incidence "within narrow limits" has absolutely no basis even by indirect inference from the article. It is important to emphasize this point because other scientists have made statements implying similar beliefs about the precision of the calculations.

Finally, a word of caution is necessary. This article may be interpreted by some readers as representing a point of view opposite to that inferred by Lewis (and adopted more confidently by others), but in reality its only purpose is to emphasize the shaky grounds on which any definite conclusion would be based. Nothing that is said here should be taken as an argument supporting the hypothesis of a threshold dose for the induction of leukemia by ionizing radiation. An attempt to use the same data to establish a threshold would be open to similar criticism. The only sound conclusion to be made is that, in spite of the established fact that ionizing radiation induces leukemia, evidence at the present time is insufficient to justify any hypothesis about the *shape* of the dose-response curve. We can only hope that scientists working on this critical problem will soon provide us with adequate evidence.

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Representative HOLIFIELD. Thank you, Dr. Law. We have a few questions. We will probably have to put on Dr. Crow and Dr. Russell as the first afternoon witnesses because of the time lag. Dr. Tompkins is now going to direct some questions to whichever of you gentlemen wishes to answer, or both.

Dr. TOMPKINS. Thank you, Mr. Holifield.

Dr. Law, I would like to firm my understanding of one part of your prepared testimony. In your prepared statement you say that the latest evidence obtained through 1957 concerning leukemia in the Hiroshima survivors gives no indication of a decline in the incidence or that a plateau in the occurrence of leukemia has been reached. Thus the likelihood must be considered that the final results, when the entire population has died, may indeed show different relationship than the present results would indicate.

I would like to confirm, first of all, that the word "no" is there by intent and really reflects the trend of the data.

Dr. LAW. This is my interpretation of the data as I have seen them through 1957. Of course, I understand that there still may be additional cases recorded for 1957. I do not detect a decline in the frequency or detect that a plateau has been reached for the frequency of the disease.

Dr. TOMPKINS. I just wanted to establish firmly that your interpretation of the data was no decline.

Dr. LAW. That is right. This is my interpretation.

Dr. TOMPKINS. I just wanted to be sure that point was clearly established. I would like to address the next set of questions to both of you jointly, if I may.

I believe, Dr. Brues, in your testimony you covered the question of how many, or the relationship between leukemia or cancer incidence in mice and the concentrations of strontium. I believe both of you have touched on that question. It is my understanding that Dr. Finkel of Argonne did a very nice study involving this question and came to the conclusion that indeed a threshold does exist. She further came to the conclusion, as far as she could tell, no one at the present time has been a victim of leukemia due to fallout radiation.

It is also our understanding that based on these same data, Dr. Pauling has issued a paper in which he concludes that the information presented is statistically not capable of distinguishing between the question of threshold or no threshold.

I would like to ask both of you gentlemen to please evaluate the status of this question at the present time. The question is: Is there enough evidence to draw a conclusion or is there not enough evidence to draw a conclusion?

Dr. BRUES. I think there is not enough evidence at the present time to draw a conclusion that no case of leukemia has been produced by present fallout. I believe the sentence in question in Dr. Finkel's paper was one in which she indicated that it appeared extremely likely that this was the case. The rather scholarly mathematical treatise based on this paper which Dr. Pauling published in the proceedings of the National Academy of Sciences was largely directed to the fact, which I think Dr. Finkel essentially agrees to, that an airtight proof on this had not been established, and that this could not be established without the use of many millions or billions of mice, which

is the statement that I made earlier in my testimony. That is a pure matter of statistics. The statement on the other hand was based on the fact that in the range where values are statistically significant, they do not fall on the straight line which comes out at the zero point, the one which I drew on the blackboard, but that in every case or practically every case the slope is steeper than that. Those who have been proponents of the opposite point of view, I have noticed, have carefully neglected to take this matter into consideration, and I think this is an important point.

I think we all agree that we need to know more of this, and this is why some experimental work has been undertaken to use considerably larger numbers to get more significant points.

Dr. LAW. May I answer, too, since you addressed your question to me? I have said here in the statement that the present data though probably inadequate are consistent with the concept that there exists a threshold dose. They certainly are far from proving this. I feel that there are few data, even the published results of the studies of the strontium 0. I feel they are probably inadequate to solve the question at the moment of threshold or the question of proportionality, which I have not discussed. I feel that most of the data obtained from the experimental animals probably are not sufficient to answer this. This certainly is not proof that there is a threshold. It is consistent with the concept that there may be a threshold.

Dr. TOMPKINS. If one had to choose up sides on the threshold versus nonthreshold, should we then conclude that you would favor joining the ranks of the threshold advocates, as far as your experience and information is concerned? This is the impression I got from your testimony.

Dr. LAW. I feel that the inadequate data concerning only leukemia point in this direction. I want to point out again that there are so many variables such as the influence of the age of tissue irradiated or of strain or the possibility of cocarcinogens, which have not received any attention at all and which may make all the difference in the world in the expression of these responses.

Dr. TOMPKINS. May I explore this just a little further. I believe the testimony has indicated that the linear hypothesis is based largely on evidence which has come up through the field of genetics, and it is the introduction of a genetically formulated theory and applied to all the cells of the body. It is my understanding that there are several people that really believe that this is a correct representation of a biological mechanism. There are also many who do not believe that this is true.

From your point of view, Dr. Brues, if I understand your testimony correctly, you feel that this is not a permissive translation because the one point genetic theory is not all-controlling? There are other equally important variables so you cannot use such a basic and simple hypothesis in the somatic field, is that correct?

Dr. BRUES. This is correct. Certainly as I have cited in my statement there are instances where one produces cancer by a mechanism which does not seem to be producing mutations in cells, but seems to be making life difficult for some tissue, and then cancer arises there. I don't deny that some genetic change may be a part of the cancer process. But I would emphasize that this proportional theory is

hanging everything on the mutation, and saying that nothing else counts, or at least nothing else that has anything to do with radiation counts. I think that is erroneous. That is why I hesitate to say there is an absolute threshold. I can visualize people who have a perfect setup, a tissue that is in poor shape, exposure to a cocarcinogenic chemical, other genetic predilections, who might be all ready to develop cancer. They might get it from a single hit.

Dr. TOMPKINS. Then I understand that the conclusions of the 1957 hearings are essentially still valid; namely, that all the experimental observations that can be obtained on this subject do require total doses considerably higher than those under consideration at this time. Therefore, all conclusions by whatever hypothesis must be extrapolated a considerable distance from the current data. Is this a fair summarization of the current status?

Dr. LAW. Yes.

Dr. BRUES. Yes.

Dr. TOMPKINS. I would like to push this point a little further but I think there are other points that are equally important, and therefore I will drop for the moment, unless one of you has something additional you would like to say.

Dr. LAW. I did not have a chance to answer your question concerning the genetic and nongenetic concepts if this is what they are. I should think that at some time and some place some cancer is the result of a somatic mutation. The difficulty, of course, is in the attempt to prove this—this has been argued back and forth for years. When one is concerned with genetic effects, this is a simple matter of mating two cells and getting results. In somatic cells, for example, mammalian cells do not mate or fuse or exchange genetic material. One must come up with some other technique to show that a somatic change is taking place in the cell. Perhaps this is possible or will be possible in time. It is possible to show somatic changes in bacteria and other organisms in which genetic transfer of material occurs from cell to cell without mating. I know of no evidence available at the present time to show that this happens in mammalian cancer cells. If a cell actually mutates or a genetic mutation occurs as a result of radiation, we know practically nothing of the influence of the surrounding cellular environment on that cell, and of the processes of selection or of selective killing. Perhaps the mutated cell is at a disadvantage as far as the rest of the tissues are concerned. It is a question that cannot be answered directly.

Dr. TOMPKINS. Dr. Brues, you quoted some results which I gathered from your statement was a personal communication from Dr. Upton. For the record, do you know what the dose rates or total doses used in the experiments were?

Dr. BRUES. I am sorry I don't have Dr. Upton's letter here. The single doses were fairly high single doses. I believe more than 100 roentgens. The chronic dosage was done in a cobalt exposure room where the dose was given 23 hours a day, that is, almost continuously over the course of several months. Total doses came out the same. In one case he saw no leukemia, and in the other case that was the predominant disease that the animal suffered.

Dr. TOMPKINS. Dr. Brues, do you know whether any of the animals were irradiated during the fetal stage or were all of the animals in this experiment irradiated after birth?

Dr. BRUES. This is another matter which I found rather interesting. This was in a paper delivered by Dr. Upton to the meeting of the American Association for Cancer Research last month. He irradiated animals susceptible to myeloid leukemia—RF mice—at various ages, giving them doses of 50 or 100 roentgens. Again I don't remember the exact dose, but it was a good leukemogenic dose. He found that the younger animals just after birth did not develop leukemia to any great extent, although there was a period in early youth when the animals did. He then pushed the thing back a little further and irradiated these animals in the last 2 or 3 days before delivery which would correspond to the time under consideration in Dr. Stewart's investigation, and he found that these mice did not develop leukemia. In fact, these mice had less leukemia when they grew up than did the others.

This may indicate a difference between mice and men. I am not just sure what it does indicate. It indicates that we cannot go directly into a mouse experiment which might show up the details of the Stewart phenomenon without a little more investigation.

Dr. TOMPKINS. I would like to move to another question, if I may. Your testimony, Dr. Brues, stated with regard to some of the material provided by Dr. Marinelli that in Lemont, Ill., that the gamma background was approximately three-quarters of the natural dose rate. Would you explain to us, please, if the dose rate is three quarters background, why is not a 70- to 30-year dose also raised by the same amount?

Dr. BRUES. I am not sure that I exactly understood the question.

Dr. TOMPKINS. Let me rephrase the question. You have stated you are dealing with short-lived materials. You also noted that in order to establish the actual significance of the dose rates that you were measuring you had to take into account the decay rate. My question is, it is customary among many people to multiply an observed dose rate at any time, whatever time is under consideration, and from this computing the total dose and hence the biological significance. I would expect many people to do the same thing with respect to this number. My question is why isn't it right?

Dr. BRUES. This is the reason why these data were expressed in microrads per hour instead of the millirads per year which would give that impression perhaps more readily. I would say that the total dose over the course of the year following this—one can't say exactly what this would be because one does not know how much more zirconium is coming down while it decays—the total dose would be of the order of a tenth of the background dose. At the moment when this observation was taken, the dose rate was three-quarters of natural background.

Dr. TOMPKINS. Can the implication be drawn from this testimony that in dealing with fission products, particularly short lived fission products, one cannot simply multiply the dose rate observed at any time by any other convenient time. The relationship between time, actual radiation delivered, and observed dose rate is an extremely complicated one and correct deduction of the hazard requires that this complication be reflected in the analysis.

Dr. BRUES. That is quite right.

Dr. TOMPKINS. I would like to conclude now with just one point

which I think perhaps is the most important. There has been submitted for the record of the committee a statement by Dr. Lewis, of the California Institute of Technology, which bears on the problem of possibility of tumors induced in thyroids of children and also another section covering the relationship between strontium 90 and leukemia (see p. 1552). Inasmuch as this testimony will appear in toto in the record, but has not appeared, and in order to establish a background, I would like to very, very briefly have the principal points which Dr. Lewis made, and then ask your opinion of one of his conclusions.

Briefly, he has come to the conclusion from the amount of iodine currently found in milk, as testified to yesterday by Dr. Wolf of the Public Health Service, it was possible that the thyroids of children drinking this milk would receive from this iodine 131 approximatley 1.2 rads which he translates to roughly a doubling of the dose to the thyroid received from natural background. He also notes or raises the question whether radio iodine levels as low as these are capable of producing thyroid cancer is not known, and may not be known for many years.

Further on, drawing again from exposure much greater than that of background, and with the assumption that proportionality is indeed permissible, he finds that it is possible to compute that of approximately 40 million children under 10 in the United States, that will have received this amount of radiation over the past 5 years, that some 80 to 1,600 cases of thyroid cancer might be expected, assuming these translations are indeed permissible.

He also goes on to state that there is absolutely no way to demonstrate this. However, his concluding sentence applying to the treatment of thyroid cancer is probably one of the most significant that I have seen in a long time. I would like to read it:

> On the linear hypothesis, it is the average dose over the entire population of the United States which is important and local hotspots do not then have the large significance that is often attributed to them nor from the linear hypothesis does the individual who receives a large does of radiation need to be alarmed since the individual's probability of developing a malignancy remains relatively low, even after heavy doses of exposure.

In view of the fact that Dr. Lewis has been perhaps the principal proponent of the proportionality theory and it is his methods of computation that are often used by others, I think this interpretation of a relationship between or an application of a linear theory to a population is very, very significant. I would first of all like to obtain from you gentlemen your evaluation of this conclusion.

Dr. BRUES. Am I to respond.

Representative HOLIFIELD. May I pose a question and ask Dr. Tompkins, your succession of experts from the article are in harmony and purpose of the content of the article. They are not selected out of context?

Dr. TOMPKINS. My purpose of going through the summary was to establish the context under which Dr. Lewis made this final statement, so that it would not be misinterpreted or applied to places to which he did not intend.

Representative HOLIFIELD. You gentlemen have read the statement of Dr. Lewis. Since he is not present, I want to establish this point in fairness to him. Not in doubt of my staff, I admit. It is for the record.

Dr. BRUES. Yes; I had a chance to read this briefly this morning. I will say that I think I agree with this statement of Dr. Lewis. This is not to be taken as agreement with some of his other hypotheses. What he is saying here, which I quite agree with, is that if the proportional hypothesis is correct, then we don't need to worry about the distribution between high doses in some areas, and low doses in others. Whereas, if the curved line is the correct one and the effect goes up very much as you get higher doses, then you should want to look very sharply at those points where the dosage is high, because this is where you might get into difficulties. I think this is a very interesting statement. I quite agree with it there in the sense that in selecting one or the other of these hypotheses, you would be inclined to take a somewhat different view.

Representative HOLIFIELD. For further clarification this refers to somatic effects and not genetic effects.

Dr. BRUES. As I understand it, he is referring here to somatic effects on thyroid. In the case of genetic effects, I would say the distribution probably almost is irrelevant, but not entirely.

Dr. TOMPKINS. I would like to point out that you can carry this method one step further, even. The conclusion is that one is entitled to draw from this statement, and I am sure these conclusions will be drawn, that is in the proper treatment of the linearity hypothesis as applied to large populations, one is not entitled to draw a correspondingly direct relationship to the personal injury to any individual. If such a relationship in truth exists, one would not be entitled to come to this conclusion. Individual hotspots is of small concern and no individual need be alarmed at personal injury to himself. I wanted to bring the point up because it is the confusion between what happens to the individual versus what must be considered as something which can only be seen in terms of a whole population has been one of the main reasons why it has been so extremely difficult to get what looks like a clear statement on the subject.

I would like to ask one question of both of you gentlemen. Dr. Lewis' statement applies specifically to thyroid and low levels of iodine 131. However, I think the principle is a fundamental one. Do you gentlemen believe that one is entitled to draw the same correlation between strontium 90 in the bone to local hotspots and local excesses of concentrations for strontium 90 distribution that has been derived by Dr. Lewis for the thyroid? Is such a translation valid?

Dr. LAW. I am sorry that I have not had a chance to read this summary, and I would rather not comment at this time.

Representative HOLIFIELD. We will take this matter up further in the panel this afternoon, and there will be an opportunity at that time. Do you care to make a comment, Dr. Brues?

Dr. BRUES. If we were to accept the linear hypothesis literally, and I trust I have made it clear that I don't, then I think that the hotspot distribution ceases to be of importance until one gets up to levels of radiation very much higher which are going to kill individuals in an obvious way. It is a little difficult to answer a question which is based on a premise to which I don't subscribe.

Dr. TOMPKINS. I would like to make one other observation. I agree that many of our witnesses have not been concerned about the hotspot problem. However, on behalf of the committee I am also

aware that people who may be living in such areas are concerned, and it is part of my purpose to attempt to draw the specific technical facts that lead to the conclusion so that they can reach this themselves.

Representative HOLIFIELD. Thank you, Dr. Tompkins. Thank you, gentlemen, for your testimony. We will excuse you at this time, if there are no further questions.

I would like to insert at this point a statement by Dr. E. B. Lewis: (The statement referred to follows:)

STATEMENT OF DR. E. B. LEWIS[1]

This statement will be concerned with two aspects of the somatic effects of fallout radiation. Part I is devoted to a serious fallout hazard whose importance had not yet been fully appreciated at the time of the 1957 hearings on fallout of the Special Subcommittee on Radiation. Part II is a brief account of some developments since 1957 in the field of human leukemia and ionizing radiation.

PART I. THE RADIATION HAZARD FROM THE RADIOIODINES IN FALLOUT

It has become increasingly clear since 1957 that the radioiodines in fallout are a special hazard to infants and children. This hazard arises from a variety of reasons. Radioiodine is a significant fraction of the fresh fission products released by nuclear weapons explosions. Grazing cattle ingest and inhale the radioiodine in fallout and then concentrate it in their milk. Infants and children are expected to ingest more of the isotope than will adults since fresh cow's milk is the principal source of fallout radioiodine in the human diet and young people obviously drink more fresh milk than do adults. As has long been known, iodine isotopes, natural and radioactive, concentrate in the thyroid gland. Moreover, for the same amount of radioiodine orally ingested, the infant thyroid receives some 15 to 20 times the dose that the adult thyroid receives. (Briefly, this is because more radioiodine is taken up by the infant than by the adult thyroid; as a result many more of the short-ranged iodine-131 beta rays will be generated in a gram of infant than in a gram of adult thyroid tissue.). Finally, in spite of its small size, the infant thyroid may be more susceptible than the adult thyroid to cancer induction by ionizing radiation.

It should be noted that the special hazard of radioiodine to infants and young children was clearly recognized as early as 1955 in connection with considerations of hazards arising from nuclear reactors (1). Its importance has been clearly brought out more recently in studies of the Windscale accident in England (2).

From published data by Campbell et al. (3) on the amount of iodine-131 in fresh cow's milk during a 16-month period commencing June 1957, it can be estimated (4) that the thyroid glands of the average infant and young child in the United States have received doses of beta radiation from this isotope that amount to roughly 0.1 rad to 0.2 rad per year. (This is approximately one to two times the dose they would receive annually from natural background radiation.). There is some reason to believe that such doses have been delivered annually over the last 4 or 5 years of weapons testing.

Whether radioiodine levels as low as these are capable of producing thyroid cancer is not known and may not be known for many years. However, it is known from studies by Simpson and Hempelmann (5) that individuals who received X-ray doses of several hundred r. units to the chest region in infancy for treatment of an enlarged thymus show a highly increased rate of thyroid cancer. Thus, these investigators found 10 cases of thyroid cancer in 1,502 children who had been irradiated in this way and who were traced for an average followup period of about 13 years. In the "control" composed of 1,933 siblings, no cases of thyroid cancer were observed. The observed increase in the irradiated series is statistically highly significant. The elapsed time between irradiation and onset of thyroid cancer in this series of individuals ranged between 5 and 20 years. The average absorbed dose delivered to the thyroid gland of the 1502 treated infants is not known exactly but can be inferred to be

[1] B.A., University of Minnesota, 1939; Ph. D., California Institute of Technology, 1942. Professor of biology, California Institute of Technology, Pasadena, Calif.

in the range of about 200 to 400 rads. If is is assumed that a linear relationship exists between incidence of thyroid cancer cases (6) per year and dose delivered to the thyroid region in infancy, then the probability of thyroid cancer lies roughly between 5×10^{-7} and 5×10^{-6} per individual per rad per year over at least the first 15 to 20 years following the irradiation. Stated in another way, if the relationship between incidence of thyroid cancer and dose is a linearly proportional one between 0 and 400 rads, then a single dose of 1 rad delivered to a population of 1 million infants would be expected to give about 10 to 100 cases of thyroid cancer over the first 20 years of life of that group. To have a minimum estimate it will be assumed that this is also the number of cases which would develop over the entire lifespan of this group as a result of the initial irradiation.

Recent evidence indicates that children as well as infants who receive thyroid irradiation have an increased thyroid cancer experience. That is, children treated for hyperthyroidism with radioiodine showed an increased rate of thyroid cancer (15).

Over the last 5 years it may be surmised that about 40 million children (under 10) in the United States have experienced an average total accumulated dose from the radioiodines in fallout that is equal to about 0.2 to 0.4 rad. If such individuals experience the same probability of thyroid cancer per year per rad as the infants in the thymus-irradiated series, then some 80 to 1,600 cases of thyroid cancer might be expected to develop over the lifetimes of these 40 million individuals. The number of cases predicted in this way is seen to be a small percentage of the total number of individuals involved. However, neither total number of cases nor the percentage they represent of the total population are adequate measures of the extent of human suffering which would be involved. These numerical estimates have been presented merely in order to give some idea of the magnitude of the damage which might result in the United States from the radioiodine released in past weapons tests.

If there is a threshold for the induction of thyroid cancer and if this threshold has not yet been exceeded, then no cases of thyroid cancer may trace to fallout from past weapons tests. On the other hand, there have been extremely wide variations in radioiodine levels, with time and with locality, which will have to be taken into account before it can be said even on the threshold hypothesis that no damage could have resulted from past tests. Moreover, many children receive doses of medical X-rays that are near if not over the threshold dose for the induction of thyroid malignancy and on the threshold hypothesis these individuals are those most sensitive to the added radiation from fallout. On the linear hypothesis it is the average dose over the entire population of the United States which is important and local hot spots do not then have the alarming significance that is often attributed to them nor, from the linear hypothesis, does the individual who receives a large does of radiation need to be alarmed, since the individual's probability of developing a malignancy remains relatively low even after heavy dose exposures.

PART II. NEW FINDINGS IN THE FIELD OF HUMAN LEUKEMIA AND IONIZING RADIATION

Since 1957, the most significant development in the field of human leukemia in relation to ionizing radiation has probably been the publication in June 1958 of the extended findings of Alice Stewart and collaborators (7). These investigators studied the radiation histories of the mothers of the majority (83.6 percent) of children under age 10 who were reported to have died of leukemia or other malignant disease in England and Wales during the years 1953–55. Comparison with a "control" group of 1,742 mothers of a matched set of five children, showed that a greater number of mothers of the children who developed cancer had received abdominal X-ray examinations during the relevant pregnancy. Moreover, this difference was statistically highly significant. Recently, Lejeune and Turpin (8) have concluded from an analysis of these results that a dose of one to three r units delivered to a fetus 7 to 8 months old suffices to double the normal incidence of leukemia in children under 10 years of age.

Noteworthy also are the studies of Manning and Carroll (9), and of Polhemus and Koch (10) which show a positive association between leukemia and therapeutic X-rays in childhood. Studies by Kaplan (11), and by Polhemus and Koch also consistently show a positive association between prenatal X-ray exposure and development of leukemia in childhood. However, a statistically significant association was found only in the case in which Kaplan compared prenatal exposure in leukemic children with prenatal exposure of the sibs of such children. In this connection it should be noted that each of the U.S.

studies has thus far involved a much smaller number of leukemia cases than did the British studies. There are also added difficulties in the U.S. studies owing to apparently more widespread irradiation of the "control" children in this country than in Great Britain.

These studies and others (12–14) that have been reported since 1957 have contributed results which are in substantial agreement with the conclusions drawn in testimony presented by the present witness at the 1957 hearings. As far as evaluating the risk of developing leukemia is concerned, the newer findings emphasize the importance of keeping in mind the possibility of a greatly increased sensitivity of the fetus (and perhaps of the young child) to radiation-induced leukemia. No attempt will be made here to evaluate the extent of increase since 1957 in the dose to the bone marrow from fallout levels of radiocesium and the radiostrontiums. However, it should be noted that the dose to children's bone marrow from past weapons tests has probably been much lower than the dose to their thyroid glands (pt. I). Since the radioiodine from past weapons tests has now largely vanished while strontium 90 and, to a lesser extent, cesium 137 will continue to irradiate bone marrow for some time to come it is difficult to evaluate the relative number of thyroid cancers verus leukemias which might result from fallout already released. It, of course, is still more difficult to compare the number of cases of somatic versus genetic injury which might result from fallout radiation. Presumably, the genetic damage is the more significant in terms of individuals ultimately affected but this is no longer quite clear in view of the relatively much higher doses that radioiodine delivers to the thyroid compared to fallout radiation doses to the gonads.

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Representative HOLIFIELD. We are going to request Dr. Wright Langham to come to the stand for a few minutes. Senator Anderson has some questions as a result of former testimony.

STATEMENT OF DR. WRIGHT LANGHAM,[1] LOS ALAMOS SCIENTIFIC LABORATORY

Chairman ANDERSON. Doctor, I think as a preface, I ought to show you a cartoon which you may not have seen, which shows a great safe with some iron chains around it, entitled "Information on Nuclear

[1] See biographical sketch, p. 1061.

Fallout," a heavily barred door, bundles of keys, and a man standing there saying, "What makes you think we are trying to conceal anything?"

I was guilty of a little article in a magazine last Sunday in the New York Times magazine in which I mentioned the earlier study which you had made along with Mr. Cowan and E. C. Anderson in 1957. I said the report was received by the Atomic Energy Commission on October 21, 1957, and attempts began immediately to have it declassified. The report finally was declassified about 2 week ago—we were having some interesting comments on declassification at that time—and was sent to me by Atomic Energy Commissioner Willard F. Libby with a notation that it was somewhat out of date. I said in this article I understood that Dr. Langham and his colleagues are in the process of preparing an updated version of their study.

It would be interesting to see whether this paper, too, will hibernate for 3 years before it is turned loose.

Just this morning the Joint Committee received a letter from the Atomic Energy Commission stating that it is impossible to declassify your study because it contains material related to the United States-United Kingdom total fission yield. This has a confidential tag on it, but it says if the document is not used, it can be unclassified. It says to make it possible to publish these charts, which are referred to in another study, it was necessary for the Commission to obtain the concurrence of the United Kingdom and of the U.S. intelligence agencies. In granting these concurrences the United Kingdom and the agencies made it clear to the Commission that the charts represented the furthest extent to which they felt it possible to go without jeopardizing information of vital concern to them. Consequently, in reviewing Dr. Wright Langham's revised article, we find it is not possible to declassify it, since taken in conjunction with the charts just described, it will release the United Kingdom contribution to radioactive content of the atmosphere for which we do not have United Kingdom concurrence.

In addition it would also release information concerning the Soviet contribution for which intelligence approval is not available.

I would like to see your article published. I looked at it, and I thought it was fine. Is there any way that you can avoid reference to the charts in question and give us the updated version of your previous study, that study now having been declassified?

Dr. LANGHAM. I think, Senator Anderson, yes, this can be done. The history more or less of this document was, I think about as follows: After the last hearings you may recall that an issue was made over the testimony submitted by Dr. Lapp as regards the rate of production of strontium 90 from nuclear weapons tests. I, knowing certain classified information, knew that Dr. Lapp had, from his sources which were unclassified, overestimated the production of strontium 90. I went back to Los Alamos and proceeded to submit a document for declassification which would give numbers certainly more precise than the ones Dr. Lapp had been able to manage from his unclassified sources.

The document went out according to the dates you have mentioned. I wrote Dr. Libby, and Dr. Libby assured me that it was being considered and it was finally declassified.

Chairman ANDERSON. Yes; but only after some comment by me about classification a couple or 3 weeks ago. It was reposing very safely up until that time. I am not trying to question your statement. As you know, Doctor, I have the greatest respect for you and your work. I should not interrupt. You go ahead with your statement.

Dr. LANGHAM. At least I was notified that the document was declassified. This much I do know. The document did contain information which made it unclassifiable by the local Los Alamos office, and it was not declassifiable according to the classification guide, which meant that it required higher action than could be given locally.

Chairman ANDERSON. Didn't the Los Alamos people think it was perfectly safe to turn it loose in 1957?

Dr. LANGHAM. They did the only thing which they could do, which was to send it to higher authority with the recommendation that they thought it should be declassified. This they did.

Chairman ANDERSON. I am sorry. I did not mean they declassified. They sent it on saying it could be declassified.

Dr. LANGHAM. They said in their opinion it could be and should be declassified.

Chairman ANDERSON. Should be declassified. It got in here in October 1957, and in April of 1959 it was declassified.

Dr. LANGHAM. Yes.

Chairman ANDERSON. At that time I not only requested of them, but had the privilege of visiting you about it, and you informed me that you were preparing an updated version. I awaited that updated version with a great deal of interest, I think the public might have a right to it. I see the statement by the General Advisory Committee to the U.S. Atomic Energy Commission dated May 4, and they make a finding:

> We find that the Atomic Energy Commission has released all significant data to other agencies and to the public—

significant fallout data. I regarded your study as significant fallout data. I am sorry to find that it is tied up and—this letter is not secret when taken away from the document—this last paragraph reads, "The Commission believes that the publication of the three charts"—and those were not in your manuscript?

Dr. LANGHAM. That is right.

Chairman ANDERSON (reading):

> Above provides more information than the Langham article. The Commission does not believe, therefore, that the restriction is based upon the AEC which prevents the declassification of Dr. Langham's article at this time will have any significant effect upon the public's understanding of the fallout problem.

Of course, I think the public has a right to decide whether it needs some help in understanding the fallout problem. The Commission may not. Therefore, I was wondering if there was any way that these charts of yours, which, as I understand, do not refer to the three charts he is referring to, could be reworked by you in another attempt to see if we can get it to the public?

Dr. LANGHAM. They certainly can be reworked. I am sure the conflict here is strictly one of manner of presentation. I believe the three charts that have been released have released more information than would have been released had they adopted declassifying my

article, and keeping their charts classified. It so happens that they could not declassify both of them. They had to decide whether to hold mine classified or to hold the charts which they had prepared. It is a manner of either method of presentation would have been declassifiable, but both together would not. Someone merely had to make the decision to hold mine classified and declassify those, or keep theirs classified and declassify mine. I think what they did is fine in this regard. I am sure the thing has been accomplished which I know I am proud to have accomplished, and which I think you would be, too. A lot of good information has been made available to the public.

Chairman ANDERSON. Yes, I am sure of that. I want a statement by you as to what possibility there was on the additional charts. I take it, then, that you feel it was better to have released these charts than to have released your article if only one could be made public.

Dr. LANGHAM. Yes, I sincerely believe this. I still, of course, have the opportunity of going back now and reworking my paper and using the information which now I know is declassified, and perhaps in this case I will not have to send it all the way to Washington to get it declassified.

Chairman ANDERSON. I appreciate that very much, Doctor. As you know, I have quoted you in season and out of season for a long time, and visited you and talked these things over with you, and have the greatest respect for the work you have done. I have a telegram which you may decline to comment on if you wish, but it comes from a reputable publication, and got into my office only a few minutes ago:

> Dr. Wright Langham testified at yesterday's hearing that with no further tests average strontium 90 bone concentration of the world's population will reach 7 or 8 strontium units. Since infants and young children accumulate strontium 90 at nearly 10 times the rate that adults do, I respectfully urge that you question Dr. Langham as to the bone level that may be expected in children from zero to 24 years of age.

Would you want to comment on that? I realize you have no obligation to.

Dr. LANGHAM. Yes, of course. The population weighted average level of 7 micromicrocuries per gram of calcium is a level projected to equilibrium conditions, which means that this is the maximum that children may be expected to reach or anyone for that matter, who comes into equilibrium with the biospheric contamination. It is merely that those of us who formed most of our bones before this level of fallout occurred will never reach equilibrium. We will not have time. While children will approach the equilibrium level. So the value that is given is the maximum average equilibrium level of children or anyone who happens to come into equilibrium with the biosphere. It, of course, will be children that will come closest to equilibrium and reach equilibrium first.

Representative DURHAM. Doctor, would you say what a micromicrocurie means?

Dr. LANGHAM. A micromicrocurie per gram of calcium means that each gram of calcium in the bones of an individual who has one micromicrocurie will be receiving only two strontium 90 beta ray disintegrations per minute. This is an extremely small amount.

Representative DURHAM. Does it mean a millionth of a millionth?

Dr. LANGHAM. If you wish to express it in curies, it is a millionth of a millionth of a curie.

Chairman ANDERSON. I want to explain, Mr. Chairman, I am not trying to be critical of the way you have run this hearing, because I think it is very fine. We all get these comments and requests for information. I have been trying to say rather than argue about a residence half life of 2 years or 7 years, I think the Joint Committee may be asking one question of this particular group—Do we have enough data at this time about fallout, its nature, and its effects, on which to base a sound policy for living with present contamination and for making decisions about further contamination? Is the circle of the probable error so small as to permit us to lay out a program for the future or is it so large as to make policy decisions dangerous until future data are available?

I want to congratulate the witnesses that have thus far appeared. As you know, I have been engaged in other activities in other parts of the Capitol. I nonetheless appreciate the testimony that has been given.

I say to you, Dr. Langham, I did not call you back to try to pick any quarrel with you on the fact that this is not released. I only wanted to find out for myself so that I might explain to others why your material has not been released. I think the answer is a very satisfactory one, and I appreciate it.

Dr. LANGHAM. Thank you.

Representative HOLIFIELD. Thank you, Senator. Thank you, Dr. Langham.

The question the Senator proposed will be the subject of part of the panel discussion this afternoon. We will have a departure from the printed outline for a few minutes. Senator Pastore brought into the early morning session the article which appeared in a local newspaper, and the Chair made some comments on the article and the author of the article, my friend, Ed Gamarekian, has requested to make a statement regarding the article. So he will be given that courtesy in all fairness to him at the beginning of the afternnon session. The subject involved in his article will also be a subject of questions to the panel this afternoon. Following Mr. Gamarekian's statement we will have genetics effects of radiation, Dr. Crow and Dr. Russell, of Oak Ridge National Laboratory, as the first two witnesses on the schedule.

The committee is adjourned.

(Thereupon at 12:55 p.m., a recess was taken until 2:00 p.m., the same day.)

AFTERNOON SESSION

Representative HOLIFIELD. The committee will be in order.

This afternoon we will briefly depart from our schedule to hear a statement from Mr. Edward Gamarekian. Mr. Gamarekian has asked to be heard regarding discussions this morning on his article dealing with radioactive content of bread in New York State.

While we are departing from our regular list of witnesses at this time I thought in fairness to Mr. Gamarekian who is a very fine reporter and knows a great deal about atomic energy that he should

be given an opportunity to speak at this time. The subcommittee is glad to approve your request.

(The article by Edward Gamarekian referred to above follows:)

[Article in the Washington Post and Times Herald, Thursday, May 7, 1959]

AEC Reveals New York Bread Exceeded Strontium 90 Limit

(By Edward Gamarekian, staff reporter)

Atomic Energy Commission figures inserted in the record of a congressional hearing yesterday disclosed that the white bread sold in a New York City supermarket on February 19 had a radioactive strontium 90 content equal to four times the maximum "permissible" limit.

Whole wheat flour from an Illinois mill reached levels 6 times the limit when compared in one of the usual ways and 18 times the limit when compared a second way, the figures showed.

When the AEC published the figures in a report on milk and foods that was submitted to the Joint Congressional Subcommittee on Radiation it did not make any comparisons with the maximum "permissible" limit for foods. The comparison is based on the new limits adopted by the International Commission on Radiological Protection. AEC representatives have indicated during the past 2 days of hearings that they have accepted the new limits, at least unofficially.

EVEN OVER OLD LIMITS

The U.S. Committee on Radiation Protection has not yet issued newly revised "permissible" limits for the general population. Use of the old limits, which are 2.4 times as high, would still put the New York bread and Illinois flour over the "permissible" level.

An AEC spokesman said last night that the white bread samples tested may have become mixed up with some whole wheat bread samples from the same baker, since the whole wheat bread showed strontium 90 levels only one-fourteenth as high—a result just the opposite of that expected since milling removes the strontium 90 deposited on the wheat by rainfall. New samples from the same baker are being checked along with other samples from other parts of the country, he said.

The results will not be available for at least 2 months. The sources of the wheat for the New York bread and the Illinois whole wheat flour are not known, he stated.

Several weeks ago, Minnesota wheat samples from the 1956–58 period showed comparable levels of strontium 90.

Another AEC report inserted in the record yesterday showed that in 1957, the strontium 90 level in plants grown in several parts of the United States had exceeded the "permissible" limit for foods. Although the plants were not the kind eaten by humans, they indicated what the strontium 90 level might be in edible varieties.

SAMPLES FROM FOUR STATES

"Permissible" limits were exceeded in samples collected from Georgia, New Jersey, New York and Utah.

None of this information was included in the oral testimony yesterday. All of it was in printed reports submitted for the record. The Government scientists who testified kept to average figures which were low and assured the Congressmen there was no cause for alarm.

Dr. C. L. Comar of Cornell University described a number of ways in which the strontium 90 hazard might be reduced but told the committee there was no need to take such action at the present time.

Several times during yesterday's hearing, Committee Chairman Chet Holifield (Democrat, California), asked the scientists who were talking in terms of national and global averages to describe the situation in the "hot spots"—the sections of the country in which the radioactivity is well above average. He was promised each time that the "hot spots" would be discussed at a later time.

During the noon recess, however, one of the scientists stated privately that the effects in "hot spots" could be "five times the average and even more."

Senator Clinton P. Anderson, chairman of the Joint Atomic Energy Committee, took sharp issue at the morning session with Merrill Eisenbud, manager of

the AEC's New York operations office. Eisenbud had just stated that the faster rate of fallout now taking place had the advantage of bringing the atomic debris out of the atmosphere sooner.

Anderson interrupted: "When you were here at the fallout hearings 2 years ago, you told us how good it was that the atomic debris was not coming down so fast, since this gave it time to lose some of its radioactivity.

"Now you tell us, how good it is that it's coming down so fast, since this means there is less left in the stratosphere.

"No matter what happens, you keep telling us how good it is. Now which is it?"

Eisenbud had no reply. There was an embarrassing pause and the hearing moved on.

Would you please proceed?

STATEMENT OF EDWARD GAMAREKIAN, REPORTER, THE WASHINGTON POST

Mr. GAMAREKIAN. I appreciate this opportunity to testify before you because I know it is highly irregular for a member of the press to appear.

This morning some statements were made about an article which appeared in this morning's paper, which I wrote, and the impression was left that the article was irresponsible, and this is why I asked to have a few minutes to testify.

I submit, Mr. Chairman, that this article was not irresponsible, that these are the facts based on figures which are not my figures but which are figures which appeared in the Atomic Energy Commission's report presented to this committee yesterday.

I would like to begin by reading the lead paragraph of the article because I think it sums up the item in question.

Atomic Energy Commission figures inserted in the record of the congressional hearing yesterday disclosed that the white bread sold in a New York supermarket on February 19 had a radioactive strontium 90 content equal to four times the maximum permissible limit. Whole wheat flour from an Illinois mill equaled six times the limit when compared a second way the figures told.

Mr. Chairman, because it is difficult to deal with figures with the general public we did not give the actual numbers. So to present this I would now like to give the figure for the maximum permissible limits. This figure has been established by the National and International Committees on Radiation Protection and is established for water. Until a few weeks ago the figures for water were 80 micromicrocuries per liter or per kilogram.

The Public Health Service has adopted this figure for milk. The Atomic Energy Commission has adopted this figure for food—the figure of 80 micromicrocuries per kilogram. This figure is based on a total diet.

It is based on the fact that we are not only getting strontium 90 in milk but getting it in all other foods. The limit of 80 is based on the total average intake. Milk is used for monitoring because it is a key food in the diet. We know that when strontium 90 reaches a value in milk of, say, 10, that it is 7 times as much in plant foods because the cow discriminates against strontium by a factor of 7. From milk data, therefore, one can estimate the strontium 90 in the total diet.

So the figure of 80 is the maximum permissible concentration for all foods. This is the base.

For the past few years the national and international committees have been revising these figures. The international committee came out recently—actually their report was adopted last September—with new figures, which dropped the limits to 33.

The national committee has not come out with its figures yet, but said in a recent release that it considers the international committee's figures to be sound. Dr. Dunham in his testimony the first day referred to these figures, so I assume that the Atomic Energy Commission considers them sound also. So we start with the figure of 33 and the units are micromicrocuries per kilogram.

Here is the report that was submitted yesterday. It is entitled, "Data on Strontium 90 Content of Milk and Other Foods During 1958." In table 5, it gives figures for 12 loaves of white bread purchased in a New York supermarket and gives figures of 126 micromicrocuries per kilogram.

That is why I wrote this morning, Mr. Chairman, that bread is four times the maximum permissible concentration. One hundred and twenty-six is 4 times 33, approximately. You notice in the figures above for whole wheat a value of 198 micromicrocuries per kilogram. This is responsible for my statement that in Illinois wheat the levels had reached six times the maximum permissible concentration.

There is a second way of calculating the relative level, and I don't want to go into details here, but if you use that method, then the 598 in the table is 18 times the permissible limit. There are two ways of calculating it and the scientists may go into that here. It would take some time to explain.

This, Mr. Chairman, explains my story.

The question has been raised as to what this means in terms of the total diet. Let me say first that four times the maximum permissible concentration means not that you have to eat 3 pounds of bread a day, as was suggested, but that you have to eat only half a pound of bread a day.

Mr. Chairman, we are eating—on the average—a quarter of a pound bread a day in this country according to the figures of the Department of Agriculture.

I would like to point out to the committee again that these figures are not mine. These are numbers which came from the Atomic Energy Commission, the Public Health Service and other agencies. I am only a newspaperman, although I have had some 13 years of experience in research and development.

I would like to point out further that these maximum permissible concentrations are based on a single isotope, strontium 90. They do not take into consideration the potassium that is also getting into the bone, the radium present in the background which is also getting into the bone, and the raw uranium and plutonium which are also getting into the bone.

I have talked with the scientists involved in this problem at length about this and they say there is just no way of working out a limit for one substance based on the intake of all the various kinds. This has to be figured on each individual case. So the situation is more complicated, and even worse than we indicate in the story.

Now, just a few figures to indicate that these are not isolated cases—that this is not happening to bread and wheat alone but is happening to other foods also. In this document submitted yesterday there is a table called, "The HASL Pasture Survey," which gives the strontium 90 content of pasture foods. These are not foods eaten by humans, but we assume they apply to foods which humans eat also. These figures are given in slightly different units.

They are put on a per gram of calcium basis. You will notice the figures are quite high. If we went into the detail, we could show they are well above the maximum permissible limit for plant foods in Georgia, New Jersey, New York, and Utah. If you check some of the figures from other States, you will find they are over the limit also.

This is not just happening to bread, Mr. Chairman. This is happening to vegetables and other foods.

The report of the Atomic Energy Commission last year showed that the maximum permissible concentrations had been exceeded not only in wheat but in rice and oats. The report showed it was not only happening in this country but in Western Europe and Asia.

The final item I wish to bring up here, Mr. Chairman, is what this means in terms of a lifetime. It is true that the maximum permissible concentrations are based on a lifetime dose, but I think we have to be concerned about what is happening at the present time. We are not sure that the tests are going to be discontinued. These figures must be presented to the public now. Seventy years from now it may be too late.

One more item, Mr. Chairman. Again I am going to quote from an Atomic Energy Commission publication. That is on the question of what a maximum permissible concentration means in terms of the incidence of cancer and other things. They say that the new maximum permissible concentrations for radioactive substances that produce genetic effects would give rise ultimately to 500,000 defects per generation in the United States.

Dr. Dunham gave some figures on Monday for somatic effects which indicate that if we reached the maximum permissible concentration for strontium 90 alone there might be as many as 660 cases of leukemia per year in this country.

That is all, Mr. Chairman.

Representative HOLIFIELD. Thank you very much for your statement. The Chair wishes to assure you that there was no intent to reflect upon your own sincerity and honesty in your professional capacity as a reporter. The Chair has read your articles many times with a great deal of interest. The content of the article will be discussed in this afternoon's panel.

So the Chair will not at this time indulge in his desire to question you. Thank you very much.

Dr. James Crow, of the University of Wisconsin, and Dr. W. L. Russell, of the Oak Ridge National Laboratory, will testify on the genetic effects of radiation.

Gentlemen, as you know we are running a little behind time through no fault of your own and we certainly want to get the benefit of your testimony this afternoon in as expeditious a manner as possible without omitting important testimony.

We want to be sure that we get the content of your thinking on these subjects. This is a very important subject. Would you like to proceed?

STATEMENT OF DR. JAMES CROW[1] OF THE UNIVERSITY OF WISCONSIN

Dr. CROW. Yes, thank you, Mr. Chairman. I will try to keep it brief.

Two years ago a group of geneticists, of which I was one, appeared before this committee to discuss the genetic effects of radiations. The general conclusions stated at that time were as follows:

1. Radiation causes the production of mutations—mutations being heritable changes in the genetic material. The effect of radiation seems to be an increase in the rate of occurrence of effects that are already taking place rather than production of totally new effects. This means that a genetic effect, such as an abnormality or a disease, caused by radiation is not distinguishable from an effect arising from other causes.

2. The great majority of mutations whose effects are large enough to be detected are harmful to a greater or lesser extent, whether they occur spontaneously or are radiation induced. This is experimentally demonstrable; it is also to be expected on theoretical grounds.

3. There does not seem to be any threshold dose below which no effect is produced; no dose, however small, is free of all risk of mutation.

4. The total genetic damage on future generations is largely independent of the distribution of the dose in the population. That is, the same total consequences are predicted if 10 percent of the population get 5 roentgens as if 50 percent get 1 roentgen.

5. The effect of the radiation, at the low doses we are concerned with here, is simply proportional to the total dose received by the reproductive cells.

6. The genetic effect of a given amount of radiation received by a person is the same whether that amount is given quickly or slowly over a long period of time. This conclusion requires revision, as I shall discuss shortly.

7. Some of the genetic defects will be major catastrophes to the individual experiencing them, such things as body malformations, feeble mindedness, and serious hereditary disease, both physical and mental. A much larger number of mutants are likely to be those causing minor impairments of body functions so as to lead to a slightly decreased life expectancy and enhanced susceptibility to diseases and environmental effects.

It is difficult to make a comparison between these kinds of effects in terms of their impact on the happiness and general well-being of mankind in the future. But it must be pointed out that mutations causing a small effect, because they are less likely to lead to increased

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early death and sterility, will persist in the population correspondingly longer.

Since so many more persons are affected the total impact of mutations with small effect is likely to be greater than might at first be thought.

8. Quantitative estimates of the genetic effects of radiation on man are not possible from human data except within extremely broad limits. Extrapolation from mice and other experimental organisms is at best uncertain. Such estimates were given at the previous hearings, where their tentative nature and the possibility of their gross inaccuracy were emphasized.

The general conclusion was that the amount of genetic damage to be felt in future generations as a consequence of bomb testing carried out thus far is a very tiny fraction of the total of human misery. Yet the number of persons exposed and the fuure world population is so large that the actual number of genetic casualties may be hundreds or thousands or more.

How well do these conclusions hold up 2 years later?

The general conclusions are the same. Genetic research throughout the world in the meantime has added to our basic knowledge, given considerably more data, and in general has reinforced the conclusions of 2 years ago.

Probably the most important experimental results, from the standpoint of changing any previous conclusion, are Dr. Russell's experiments on dose intensity in mice at Oak Ridge. Previously it had been thought that a given amount of radiation caused the same effect whether given at a high or low rat. This has been thoroughly tested in experiments on sperm irradiation in fruitflies. Radiation of high or low intensity gave the same effect as long as the same total amount was given.

However, Dr. Russell's experiments with mice give different results; he finds that low intensity radiation over a long time has less effect than the same total amount given in a short time.

Russell's mice were irradiated in the stages where germ cells were immature, whereas the Drosophila work was done mainly by irradiating mature sperm cells. Since the mouse study is on stages comparable to those where the maximum human exposure occurs, I believe the Russell results are very significant. Since Dr. Russell is here, I shall not discuss his experiment in detail.

Dr. Russell's data suggest that the effects of chronic irradiation may be only about one-fourth as much as those from acute doses. Since the numerical estimates made previously utilized data from acute radiation, these should probably be reduced proportionately.

Let me reemphasize that it is still true, as it was 2 years ago, that quantitative estimates are extremely uncertain and should be used only as rough indication of the order of magnitude of effect to be expected on the basis of present knowledge.

In summary, I should like to repeat what I said two years ago, and which I believe is still appropriate: With present levels of fallout the amount of genetic damage from this source is such as to cause an extremely minute fraction of the total human death, disease and misery. The effect is almost certain not to be detectable by any foreseeable measurement.

Yet, the number of persons exposed to fallout is as large as the world population, and, therefore, it is likey that tens or hundreds of thousands or more persons will be diseased, or deformed, or die prematurely, or be otherwise impaired if the present rates continue.

Representative HOLIFIELD. If the present rate continues?

Dr. CROW. That is correct.

Representative HOLIFIELD. The Chair will have to adjourn the subcommittee for about 20 minutes for a rollcall vote in the House and if you gentlemen will wait we will be back.

(A recess was taken.)

Representative HOLIFIELD. The committee will be in order.

You may proceed.

STATEMENT OF DR. W. L. RUSSELL,[1] OAK RIDGE NATIONAL LABORATORY

Dr. RUSSELL. Mr. Chairman, at the time of the 1957 hearings on fallout, it was considered to be one of the basic tenets of radiation genetics that variation in radiation intensity (dose rate) does not affect mutation frequency. Thus, it was believed that the amount of genetic damage induced by a given total dose of radiation would be the same, regardless of whether that dose was delivered as a single burst or spread out over a long period of time.

This view was accepted in the 1956 reports of the U.S. National Academy of Sciences and the British Medical Research Council, and in the 1958 report of the United Nations. However, the experimental results upon which this conclusion was based had been obtained only from certain cell stages, particularly the mature male reproductive cells (spermatozoa) of fruitflies. As we have emphasized many times before, most of the radiation dose causing genetic hazards in man will, in the male, be accumulated not in spermatozoa, but in the immature reproductive cells (spermatogonia). In the female, the cell of corresponding importance is the oocyte. It, therefore, seemed to us to be of both practical and fundamental importance to question whether mutation frequencies observed following irradiation of these cell stages would also prove to be independent of radiation dose rate. In my testimony at the 1957 hearings on fallout, I stated that we were testing these questions by making "a comparison of the results from long-continued low-level irradiation with those from single dose acute radiation" of mice.

The results of the new test, and of several additional experiments growing out of it, are quite startling. It was found that the immature reproductive cells do, indeed, react differently from the mature ones. Thus, the mutation frequencies obtained by irradiation of the immature stages (the important ones in human hazards) were found to

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be markedly dependent on dose rate. The details of these experiments have been submitted for the record.

To summarize the results from several experiments on the immature male reproductive cells (spermatogonia), we found that the mutation frequency obtained from a dose of radiation spread out over a few weeks was only about one-fourth of that produced when the same dose of radiation was delivered in a few minutes. A similar difference in the effects of chronic and acute irradiation was also observed for the cell stage of corresponding importance in the female (the oocyte), although the ratio of effects has not yet been determined with as much precision in this sex.

When the mature reproductive cells in the male (spermatozoa) were tested in mice, the mutation frequency was found to be independent of radiation dose rate, just as it had been in the earlier work of others on fruitfly spermatozoa. Thus, as was pointed out in our first publication of these results, there is no discrepancy between mouse and fruitfly, and the explanation for the new phenomenon of intensity dependence resides in stage of reproductive cell development.

In other words, the new experiments have clearly indicated that different cell stages have basically different types of mutation process: the mutation process in spermatozoa is independent of dose rate, while that in spermatogonia and oocytes is affected by it. Thus, although the findings of others on spermatozoa have been confirmed, it has been shown that it was wrong to generalize from these findings. This was particularly misleading, insofar as practical considerations in man are concerned, because the generalization was erroneously applied to those very cell stages (the spermatogonia and oocytes) which are important in human hazards.

The finding that mutation frequency in spermatogonia and oocytes is affected by dose rate of radiation has an important bearing on questions concerning the nature of the mutation process as well as on immediate practical problems. Some of these will be briefly discussed.

The most important immediate practical conclusion is that radiation sources that deliver radiation slowly over a relatively long period of time may have considerably less genetic effect than had been expected. Such sources include background radiation, fallout, and many present and future industrial uses of radiation.

Although it is something of a relief to find that radiation delivered at a low dose rate produces fewer mutations than had been thought, it must not be forgotten that these mutation frequencies are still appreciable.

It will undoubtedly be tempting to some people to conclude that the new data on dose-rate dependence on mutation frequency provide evidence of a threshold dose for mutation production. We do not feel that it is safe to draw such a conclusion. The argument is presented in detail in the material submitted for the record.

A related problem is the question of whether the mutation frequency obtained with a given total dose would drop to a still lower level if the radiation were spread out even thinner; that is, over a still longer period of time than in the experiments so far completed. Only further experiments will give us the answer. However, it should be noted now that the results already obtained involve a dose-rate

difference of 10,000-fold (100,000-fold in one part of the experiment). In other words, the acute radiation was given at 90 r per minute, the chronic at 90 r per week, and, since there are about 10,000 minutes in a week, this is a 10,000-fold difference.

The fact that this gave only a fourfold difference in mutation frequency is perhaps some indication that a further decrease in dose rate would be unlikely to give a further drop in mutation frequency.

Representative HOLIFIELD. This factor of reduction of three-fourths in the same amount of radiation spread over a long period would be important in the event of population exposure from a nuclear bomb. If they had shelter and could withstand the immediate heavy radiation it would mean that they could absorb over a period of time smaller doses of radiation without having the same deleterious effect as if they received it in one dose at the time of the bomb explosion?

Dr. RUSSELL. Yes. I think, apart from the initial ionizing radiation from the blasts, subsequent radiation from the heavy fallout in the area of the bomb would be down at a dose-rate level which would probably match the lower dose rates used in our experiments.

So one would expect a lower hazard from any dose received in this way.

Representative HOLIFIELD. In the setting of maximum permissible doses, has this factor been taken into consideration?

Dr. RUSSELL. The question of permissible doses is going to be reconsidered by the National Academy Committee next week. Perhaps the panel will have some comments to make on that this afternoon.

Representative HOLIFIELD. I understand that they will.

Dr. RUSSELL. Yes.

The finding of a dose-rate dependence has raised important new questions. Fortunately, many of these can be answered by further experimental research, and some of them are, in fact, already being investigated in our laboratory. One obviously important piece of information that is needed now is the range of dose rates over which the mutation frequency changes from a low response to a high response.

Another important area for investigation is the mutational response to fractionated intense radiation, that is, radiation given in small bursts spaced at intervals. Of course the number of ways in which a given dose can be fractionated (magnitude of individual fractions and length of interval between fractions) is infinite. It seems almost certain that the mutational response for certain systems of fractionation will resemble that for acute, while for other systems it will resemble that for chronic irradiation. The important information that is needed is where this transition occurs. This should yield clues as to the nature of the dose-rate effect. Results from fractionation experiments will also be valuable from an immediate practical point of view, since most of the radiation arising from medical practice, which comprises such a large part of the total radiation to which we are exposed, falls into the category of fractionated exposure.

I have discussed our experiments on the effect of dose rate on mutation frequency at some length because of the startling nature of the results. It should be pointed out that these experiments represent only a portion of our work. Time does not permit the discussion of many other important investigations, for example, on the genetic na-

ture of the mutations, on the lethal and other effects of specific mutations, on the overall effect of induced mutations on various properties of the population, et cetera. A list of publications that have appeared since the 1957 hearings is appended for the record to give reference to some of these findings.

However, attention should perhaps be briefly drawn to one aspect of these additional findings. In my testimony at the 1957 hearings on fallout I mentioned that we were working on a comparison between the mutation rates in females and males. This work has now yielded the first extensive information on radiation-induced mutation rates in female mice. Most of our earlier work was restricted to the more easily performed experiments on males. In one earlier experiment on females, several years ago, we had excluded the possibility of the mutation rate in females being very much higher than that in males, but a more precise estimate was desirable. There have been some statements that the mutation rate in females may be much lower than that in males. The new data do not support these statements. In fact, so far as acute irradiation is concerned, our present estimate of mutation rate in oocytes is somewhat higher, although not greatly so, than that in spermatogonia. A publication listing some of the detailed findings in females has been submitted for the record.

That concludes the statement.

Representative HOLIFIELD. Thank you very much, Dr. Russell.

Dr. Russell, I think the work that you have done on the radiation of mice at Oak Ridge has probably been the most important work in the field of genetic diagnosis or the diagnosis of effect of radiation on mice. Could you very briefly give us the basic facts of that experiment?

Dr. RUSSELL. You mean a general summary?

Representative HOLIFIELD. I mean how many mice, how much you expose them to for how long a time and how many descendants, and so forth.

Dr. RUSSELL. In this particular experiment here?

Representative HOLIFIELD. Yes.

Dr. RUSSELL. In the data as published, which have been extended somewhat since the publication, we have three different doses for the chronic irradiation experiment and a control group. The total number of mice examined was over 100,000 in the controls and close to the same number in the experimental group. We have about a 50-percent increase on those numbers at the present time.

In the acute irradiation experiment, we had a somewhat larger total number which we collected before the chronic irradiation experiments were started. These were large-scale experiments.

Representative HOLIFIELD. You feel that those numbers are enough to give you meaningful statistics?

Dr. RUSSELL. The calculated comparison between the low level and the high intensity radiation gives us a highly significant difference by statistical tests. There seems to be no question that there is a real difference here.

The point estimate of the value of this difference is about four. This, however, has to be taken with some qualifications. It may be as low as two or perhaps as high as 12 or something of this sort.

Representative HOLIFIELD. What was the intensity that you subjected the mice?

Dr. RUSSELL. The acute irradiation was about 90 roentgens per minute, a very short time of irradiation.

Representative HOLIFIELD. Ninety roentgens per minute?

Dr. RUSSELL. The total doses were 300, 600, and 1,000 in the three different experiments.

Representative HOLIFIELD. On the same mice?

Dr. RUSSELL. Different groups of mice of the same strain.

Representative HOLIFIELD. In no case was this enough to kill the mice?

Dr. RUSSELL. The thousand roentgen dose would have been enough to kill the mice if this had been given as a whole body radiation. Here we restricted exposure to the posterior third of the body including the gonad region. In one of the other experiments at 300 roentgens we divided the mice between one group which received whole body irradiation and another group which received the same kind of partial body irradiation as was given to the thousand roentgen group, and we found no difference.

Representative HOLIFIELD. The question has come up in the hearings as to the value of extrapolation from the experiments on mice to man. Would you comment on your belief on how accurate those extrapolations would be?

Dr. RUSSELL. I don't think I would change much in my comment on this from what I said in the 1957 hearings which essentially is this: Extrapolation of principles, such as a lower mutation rate from chronic irradiation than from acute irradiation, I think could be applied to man with very little reservation.

I think this principle is most likely to be applicable to the human species. In fact I think this will apply to the fruit fly when this has been tested. Other things of this sort, such as the question of the relation between mutation rate and dose, things that involve general principles, I think can be applied very well from mouse to man.

However, with regard to the actual mutation rate itself, whether man will be equally sensitive, less sensitive, or more sensitive than the mouse, we don't really have any good information. The Hiroshima and Nagasaki studies are not out of line with the mouse information. Some people point out that so little has been actually found of a genetic nature from the Hiroshima-Nagasaki studies that this perhaps indicates some difference between mouse and man.

I think the authors of the publication on the Hiroshima-Nagasaki study do not believe this. They maintain, although their findings are meager, that the human species could very well be just as sensitive as the mouse. I think the data do not even exclude the possibility that man could be more sensitive than the mouse.

That is about the only set of data I can think of that permits any comparison between man and mouse. However, in the absence of more information, it certainly would be safer, in my view, to take the mouse data rather than data from the fruit fly or other organisms more distantly related to man than the mouse is.

Representative HOLIFIELD. Could you comment on the number of roentgens—I guess I would want to say the intensity of the dose—

which would be necessary for you to conduct an experiment which would give you two things:

One, observable effects upon the descendants of mice, and second, meaningful statistics.

Dr. RUSSELL. If you mean the overall effect in the population.

Representative HOLIFIELD. I might apply that also to the genetic and somatic.

Dr. RUSSELL. We are conducting experiments along the line of trying to measure the overall damage in a population of mice whose parents have been exposed, and other laboratories are doing similar work. This would take into account such effects as longevity, effects on the reproductive capacity of the population, the incidence of disease, and so on. These would fall into what we call the category of empirical experiments, just measuring the damage as expressed in the population instead of trying to measure mutation rates directly. I would say this is one category of our experiments. The other category which I have been reporting on here, is to try to evaluate the basic principles of radiation genetics, comparisons of various factors with each other, rather than a measure of the total damage. The measure of total damage is very difficult. For example, in experiments on longevity, it involves setting up such large experiments that the many other factors affecting longevity are canceled out and you can pinpoint the result as being due to radiation. It is not suitable to use such a type of end-measure as longevity in comparing acute and chronic radiation effects. It is much better, in trying to measure acute and chronic radiation effects, to use a more definite end-point such as the mutation rates of the type we have used.

Representative HOLIFIELD. Let me ask you this question: If you exposed parent mice to 10 roentgens, would you be able to detect somatic effects in their offspring?

Dr. RUSSELL. I think this would be extremely difficult at the present time with any kind of practical experiment that we can envision. Our lowest dose in the chronic irradiation experiment was 86 roentgens, and at this point, even with over 50,000 offspring, we are not able to tell for sure that the mutation frequency is higher than the control frequency or even that it is lower than in the acute radiation experiment. We arrived at the difference between chronic and acute at higher total doses. I think it would be very difficult and would require a tremendous experiment to determine the effect of 10 roentgens. We are trying to answer this basic question of a threshold in another way.

Representative HOLIFIELD. This is the basic question, as to whether a low rate dose actually causes somatic and genetic effects. This involves, as we both know, the threshold, and this is where the big field of argument lies in these figures that were given us.

Dr. RUSSELL. Yes. Before these new findings on the effect of dose rate it was thought that the genetic problem did not involve the question of a threshold. Now that we have found that there is a dose rate effect, this question comes up with more force.

Representative HOLIFIELD. I don't quite understand you on that.

Dr. RUSSELL. I think at the time of the last hearings, as Dr. Crow mentioned, it was generally assumed that the relation between mutation rate and dose was independent of the rate at which this dose

is given, that is, whether it is given as a single dose at one time or spread out. Our recent findings do not agree with this. This raises the question much more forcefully as to whether there might be a threshold dose for genetic effects.

Representative HOLIFIELD. Does it seem to indicate from the standpoint of low level radiation that there is less likelihood that there is a threshold?

Dr. RUSSELL. I think this raises the possibility of there being a threshold for genetic effects as there is presumably for some somatic effects. I do not subscribe to this view myself, but I think the possibility is raised. We need further experiments to go after this. One type of experiment of the sort that you mentioned is to go down to very low doses. We think there is another way of going at this, and this is to try still another experiment at high doses, but reducing the dose rate even more. We have one such experiment started some time ago which is yielding material at the present time.

Representative HOLIFIELD. How low are you going now?

Dr. RUSSELL. We have dropped down to one-tenth of the intensity that we use in this chronic experiment.

Representative HOLIFIELD. Which would be about 8 or 9 roentgens.

Dr. RUSSELL. Nine roentgens a week.

Representative HOLIFIELD. Nine roentgens a week?

Dr. RUSSELL. Instead of 90 per week.

Representative HOLIFIELD. You use that for 10 weeks?

Dr. RUSSELL. Actually we have the experiment going for a total dose of 300. That is over 30 weeks. The mice will still reproduce for a long enough period after this so that this is a feasible experiment.

Representative HOLIFIELD. As a layman it seems to indicate to me that if you are having difficulty in proving genetic effects with, say, 10 roentgens or 9 roentgens, and tracing, that when we compare it to the bomb radiation or one-tenth of the natural background radiation or seven-tenths of 1 roentgen cumulative effect, that we must take the position if that is causing mutations that it is impossible to prove. It is an estimate on prudent judgment and an extension of the line into the area of the unknown rather than the capacity to prove that that low level is causing or even natural background radiation causes mutations.

I am not trying to influence the facts, but I am trying to interpret them in my mind.

Dr. RUSSELL. Yes; if I might use the blackboard a minute, I could try to make this clear as to what our experiment might mean.

Representative HOLIFIELD. Yes.

Dr. RUSSELL. At the time of the last hearings we were thinking genetic effects would be like this; that is, linearly related to dose, regardless of the dose rate. In other words, with a total dose of 300 roentgens we would get a certain mutation rate regardless of whether this 300 was given as a single dose in a few minutes or spread out over several weeks. We now find that the mutation rate at this dose given at 90 r per week, drops down to a quarter of this figure.

Representative HOLIFIELD. If it is given at 90 roentgens a week it would be one-quarter the effect of the one shot.

Dr. RUSSELL. Yes; our next experiment is to reduce this dose rate to even a lower level, one-tenth of this dose rate, still keeping the total

does the same and then see what happens. If the mutation frequency drops down still more, then we think this would increase the evidence that there is a threshold response for genetic effects. If the mutation frequency stays up, in spite of the further reduction in dose rate, then we will think there is no threshold dose and that the response at this low level is still a linear response, although at a lower slope than with acute irradiation.

Representative HOLIFIELD. I notice you draw the line from point zero over. Would there be a break in that line as we have been told before? That is, an area in which there would be no detection of low level radiation mutations.

Dr. RUSSELL. I think if we continue to get the same point here at this total dose, then we would argue probably that the slope is linear. If we got further reduction with this lower intensity then this is some evidence for a threshold type of curve. If we had a further drop in mutation frequency with a further reduction in dose rate, then I think this would argue for a threshold response for genetic effects. If we do not, then I think it will argue for a linear response at this level. Although we can't go down and do an experiment with 10 roentgens even with a tremendous increase in our facilities, we think we can get at least some idea on this threshold question by this type of experiment.

Representative HOLIFIELD. When the field of unknown is so great and when your lack of statistical background is so little, how can these predictions as to the numbers of cases of leukemia to the whole world population be sustained?

Dr. RUSSELL. I think the leukemia estimates were direct effects on the individuals concerned rather than the genetic effects. I was talking solely about the genetic response here.

Representative HOLIFIELD. Yes; I know you were.

Dr. RUSSELL. I was discussing the threshold question for genetic effects.

Representative HOLIFIELD. The point I am getting at is this: We have, for instance, a figure that there would be 980,000 cases of leukemia in the world's population as a result of bomb tests.

Dr. RUSSELL. Yes.

Representative HOLIFIELD. The point I am raising is this: Has any scientist at this time the right to state that as an established fact?

Dr. RUSSELL. I would personally think no, but this is outside my field of competence.

Representative HOLIFIELD. Dr. Crow, what would you say to that? I don't know whether the figure is right or not.

Dr. CROW. Yes.

Representative HOLIFIELD. Is it possible for any scientists today to say accurately how many cases of leukemia or anemia there would be in the world's population as the result of bomb testing?

Dr. CROW. I think it is not possible.

Representative HOLIFIELD. It is what?

Dr. CROW. I think it is not possible to state with accuracy.

Representative HOLIFIELD. Then let me ask you another question: Can they state it on good judgment or prudent judgment that they believe that there is a certain amount? Is there a uniformity of belief as to the numbers? We hear these numbers thrown about in such different numbers by different scientists that we are trying to seek some kind of evaluation of them. As a matter of fact, they are scaring

a lot of people. If this possibility is there, regardless of the fact that we can't prove it, we would like to have some testimony on that.

Dr. CROW. I think what I would want to say, Mr. Holifield, is that there are reasonable assumptions that one can make that lead to predictable conclusions—numerical conclusions. These assumptions have some evidence in favor of them. There is considerable doubt about them, however. One can make estimates on the basis of such an assumption. This does not mean that the person who makes the estimate is being irresponsible. He is making it on one particular basis. I think it should be understood that it is made on a very tenuous basis. May I say one other thing here.

When you say leukemia I think you automatically imply somatic and not genetic effects. I think both Dr. Russell and I feel more competent to discuss genetic effects rather than somatic.

Representative HOLIFIELD. Maybe I can change it to the genetic effect and ask you to comment on that.

Dr. CROW. I would be glad to do so because 2 years ago I did present such a set of estimates based on genetic assumptions.

Representative HOLIFIELD. In fact, the committee asked you for them.

Dr. CROW. That is right.

Representative HOLIFIELD. We are not criticizing you because you did but we are trying to seek the meaning of it.

Dr. CROW. I had the belief then and still hold that a poor estimate is better than none at all. Having said this, it seems to me that the assumption that was used at that time of complete dose independent and complete linearity is not as strong an assumption now as it was then because of the results that Dr. Russell has just described. I would like to go on and say that one should not from this conclude that Dr. Russell's low dose results prove the existence of a threshold. The data taken at face value are consistent either way, I think it would be fair to say.

Representative HOLIFIELD. Do you agree with that, Dr. Russell?

Dr. RUSSELL. Yes. In fact, as I said in my statement I am inclined to think that they do not indicate a threshold. But we will have to wait for further experiments. I think the experiment that I described is perhaps the best approach that we can use at the present time on this. I might say a little bit more about this. The facts are, in the first place, that mutations, once they are induced, seem to be very stable. They do not show any evidence of repair. Yet the relation between mutation rate and dose intensity or dose rate seems to indicate some sort of repair mechanism.

It is almost impossible to conceive how spreading out the dose can give you a lower mutational response unless there is some kind of repair. How can these two things be reconciled? We think they can be reconciled by assuming that the repair is not on a completed mutation but on some intermediate step in the process of the mutation.

Representative DURHAM. This is from generation to generation.

Dr. RUSSELL. It is persistent in stability over many generations once it is induced. It is very rare that an induced mutation will revert back to the state it was in before.

We also feel there may be a certain proportion of mutations that are not capable of repair even in the premutational steps. These

represent the mutation frequency that we have obtained with our low intensity experiments.

If this is so, then we would expect a linear relation between mutation rate and dose even with this low intensity of radiation.

This is hypothesis, but I think the most cogent piece of evidence in support of it is that this 10,000-fold change in intensity from 90 r a minute to 90 r a week has only dropped our mutation rate to about a quarter. I would suspect if there really were a threshold we would have found a much larger drop in this mutation frequency. We tried, in our first publication on this, to discourage people from jumping to the conclusion, as I think some would be tempted to do, that this argues for a threshold response.

Representative HOLIFIELD. Of course, there is always impatience that we do not solve all of the problems in this field in one set of hearings. It is refreshing to have a balanced presentation which points out that it might go either way.

It is also refreshing to know that we have made some progress in the last 2 years. I know from talking with other medical and genetic people that they feel that they owe a great deal to this experimental work you are doing at Oak Ridge and the committee certainly wants to express its appreciation for the fine work that you are doing down there and indicate our support in every way for it.

Dr. RUSSELL. Thank you.

Representative HOLIFIELD. Are there any questions?

Representative WESTLAND. Dr. Russell, are you saying that where the subject gets this 300 roentgen dose in one shot it produces this extreme rate of mutation? Is that correct?

Dr. RUSSELL. Yes.

Representative WESTLAND. Suppose no further dosage is given to this subject, does this mutation continue, and if so, for how long?

Dr. RUSSELL. We had quite good evidence, I think, on this at the time of the last hearings. There seems to be no recovery in the individual himself. If you have had a total dose of irradiation before a certain time, then the probability of mutation appearing in the offspring will be the same throughout the rest of the reproductive period. There will be no significant dropping off in mutation rate. We don't have this down to the point where we could say we can exclude the possibility of, say, a 5 or 10 percent dropping off, but there certainly cannot be as much as 50 percent dropping off in the mutation rate. The type of recovery that we are talking about that gives us the lower response with lower dose rates is entirely different from this. What you are asking about concerns the mutations being stable once they are induced. Once they are induced in the immature reproductive cells these cells continue to bud off mutant mature cells throughout the reproductive life. There is no recovery in the individual once the mutational process is completed.

Representative HOLIFIELD. Are there any further questions?

Dr. Crow, we don't want you to leave the stand thinking that we don't appreciate your work because we know of your work as well as Dr. Russell's. This is a field that is of most concern to the people of America, that is, this genetic effects field. Certainly there should be no diminution of funds or the program of research in this field. We

hope that you will have some more information to give us 2 years from now if we have another hearing.

Thank you very much.

Dr. Crow, you are slated in the next discussion so you might as well keep your seat.

The next discussion is permissible exposure levels. Dr. G. Failla of Columbia University, will you please come forward, as well as Dr. Crow. Dr. Failla, would you like to lead off? What type of work are you doing at Columbia?

STATEMENT OF DR. G. FAILLA,[1] COLUMBIA UNIVERSITY

Dr. FAILLA. I am in radiological research, largely on the dosimetry side rather than the biological. But we also do a good deal of radiobiology in my own laboratory.

Representative HOLIFIELD. We know, of course, that you are a well-known authority in this field. I am going to ask you now in order

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[1] Occupation: Physicist.

Education: Columbia University, Engineering School, 1911–15, E.E.; Columbia University Graduate School, 1915–17, M.A.; University of Paris, 1923, D. Sc.; University of Rochester, D. Sc. (honorary), 1949.

Brief career summary: Joined staff of Memorial Hospital, New York City, in 1915 and later established there a physics and biophysics laboratory for radiation research. Developed numerous devices used in the treatment of cancer with radium, radon, and X-rays. Developed new methods of dosage measurement. Directed research in radiological physics and radiobiology to end of 1942. Became professor of radiology (physics) and director of Radiological Research Laboratory at the College of Physicians and Surgeons of Columbia University in 1943. Became associated with "Manhattan Project" in 1942 in connection with the protection of personnel from radiological hazards. Directed special research project for "Manhattan District" and Atomic Energy Commission from 1944 to date.

Principal professional or business activities: Physicist, Memorial Hospital, 1915–42. Professor, College of Physicians and Surgeons, Columbia University, 1943 to date. Assistant to scientific attaché, American Embassy, Rome, 1918–19. Consultant to: Metallurgical Laboratory, University of Chicago, 1943–46; Argonne National Laboratory, 1946 to date; Los Alamos Scientific Laboratory, 1947–48; Brookhaven National Laboratory, 1947–57; nuclear engine propulsion aircraft project, 1948–50; Council on Physical Therapy, American Medical Association, 1941–48; U.S. Public Health Service; U.S. Veterans' Administration, 1942 to date; Memorial Cancer Center, 1950 to date; Roosevelt Hospital, 1950–57; Chief, Biophysics Branch, Division of Biology and Medicine, U.S. Atomic Energy Commission, 1948.

Related and other international scientific activities: Official representative of American Physical Society to First International Congress of Radiology, London, 1925; American Radium Society delegate to Fourth International Congress of Radiology, Zurich, 1934; chairman, radiophysics section, and honorary chairman, International Committee on Radiological Units, Fifth International Congress of Radiology, Chicago, 1937; vice president emeritus, Sixth International Congress of Radiology, London, 1950; Seventh International Congress of Radiology, Copenhagen, 1953; Eighth International Congress of Radiology, Mexico City, 1956; chairman, Subcommittee on External Radiation, International Commission on Radiological Protection, 1952–58; vice chairman, International Commission on Radiological Protection, 1956 to date; member, International Commission on Radiological Units; honorary member, British Institute of Radiology, 1950 to date; International Joint Committee on Radiobiology, 1950–56.

National scientific activities: Section 17.2, National Defense Research Committee, member, 1942–45; Radiobiology Panel of Committee on Growth of National Research Council, 1945–47; executive committee, National Committee on Radiation Protection, 1946–57; chairman, Subcommittee on External Radiation, National Committee on Radiation Protection, 1946–57; chairman, Radiological Instrument Panel of Armed Forces Special Weapons Project, 1948 to date; Subcommittee on Human Applications, AEC, 1947–50; chairman, Advisory Committee on Isotope Distribution, AEC, 1947–50; member, Advisory Committee on Biology and Medicine, AEC, 1951–54, and chairman, 1955–57; member, Genetics Committee, National Academy of Sciences Committee on Biological Effects of Atomic Radiation, 1956 to date.

Memberships: Sigma Xi, American Association for Advancement of Science (fellow); American Physical Society (fellow); Optical Society of America; New York Electrical Society (president, 1940); New York Academy of Sciences; American Radium Society; American Roentgen Ray Society; Radiological Society of North America; Harvey Society; Corporation of Marine Biological Laboratory (trustee, 1948 to date); Radiation Research Society (president, 1954); Gerontological Society.

Other important career details: Pulitzer scholarship, 1911–15; Leonard Prize, American Roentgen Ray Society, 1923, 1925; Janeway Medal, American Radium Society, 1939; Caldwell Medal, American Roentgen Ray Society, 1945; Gold Medal, Radiological Society of North America, 1947; honorary member, British Institute of Radiology; American Cancer Society Annual National Award, 1956; honorary member, James Ewing Society.

Published numerous articles on radiological physics and radiobiology.

that we do not lose any of your presentation to pull up that microphone close to you and speak right into it.

Senator GORE. Mr. Chairman, I would like to inquire whether he holds a pessimistic or optimistic chair.

Dr. FAILLA. I think I am neutral.

Senator GORE. You admit it?

Dr. FAILLA. That is right.

Representative HOLIFIELD. There is an in-between, a realist.

Dr. FAILLA. At least I believe I hold the middle ground in this field rather than one or the other extreme.

Senator GORE. I warn you, Doctor, that politicians have long since staked out a claim to the middle ground.

Dr. FAILLA. And they sometimes get into trouble.

Senator GORE. That seems to be the safest way.

Representative HOLIFIELD. You may proceed, sir.

Dr. FAILLA. Mr. Chairman, the hour is late. I have prepared a document here that perhaps could be incorporated in the proceedings.

Representative HOLIFIELD. We will be glad to accept it for the record, sir.

(The formal statement by Dr. Failla follows:)

EVOLUTION OF RADIATION PROTECTION

(By G. Failla, Director, Radiological Research Laboratory, College of Physicians and Surgeons, Columbia University)

1. Permissible limits of occupational exposure to ionizing radiation

Soon after the discovery of X-rays (1895) it became apparent that skin damage could be produced by exposure to this type of radiation. It was also realized that the damage extended to the deeper tissues and in fact it was this observation that led to the use of X-rays in the treatment of cancer. It did not take long to realize that any body tissue or organ could be damaged if the amount of radiation reaching it was sufficiently large. The existence of a "latent period," or delay between the exposure and the manifestation of the injury, was noted early, but this refers only to effects observable within a few weeks or a few months at the most, which may be referred to as the short-term effects of radiation. Obviously, the long-term effects of overexposure to radiation (e.g. the production of cancer) did not become apparent for a considerable time. It is now known that the interval between acute local overexposure and the appearance of cancer of the skin may be greater than 25 years, although in general it is about 10 to 15 years. In the case of occupational exposure, the dose accumulates slowly and the delay is even greater.

By 1920 many of the early radiologists and technicians had developed cancer of the skin (usually in the hands or face, because these parts received the largest doses) and others had died of anemia and probably leukemia. During the 1920's there was considerable activity in different countries to improve the protection of radiologists and technicians. In order to establish some norms, physical measurements of radiation exposure were made in many radiological installations. Since the roentgen, as a unit of dose was not adopted internationally until 1928, the measurements were usually expressed in terms of the "acute" (single exposure) X-ray dose required to produce a reddening of the skin (the "erythema dose"). From these studies evolved the concept of a "tolerance dose" and the values suggested for it ranged from 0.002 to 0.01 of an erythema dose per month.

It is important to note the process by which these values were arrived at. The measurements mentioned above, established (albeit with rather low accuracy) the degress of occupational exposure of personnel in typical departments of radiology. This could be related to the state of health of those who had been exposed essentially at this rate for a number of years. By comparing the conditions of exposure with those to which certain workers had been subjected and

had been or had not been injured, an estimate of a "safe" rate of exposure was made. The thinking at the time was that if no appreciable injury was evident within several years, it would be safe for the individual to be exposed at the same rate indefinitely. Skin damage was the main problem in those days and there was ample clinical experience to support the view that substantial recovery took place. Accordingly, it was expected that if the rate of exposure was low enough, tissue damage would be corrected by recovery and repair processes and no appreciable injury would result in the lifetime of the individual. It should be noted that repair of damage (e.g. the healing of a wound, the replacement of lost blood) is a normal physiological process and at the time it was reasonable to suppose that it applied to radiation damage as well. As a matter of fact, it does apply, but the difference is that after gross tissue repair has occurred there is still some residual damage—often imperceptible at first—which persists and may lead to serious complications many years later. Another important point to keep in mind is that the realization of the dangers of exposure to ionizing radiation was a gradual process and that at each step greater precautions were taken. Thus, at any particular time the exposure of personnel was less than it had been 10 or 15 years before and, therefore, the injuries seen then in the older radiologists had been produced under conditions that no longer existed.

The International X-ray and Radium Protection Commission (which later became the International Commission on Radiological Protection—ICRP) was established in 1928 by the Second International Congress of Radiology. This Commission recommended a "tolerance dose" of 0.2 r per day in 1934. The same valve was recommended in this country by the Advisory Committee on X-ray and Radium Protection (which later became the National Committee on Radiation Protection—NCRP). Some time before 1920 I established the practice of taking monthly blood counts of all technicians working with radium in my laboratory, although by the nature of the work the chief hazard was injury of the skin of the hands. In 1927 I designed the first teletherapy apparatus with 4 grams of radium in which it was possible to "shut off" the gamma ray beam during the process of adjusting it to the patient. This device was operated 24 hours a day and three nurses in 8-hour shifts were in attendance. In the course of operating this device and watching the patient through a lead glass window, these nurses were exposed to some radiation. The exposure was determined by film badges calibrated with radium gamma rays. Since blood counts were taken every month, a few years later (1932) it was possible to establish that this rate of exposure did not produce perceptible changes in the blood counts of two of the nurses who had had the longest exposure; namely 4.5 and 3 years, respectively. These physical measurments and blood counts provided the basis for a "radium protection chart" in which I related the amount of radium, the lead thickness of the shield and the distance that would provide "adequate" protection. The chart was later included in HB23 (1938). This development is recounted here because it provides the background for the adoption in 1936 of a tolerance dose of 0.1 r per day by the Advisory Committee on X-ray and Radium Protection, when the internationally recommended value was 0.2 r per day. By this time reasonable agreement had been reached on the measurement of radium gamma ray doses in terms of the roentgen, and it turned out that my "radium protection chart" (which in itself did not involve the dose) corresponded to a tolerance dose of very nearly 0.1 r per day. This value had the merit that it was based on actual experience indicating a lack of effect on blood count in two technicians who had been exposed for a few years. This could hardy be considered satisfactory evidence, but it was better than anything available at the time. Perhaps exposure at the rate of 0.2 r per day would not have produced any perceptible changes in blood count in the two technicians either, but the general feeling of lowering the tolerance dose was in the air and, at my suggestion, the committee adopted 0.1 r per day as the tolerance dose. HB 20 (1936). I might add that at that time the genetic hazard of exposure to ionizing radiation was beginning to be recognized by nongeneticists, largely through the efforts of H. J. Muller, who was the leading expert in this field. Suggested values of the tolerance dose in chronological order are given in the appended table, reproduced from "Radiation Protection" by Braestrup and Wyckoff.

In the development of the atomic bomb great caution was exercised and all protective measures were designed to comply with the adopted maximum daily dose of 0.1 r. (While many individuals were involved in radiation protection, Dr. Robert Stone deserves the chief credit in that he set the pattern in the early

days of the plutonium project.) New problems had to be faced and even new units had to be coined.

After the war it became evident that the radiation protection problem had reached proportions undreamed of before. Accordingly the Advisory Committee on X-ray and Radium Protection was reorganized and became the National Committee on Radiation Protection, with a much larger membership and scope of activity. I became chairman of Subcommittee I, which was charged with the task of formulating basic rules. Dr. K. Z. Morgan was made chairman of Subcommittee II dealing with the difficult problem of establishing permissible body burdens of radioisotopes and related matters. I invited two prominent geneticists (Dr. H. J. Muller and the late Dr. Donald R. Charles, later replaced by Dr. Curt Stern) to become members of Subcommittee I. This was the first time that any geneticist took a direct part in the deliberations of a radiation protection committee.

[Reproduced from "Radiation Protection," by Carl B. Braestrup and Harold O. Wyckoff. Charles C. Thomas, publisher]

TABLE I.—*Historical development of "tolerance dose"*

Date	Author	Days for 1/1000 erythema	Calculated r/day
1902	Rollins	(Photographic)	10
1925	Mutscheller	3	0.2
1925	Sievert	3	0.2
1926	Solomon	0.3	2.0
1927	Dutch Board	15.0	0.04
1928	Barclay and Cox	3.5	0.17
1928	Kaye	5	0.12
1932	Failla (gamma rays)	30.0	[1] 0.1
1932	Stenstrom	3.7	0.16
1934	I.C.R.P.[2]		0.2
1936	N.C.R.P.[3]		0.1
1949	N.C.R.P.		[4] 0.05
1950	I.C.R.P.		[4] 0.05

[1] Estimated from gamma-ray erythema=1,800 r. Other calculations on erythema=600 r.
[2] International Commission on Radiological Protection, then known as International X-ray and Radium Protection Commission.
[3] National Committee on Radiation Protection and Measurements, then known as Advisory Committee on X-ray and Radium Protection.
[4] Based on a weekly irradiation of 0.3 r in air.

The subcommittee realized at the outset that the development of the atomic bomb made necessary an entirely new approach to the protection problem. Before the war occupational exposure occurred largely in medical installations and one had to deal mostly with X-rays produced at voltages in the range of 80 to 250 kilovolts. Therefore, it was sufficient to express a "tolerance dose" in terms of roentgens per day without being too specific as to the region of the body to which this applied. In general in this country a measurement for protection purposes was made "in air," without the presence of the human body, at the place where the worker would be in the performance of his duties. The new sources that became available after the war provided radiation of different types and widely different penetrating powers. Also, radioactive material entering the body would go to some internal organs and would irradiate them even though the radiation they emitted was of very low penetrating power. These problems, of course, had to be faced during the war and the tolerance dose of 0.1 r per day (or its equivalent for nonelectromagnetic radiation) was made to apply to the dose delivered to any body tissue.

The subcommittee introduced the concept of "critical organs" and specified the harmful effects to be guarded against. The specified critical organs and effects (in parentheses) were: skin (cancer), bloodforming organs (leukemia), gonads (sterility), lenses of the eyes (cataracts). This refers primarily to exposure resulting from sources outside of the body. For "internal emitters" organs in which a radioactive material concentrates to a high degree, constitute critical organs also. In principle the "permissible dose" (see below) for each critical organ should be chosen on the basis of the radiosensitivity of the organ with respect to the pertinent harmful effect, the seriousness of this effect and other considerations. There was not sufficient information at the time to do this quantitatively for each critical organ and effect. Therefore, a distinction

was made only between skin and the other critical tissues, in that the permissible dose for the skin was set at twice the value recommended for the other critical tissues. There was a sound basis for this because much more was known about long-term skin damage in radiologists and cancer of the skin is readily curable in most cases, while leukemia is always fatal.

Long-term experiments on animals carried out during the war indicated that there might be a shortening of life due to nonspecific causes, even at low levels of chronic exposure. Experiments and theoretical considerations indicated that genetic damage should be expected even from very low doses. For these reasons (and less tangible ones) the feeling had grown in previous years that the concept of a "tolerance dose" *as a safe dose* should be abandoned. To make the change perfectly clear the subcommittee decided to use a new expression—the permissible dose. The following statement from HB 59 defines and explains the new terminology.

"4.3 PERMISSIBLE DOSE

"The concept of a tolerance dose involves the assumption that if the dose is lower than a certain value—the threshold value—no injury results. Since it seems well established that there is no threshold dose for the production of gene mutations by radiation, it follows that strictly speaking there is no such thing as a tolerance dose when all possible effects of radiation on the individual and future generations are included. In connection with the protection problem the expression has been used in a more liberal sense, namely, to represent a dose that may be expected to produce only "tolerable" deleterious effects, if any are produced at all. Since it is desirable to avoid this ambiguity the expression "permissible dose" is much to be preferred.

"It is now necessary to give this expression a more precise meaning. In the first place it is well to state explicitly that the concept of a permissible dose envisages the possibility of radiation injury manifestable during the lifetime of the exposed individual or in subsequent generations. However, the probability of the occurrence of such injuries must be so low that the risk would be readily acceptable to the average individual. Permissible dose may then be defined as the dose of ionizing radiation that, in the light of present knowledge, is not expected to cause appreciable bodily injury to a person at any time during his lifetime. As used here 'appreciable bodily injury' means any bodily injury or effect that the average person would regard as being objectionable and/or competent medical authorities would regard as being deleterious to the health and well-being of the individual. 'Dose' is used here in its radiological sense and particularly as tissue dose in the irradiated tissue, organ, or region of interest. What constitutes the region of interest depends on the conditions of exposure and must be taken into account in assigning numerical values to the permissible dose or doses applicable to a given set of conditions."

The quotation is pertinent at this time because there seems to be considerable confusion as to the exact meaning of "maximum permissible dose." The word "permissible" is certainly appropriate in a legal sense. Thus, the speed limit posted on a road represents the maximum permissible speed, but there is no implication that if this speed is not exceeded there will be no traffic accidents on that road. (Latest definition is given in sec. 2, below.)

As mentioned before, when the "tolerance dose" of 0.1 r per day was recommended, occupational exposure consisted largely of exposure to medium-voltage X-rays. In the case of radiologists the exposure was mostly from fluoroscopes and the radiation traversed the body in the general direction of front to back, although scattered radiation could come from any direction. With the more powerful sources of radiation that came into use during and after the war, and the diversity of applications outside of the medical field, it became evident that the distribution of radiation in the human body could be much more uniform than it had been before. To take care of this situation, the subcommittee decided to assign to all critical organs except the skin, permissible doses that *numerically* were about one-half of the previous tolerance dose, that is 0.3 r per week. The permissible dose for the skin was set at 0.6 r per week (numerically the same as the previous tolerance dose in a period of 6 days). Actually a direct comparison cannot be made because the basis of measurement is different in the two cases: the tolerance dose was usually measured "in air," while the new permissible doses were expressed in terms of the doses received by specified organs. On the whole the new recommendations were intended to insure greater protection, but it is difficult to say by what factor.

The subcommittee changed the time basis from 1 day to a week ("7 consecutive days") for practical reasons. If the limit is for 1 day, then in order to be sure that it is not exceeded, the monitoring device (e.g. a film badge) must be read every day. This is wholly unnecessary when the working conditions are such that no large fluctuations of exposure occur from day to day. Furthermore, when the dose is very small it may be expected that the rate at which it is received is not very important in the long run.

The subcommittee introduced several other innovations, which need not be discussed here. One deserves brief mention because it marks the beginning of the recommendations for a lower permissible dose for persons nonoccupationally exposed. The pertinent statement in HB 59 is as follows:

"Because it is impractical to keep account of the exposure of individuals outside an area in which occupational exposure occurs, it is necessary to make sure that the weekly dose received by minors outside said area be negligible insofar as subsequent occupational exposure is concerned. A factor of 10 is considered satisfactory for this purpose. Therefore, it is recommended that in cases in which minors may be exposed to radiation in the course of their normal activities, protective measures be taken to make sure that no minor actually receives radiation at a weekly rate higher than one-tenth the respective basic permissible weekly doses for the critical organs and other body tissues, according to the basic dose distribution."

All the recommendations made by the subcommittee were accepted by NCRP and were published as HB 59 in 1954. Actually agreement had been reached on most of the recommendations at the first meeting of the subcommittee in 1948. Informal reports were made by the chairman of NCRP to other subcommittees and the new limits were used as a basis for their recommendations even before HB 59 was published.

In 1948 I went to England to discuss the proposed new permissible limits with the British counterpart of our NCRP. Our basic proposals were accepted. They were also accepted later by the International Commission on Radiological Protection. (Compare HB 59 and Recommendations of ICRP published in 1955 as Supplement No. 6 of British Journal of Radiology.)

While the genetic hazard was discussed by subcommittee I no specific recommendations were made in HB 59. This was done (with the approval of the two geneticists on the subcommittee) simply because the handbook dealt with occupational exposure and the genetic effects of such exposure at the recommended limits on the whole population would be minimal. It was planned to prepare a second report dealing exclusively with the genetic problem. As chairman of the subcommittee I was about to arrange a meeting of the leading geneticists in this country to discuss the problem, when I heard that the National Academy of Sciences had been asked by the Rockefeller Foundation to study the biological effects of atomic radiations. The reports of this study were published in 1956 and an extensive discussion of genetic effects is included. A similar report was issued simultaneously by the British Medical Research Council. The problem was also discussed by the ICRP at its meeting in Geneva, April 1956, and a preliminary statement was issued later. More will be said later.

2. Latest recommendation of the International Commission on Radiological Protection

At the Geneva meeting of ICRP (April 1956) I proposed a further reduction in the permissible dose for occupational exposure of the whole body. The reasons are given in the Commission's report "Radiation Protection—Recommendations of the International Commission on Radiological Protection" just published by Pergamon Press. A brief summary will be given here.

It has always been emphasized by both NCRP and ICRP that the exposure of radiation workers should be kept as low as practicable, irrespective of maximum permissible limits. In general there has been very good cooperation on the part of responsible authorities in this respect and liberal factors of safety have been introduced in the design of radiation installations to reduce the actual exposure of workers much below permissible limits. Thus in AEC installations the great majority of workers received less than one-tenth the yearly permissible dose in 1955. However, the 1955 Conference on the Peaceful Uses of Atomic Energy aroused great interest in the development of atomic powerplants throughout the world. In time this would greatly increase the number of persons occupationally exposed and would also bring about actual or potential exposure of other persons and the population as a whole. More importantly, the pressure for producing power economically might well do away with the "safety factors" mentioned

above. Also, the average duration of occupational exposure per individual worker might increase.

On the biological side, it appeard that perhaps "recovery" plays a less important part in the long-term effects of radiation to be expected from continued exposure at low levels, than was earlier supposed. The higher than normal incidence of leukemia in radiologists has been well established. A statistical study by Dr. Shields Warren showed that the average age at death was about 5 years lower for radiologists than for other specialists, presumably not exposed to radiation in the exercise of their professions. (This was later found to be due largely to differences in the age distributions of the groups of specialists. A similar survey made in England recently shows no significant difference in longevity between radiologists and other medical specialists. Nevertheless, the possibility of a certain degree of life shortening attributable to whole body exposure to penetrating radiation cannot be excluded.)

The ICRP recommended a reduction in the maximum permissible dose for whole body occupational exposure by approximately a factor of three. It retains essentially the former limits for (internal) exposure of single organs, except the gonads. In addition certain changes of practical importance were also made. As mentioned above, in AEC installations the actual dose received by the average worker was much less than the maximum permitted value. Although no similar information is available for other installations, this must also be true in general. However, certain operations, especially in cases of emergency, cannot be carried out at all—or at least economically—without exposing workers temporarily to higher levels of radiation. Therefore, the ICRP made provisions for higher temporary exposure levels ("within a period of 13 consecutive weeks") and adopted a formula first introduced by NCRP for the maximum occupational dose that could be accumulated up to a given age. For the most critical organs and whole body exposure the formula is $D=5(N-18)$ where D is the accumulated dose in rems at age N years, with the proviso that in any 13 consecutive weeks the dose should not exceed 3 rems. This means that while on the average the limit for occupational exposure corresponds to 5 rems per year, some workers who have not accumulated the maximum dose at their ages may receive up to 12 rems in 1 year. This takes care of practical requirements and at the same time insures that the accumulated dose at any given age does not exceed one-third the maximum value formerly permitted. It is important to note that in effect the new recommendations do not decrease the average dose *actually received* by radiation workers by a factor of three, because the average worker never received anything like the full quota. Therefore, the new recommendations do not impose undue restrictions, but limit the dose that could possibly be accumulated by an individual to one-third of its former value.

The definition of permissible dose in the latest report of ICRP is more explicit than that in HB 59. The pertinent paragraphs are quoted below:

"(29) Any departure from the environmental conditions in which man has evolved may entail a risk of deleterious effects. It is therefore assumed that long continued exposure to ionizing radiation additional to that due to natural radiation involves some risk. However, man cannot entirely dispense with the use of ionizing radiations, and therefore the problem in practice is to limit the radiation dose to that which involves a risk that is not unacceptable to the individual and to the population at large. This is called a 'permissible dose.'

"(30) The permissible dose for an *individual* is that dose, accumulated over a long period of time or resulting from a single exposure, which, in the light of present knowledge, carries a negligible probability of severe somatic or genetic injuries; furthermore, it is such a dose that any effects that ensue more frequently are limited to those of a minor nature that would not be considered unacceptable by the exposed individual and by competent medical authorities.

"(31) Any severe somatic injuries (e.g., leukemia) that might result from exposure of individuals to the permissible dose would be limited to an exceedingly small faction of the exposed group; effects such as shortening of life-span, which might be expected to occur more frequently, would be very slight and would likely be hidden by normal biological variations. The permissible doses can therefore be expected to produce effects that could be detectable only by statistical methods applied to large groups."

It will be noted that since 1934 the maximum permissible level of continued whole body occupational exposure to ionizing radiation has been lowered in three steps from 60 to 5 rems per year. Each time the reduction has been made not because of definite knowledge of harm produced by exposure at the

higher permissible limit, but because it was judged prudent to do so on the basis of radiological experience, animal experimentation and theoretical considerations. By 1934 there was a general awareness of the hazard, and gross short-term injuries were no longer common. It would have taken at least 25 years of exposure at the same level to evaluate the long-term effects and then only by statistical methods.

It is important to note in this connection the difference between a maximum permissible limit and the average dose that workers actually received. Both NCRP and ICRP have always emphasized that the exposure should be kept as low as practicable. Designers of radiation installations have taken this seriously and have used substantial factors of safety, at least during the last 20 years. For this reason, compliance with the lower permissible limits in general has not necessitated marked changes in protective barriers or operative procedures. This refers to occupational exposure. The stringent limits for non-occupational exposure are more difficult to meet. In my opinion a further general lowering of permissible limits would impose a heavy burden on the operation of radiation installations.

A review of the latest recommendations of ICRP appeared in the London Times of April 17, 1959. It is reproduced below because it brings out very well the salient features.

"[From the London Times of Apr. 17, 1959]

"RISKS IN EXPOSURE TO RADIATION—CONCLUSIONS OF WORLD COMMISSION

(From our science correspondent)

"Questions about the maximum exposure to radiation which may be acceptable in different groups of people involve problems in human accountancy which are either new in kind or—to the extent that they are not new—have not before needed to be faced openly on a large scale. They are brought into the open in the recommendations adopted in September of last year by the International Commission on Radiological Protection and now published.[1] They deserve to be widely read: in their present, or probably in modified, form they will affect the cost of electricity generation, the use of radioisotopes in industry, and the health and lives of men.

"SCIENCE AND MEDICINE TODAY

"The first problem is that of balancing like against unlike. Logically insoluble, it is implicit in the principle of 'the greatest happiness of the greatest number,' applied by Jeremy Bentham in the late 18th century to distinguish good laws from bad. It is applied unconsciously today in the treatment of road accidents. In the last year (1952) for which an attempt to compile world figures was made, mechanically propelled vehicles caused some 80,000 deaths on the road. A very rough estimate suggests that the invention of the internal combustion engine has been responsible—on land alone—for at least some millions of deaths. Since all but a few could have been prevented by a drastic enough speed limit—say 10 m.p.h.—it is evident that legislators in all countries have effected a balance, consciously or not, between usefulness and safety. The essential new elements in the accountancy undertaken by the International Commission are that it involves the health and safety not only of those now living but also of future generations, and that they have been compelled to look consciously at the problem.

"*Modest approach*

"The International Commission was established in 1928 under the auspices of a medical body—the International Congress of Radiology—and is composed of individuals 'chosen on the basis of their recognized activity in the fields of radiology, radiation protection, physics, biology, genetics, biochemistry, and biophysics, without regard to nationality.'

"They approach their task modestly. They are aware of the fact that 'a proper balance between risks and benefits cannot yet be made, since it requires a more quantitative appraisal of the probable biological damage and the probable benefits than is presently possible.' They appreciate, too, that such a balance-sheet might be struck differently in different countries, and conclude logi-

[1] Radiation Protection. Recommendations of the International Commission on Radiological Protection. Published by the Pergamon Press for the Commission. 3s. 6d."

cally—but with some potential risk to others—that 'the final decision rests with each country.'

"The Commission strike a balance of another kind in their approach to medical uses of radiation. Starting from the point that, for large populations, 'a genetic dose of 10 rems from all man-made sources is regarded by most geneticists as the absolute maximum and all would prefer a lower dose,' they observe that in some countries a genetic dose of about 4.5 rems has been estimated from medical uses alone. Holding that this could be reduced considerably—as earlier reported—without medical loss, they take the line that it is up to the countries concerned to do so, and assume that they have 5 rems to apportion, in addition to exposure of medical origin.

"At this point it is necessary to remember that, in relation to the long-term effect of mutations, it is irrelevant (on orthodox views) whether a large population is exposed to a small dose of radiation or a smaller population to a larger one, provided that the latter is not so large as to cause significant risk of damage to the individual.

"*Atomic energy plants*

"This leads to a second, also unfamiliar, kind of arithmetic—that involved in apportioning the extra dosage considered to be permissible as between one group of people and others; those who are exposed to radiation in the course of their work, including atomic energy workers; special groups, notably those living in the neighborhood of atomic energy plants; and the population at large. Such an apportionment involves consideration of the numbers of people in each group as well as the extra dosage that they may receive. For example, if the whole of the 5 rems considered to be available were allocated to the population at large there would be no allowance left over for occupational workers and the running of atomic energy plants would be impossible. Conversely, if more than about 8.5 percent of the world population were exposed to radiation in the course of their work—with present standards of maximum individual exposure—there would be no margin for either of the other two groups.

"*Final genetic dosage*

"In the event, the Commission have refrained from making any firm recommendation on the question of apportionment, and give only illustrative figures—which include a reserve of 1.5 rems. On this basis, they consider the consequences of allocating 1 rem for occupational exposure (which would allow 8 to 16 times as many people to work in atomic energy as now do so in technologically developed countries 0.5 rem to special groups (which would allow about 3 percent of the population to be exposed in each year to the maximum permissible individual dose to the reproductive organs); and 2 rems for the population at large.

"The last of these apportionments 'is intended for planning purposes in the development of nuclear energy programs (with the associated waste disposal problems) and more extensive uses of radiation sources.' In relation to air and water, this would permit proportions of radioactive material equal to about one-hundredth of those permissible in the case of occupational workers.

"The consequences of weapons tests are not discussed; but it may be useful to add for proportion that the final genetic dosage resulting from all explosions to come is likely to be of the order of one-hundredth of the dosage allowed by the Commission for the population at large. However, the substantial point remains this: that for the peaceful application of atomic energy on a world scale, it is necessary that calculations of the kind quoted should be made—and presented as the balance sheet of risks and benefits which, in fact, they are."

3. Permissible limits for nonoccupational exposure

As already mentioned the first attempt to regulate exposure of persons outside of radiation installations was made in HB 59. This was done indirectly (see quotation in the previous section) by stipulating that children should not be exposed at a rate higher than one-tenth the maximum permissible weekly doses for occupational exposure. Note that the ostensible reason for this was to make sure that the dose received by children up to the time (18 years) that they might be employed in a radiation installation would be negligible in comparison to the occupational exposure they would receive in a lifetime. Taking this as a basis, the ICRP report published in 1955 contained the following statement: "The Commission recommends that, in the case of the prolonged ex-

posure of a large population, the maximum permissible levels should be reduced by a factor of 10 below those accepted for occupational exposures." Actually this came about through some misunderstanding, since the factor of one-tenth was never intended to apply to a large population. There has always been considerable reluctance on the part of radiation protection groups to make recommendations for the exposure of large populations: in the first place because of the limited knowledge concerning the effects of exposure at low levels of ionizing radiation; and secondly because the problem might well be considered to be outside the scope of their activities. Nevertheless, by the time that the ICRP held its meeting in Geneva in 1956, it was evident that something had to be done.

Geneticists (and others) have emphasized the genetic hazard for a long time and have recommended that exposure of the gonads be limited "as much as possible." This sort of recommendation is of no use to engineers designing nuclear powerplants, or for that matter, designing the protective shields and devices for any source of radiation. It is always "possible" to reduce the exposure if one is willing to pay the price. At what point should the engineer stop? On the other hand, lack of knowledge made everybody reluctant to set a permissible limit for the genetic dose to the whole population in numerical terms. The Genetics Committee of the National Academy of Sciences deserves the credit for resolving the impasse, by recommending that the "general population shall not receive from such sources (manmade sources) an average of more than 10 roentgens, in addition to background, of ionizing radiation as a total accumulated dose to the reproductive cells from conception to age 30." The British group made no specific numerical recommendation but indicated the order of magnitude and suggested that ICRP should set the value.

The preliminary recommendations made by ICRP in 1956 were couched in general terms but in the 1959 report they are very specific. It accepted the genetic dose of 10 r suggested by the NAS Genetics Committee as an upper limit and suggested a subdivision of this total into allotments for specific purposes, as follows:

	Rems
Medical exposure	5.0
Occupational exposure	1.0
Exposure of special groups	.5
Exposure of the population at large	2.0
Reserve	1.5

It is important to note that the ICRP does not recommend that the genetic dose from medical exposure be 5 rems. In fact it recommends "that it be kept as low as is consistent with the necessary requirements of modern medical practice." It simply recognizes the fact that the *estimated* genetic dose in some countries is close to 5 rems. The quotas for nonmedical exposure are ample to permit foreseeable expansion of the peaceful uses of atomic energy without undue restrictions.

The genetic dose to a population is the dose which, if it were received by each person from conception to the mean age of childbearing, would result in the same genetic burden to the whole population as do the actual doses received by the individuals. A permissible genetic dose is that dose which, if it were received by each person from conception to the mean age of childbearing, would result in an acceptable burden to the whole population. In order to evaluate the genetic dose one must know the relation between dose and genetic effect. It is generally assumed that the relation is a linear one, that is, that the genetic effect is proportional to the dose and that there is no dose (no "threshold dose") below which no genetic effect is produced. On this basis a large dose to a few individuals is equivalent to a small dose to a roccespondingly larger number of individuals, insofar as the genetic effect on the total group of individuals is concerned. There is a good scientific basis for this assumption, although the recent work of Dr. William L. Russell and his staff at Oak Ridge indicates that some modifications may have to be made.

In the case of somatic effects there is considerable difference of opinion as to the dose-effect relationship and in particular as to the existence of a threshold dose for any given effect. Obviously, if there is a threshold dose, those receiving lower doses would show no effect at all. If there is a threshold dose and its magnitude is higher than any dose that could be accumulated at permissible levels of occupational exposure, there is no reason to have lower permissible limits for nonoccupational exposure as regards somatic effects. The matter is

only of academic interest insofar as setting a limit to the population dose resulting from external sources of penetrating radiation is concerned. Under these conditions genetic damage is of paramount importance and the limitation of the dose to the gonads will automatically limit the dose to the rest of the body to essentially the same level. The top limit recommended by the ICRP for the genetic dose from all manmade sources (except medical exposure) is 5 rems. Therefore, this will indirectly limit the whole body dose to 5 rems in 30 years or 12.5 rems in 75 years. This is certainly satisfactory for the present time because the actual whole-body dose of penetrating radiation received by the population from manmade sources is certainly much less than this and it can only increase very slowly. Obviously, the rate of increase must be under control if the genetic dose must never reach the 5-rem limit. The ICRP (and NCRP as well) has always reserved the right to change the recommended permissible limits whenever new knowledge indicates that they should be changed.

The same limitation to the level of the genetic dose applies also to internal exposure resulting from radioisotopes that directly or indirectly contribute to the gonad dose or constitute whole-body exposure. For a radioisotope that concentrates in a single organ the recommendation is made in terms of the maximum permissible concentrations in water and air recommended by Committee II. In essence the ICRP recommends values for the population at large not in excess of one-thirtieth of the maximum permissible values, for continuous occupational exposure. Since in general the basis for calculating maximum permissible concentrations in air and water is the permissible occupational does of 0.3 rem per week in a single organ, the maximum permissible dose for an individual of the population is 0.5 rem per year. This is for a single organ and allowance must be made for any contribution to the dose in that organ by external radiation and other internal emitters. The accumulated dose in 75 years would be 37.5 rems. This is a reasonable maximum lifetime dose *for a single organ* if it is assumed that a maximum lifetime dose of 12.5 rems *for the whole body* (resulting from the permissible genetic dose) is satisfactory. There is, however, an important difference to be noted. The genetic dose is an average for the whole population, whereas the single organ dose is the maximum for an individual in the population. This means that the average single organ dose for the population would be much lower.

At the moment there is some confusion as to whether the figure suggested by ICRP is the maximum for an individual or the average for the population. In the foregoing I have assumed that it is the maximum for an individual. Dr. K. Z. Morgan in his testimony assumes that it is an average for the population. For this reason the relevant paragraphs of the ICRP report or quoted below:

"(66) No specific recommendations are made at this time as to the maximum permissible 'somatically' relevant dose to the population. However, it is expected that the maximum permissible limits of the individual total doses recommended in paragraphs 46–57 will keep the average dose in any tissue at such a level that the injuries that could possibly occur in a population would be well within acceptable limits. (See paragraph 31.)

"(67) In the case of external exposure of the whole body to penetrating radiation the restriction imposed by the genetic dose to the population automatically reduces the doses to internal organs below the *individual* maximum permissible annual doses recommended in paragraphs 46–57. Thus the probability of somatic injury in these organs is considerably lower than indicated in paragraph 66. The same thing applies to internal exposure resulting from radioisotopes that directly or indirectly contribute to the gonad dose of a population. (See addendum to paragraph 65.)

"(68) There remain for further consideration those isotopes that concentrate in specific organs (other than the gonads). In view of the existing uncertainty as to the dose-effect relationships for somatic effects, it is suggested that for planning purposes the average concentrations of such isotopes, or mixtures thereof, in air or water, applicable to the population at large, should not exceed one-thirtieth of the MCP values for continuous occupational exposure given in the report of Committee II."

Something should be said about the repeated lowering of permissible limits. As an industrial hazard, radiation exposure has received more attention and publicity than any other. In the past the cost of increased protection has not been too great and, at any rate, it has not been apparent to the public. From now on the balancing of risks and benefits will come under closer scrutiny and the matter of cost will receive more attention, especially in connection with the

development of atomic power. Present-day society accepts some risks if the advantages are regarded to outweigh them. Thus, in this country we tolerate the killing of about 40,000 persons a year in traffic accidents when it would be possible to reduce the number greatly by a drastic reduction in the speed limits. An exact balance has not been made in this case, either. Perhaps because traffic moves faster we are saving an equal number of lives in other ways. This is difficult to evaluate. In a direct way we are saving some lives because the doctor or an ambulance can get to a patient faster or fire engines can get to a fire faster. The indirect ways, which accelerate progress in every field of endeavor are more difficult to visualize.

Something should be said also about the fact that as new knowledge has accumulated the radiation hazard has acquired greater importance. One very recent discovery, however, has reversed this trend. I am referring to the paper by Russell, Russell and Kelly in the December 19, 1958, issue of Science, in which it is shown that the frequency of radiation-induced mutations is not independent of dose rate, as had been believed before. The difference found in the experiments described is about fourfold. It might be even greater at the very low dose rates involved in the radiation protection problem. This means that the "doubling dose," that is, the dose of radiation estimated to double the spontaneous mutation rate, instead of being something like 30 rems, would be 120 rems or perhaps even higher, in the range of dose rates involved in the protection problem. (It may be noted in passing that the concept of a "doubling dose" is no longer valid, when its value depends on the dose rate). Accordingly, if Russell's results are valid, the genetic hazard at low dose rates is much less than had been supposed.

4. Relation to radioactive fallout

The fallout problem has obvious political implications which are outside the province of ICRP. The Commission is concerned with exposure resulting from any manmade source and makes recommendations irrespective of what effect they may have on the fallout problem. It is natural, however, that ICRP recommendations should be used to assess the fallout hazard. Accordingly, I will indicate the application of the ICRP recommendations to the Sr^{90} problem.

The most direct way to assess the Sr^{90} hazard is to determine its concentration in the bones of children who were born and died during the fallout period. In the report of Committee II of ICRP (which is being printed) the recommended maximum body burden of Sr^{90} for occupational exposure is 2 microcuries. This corresponds to 2,000 micromicrocuries per gram of calcium in the skeleton and to a dose rate in the bone of 5.6 rads per year. Therefore, the permissible maximum for nonoccupational exposure is approximately ($\frac{1}{30}\times 5.6=$) 0.2 rad per year, or a lifetime dose of 15 rads. Since the relative biological effectiveness (RBE) of high energy beta rays is assumed to be 1 for protection purposes, this corresponds to 0.2 rem per year; which is lower than the value of 0.5 rem per year derived from the annual dose in a single organ (15 rems) permitted for occupational exposure and the one-thirtieth factor. The discrepancy between the two arises from the fact that the Sr^{90} values are based on a permissible body burden of 0.1 microcurie of Ra^{226} and certain other parameters, instead of being based on 0.3 rem per week as the maximum permissible weekly dose in bone for occupational exposure. The maximum permissible body burden for an individual of the population is then ($\frac{1}{30}\times 2=$) 0.067 microcurie, which is equivalent to 67 micromicrocuries per gram of calcium in the bone, and which delivers a lifetime (75 years) dose of 15 rads (or rems) to the bone. Since this is the maximum for an individual, the average for a large population would be much lower, perhaps one-tenth, but let us say one-fifth, *under the conditions envisaged by ICRP;* that is, the peaceful applications of atomic energy, not weapons testing. This would make the lifetime dose to the bone 3 rems as an average for the population. The corresponding dose to the bone marrow would be approximately 1 rem.

These doses may now be compared with the average lifetime dose of 12.5 rems *for the whole body* resulting from the maximum permissible genetic dose of 5 rems in 30 years, exclusive of medical exposure. It is evident that the risk from a lifetime dose of 12.5 rems *in every* tissue of the body must be much higher than the risk from lifetime doses of 3 rems and 1 rem essentially *limited to the bone and bone marrow*, respectively. Furthermore, the genetic risk for a given (small) dose is considered to be much greater than the risk of somatic effects. It is apparent, therefore, that the recommendations of the ICRP when applied to

Sr^{90} are much more conservative than the recommendations for whole body exposure, in terms of averages for the whole population. This is because under the conditions envisaged by ICRP, Sr^{90} would accumulate very slowly indeed and there is no urgency for making a more realistic balance between risks and benefits. The following quotation from the latest ICRP report is relevant in this connection: "The Commission is aware of the fact that a proper balance between risks and benefits cannot yet be made, since it requires a more quantitative appraisal of both the probable biological damage and the probable benefits than is presently possible. Furthermore, it must be realized that the factors influencing the balancing of risks and benefits will vary from country to country and that the final decision rests with each country (insofar as operations within one country do not affect other countries)."

I think it is fair to apply Russell's finding to at least some somatic effects of radiation, such as leukemia. In making estimates of the possible increase in the incidence of leukemia attributable to small doses of radiation, it has been customary (and prudent) to assume that the incidence is proportional to the dose. If this proportionality relation does not hold for genetic effects, it is reasonable to suppose that it does not hold for somatic effects. I shall now make some estimates taking this into account and involving some *personal views* concerning leukemogenesis. There is a growing feeling that somatic mutations have something to do with the induction of leukemia. If this is the case, all mutations, whether caused by radiation or in other ways, must play a part, although not necessarily a proportional part. If the "doubling dose" at low dose rates is 120 rems for genetic effects, we may assume that it is the same for the induction of leukemia. Since the background contribution is about 3 rems in the same period of time (30 years), this means that the incidence of leukemia attributable to ionizing radiation (background) is about one-fortieth of the total attributable to spontaneous somatic mutations. In this country at the present time about 12,000 cases of leukemia occur every year. However, the rate has increased rapidly in the last 20 or 30 years. Environmental factors (perhaps including manmade radiation) must be responsible, at least in part, for this increase. Taking into account improvements in diagnosis, it is fair to assume that about one-half of the present incidence is attributable to "spontaneous" mutations. Therefore, according to these assumptions, about 6,000 cases of leukemia per year are attributable to the "doubling dose" of 120 rems in 30 years, corresponding to 4 rems per year. Of the total of 6,000 cases, less than 4,000 would be of bone marrow origin.

The ICRP and its committee I recommend a maximum body burden of 2 micocuries of Sr^{90} for occupational exposure and one-thirtieth of this as the maximum for an individual of the general population. This corresponds to a dose rate in bone of 0.2 rem per year. Under the conditions envisaged by ICRP (which do not include weapons testing) the average bone dose for the population would certainly be less than one-fifth of this, that is 0.04 rem per year in bone. In bone marrow the corresponding dose rate (due to Sr^{90} in the bone) would be about one-third of this value or roughly 0.015 rem per year. Therefore, the annual increase in the number of cases of leukemia in this country attributable to the permissible population limit of Sr^{90} suggested by ICRP, would be less than $\frac{4000}{4} \times 0.015 = 15$ cases, according to the method of calculation outlined above.

The fallout problem is of vital interest to this country and should be considered on its own merits, without reference to recommendations that were not meant to apply to it in the first place. It is proper for scientists to estimate the hazards and to be conservative in making the estimates. It is also incumbent on them to point out the uncertainties involved in such estimates. On the other hand, careful estimates of the benefits to the country to be derived from additional nuclear weapons tests must be made also. The balance between risks and benefits (both of which are numerous and diverse) must be made by those responsible for the safety and future of the country, with the advice of experts in all pertinent fields.

Dr. FAILLA. Then I will limit my presentation to the minimum and then I will be available to answer any questions that may be asked.

Representative HOLIFIELD. You may proceed in your own way.

Dr. FAILLA. The question of permissible limits has been raised many times, how they are set up and why have they been lowered three times in the last 25 years or so. I think perhaps I can contribute some information on that.

Originally the realization of the dangers of radiation came from effects that were produced in a short time. Say, burns of the skin or the face, which appeared in a few weeks or a few months after the exposure to X-rays in the very early days of radiology. Obviously the late effects could not have been discovered until enough time had elapsed for them to manifest themselves. So in the beginning the protection problem was simply one of preventing these injuries that appeared early. Gradually, of course, the realization came about that there were late effects and so the protection measures were improved and more care was exercised. But then when you get to that point, you could not tell any more what effect these low level exposures had on the individuals because the effects were too low to show up in the individual in a short time.

In the meantime from general experience and radiobiological experiments and also theoretical consideration, it was realized that it was wise to keep exposure to radiation as low as possible. So the so-called permissible limits were lowered before there was any evidence or there had been any chance of getting evidence that the higher values that had been recommended before had really produced any harm.

Representative HOLIFIELD. In other words, this was an exercise of prudent caution and not based on provable laboratory experiments.

Dr. FAILLA. In part it was based on some laboratory experiments with animals but not with humans, obviously.

Representative HOLIFIELD. And not at that rate?

Dr. FAILLA. Not at that rate, no. This actually has been going on for quite some time and the latest reduction was made in 1956 at the meeting of the International Commission on Radiological Protection which took place in Geneva.

Representative HOLIFIELD. Will you trace the various levels at which they have placed this maximum permissible dose?

Representative VAN ZANDT. What year was the meeting you referred to, Doctor?

Dr. FAILLA. 1956.

Representative HOLIFIELD. Put the rates and the years on the board, Doctor.

Senator GORE. That is a dangerous year. That was the year in which certain proposals were referred to as a theatrical gesture.

Dr. FAILLA. I didn't get that last remark.

Senator GORE. I think I am out of your field.

Dr. FAILLA. I think I am out of yours.

Senator GORE. You have always been out of mine.

Dr. FAILLA. I may not remember the dates exactly, but I think it was in 1934 that the permissible level (in terms of the dose in 1 year) was something like 60 rems per year. Incidentally, at that time the rem did not exist because we were dealing almost entirely with X-rays and gamma rays. In 1936, I think, was the first time that the Na-

tional Committee on Radiation Protection lowered the limit to half of that, namely 30 rems.

Representative HOLIFIELD. That is 30 roentgens in 1 year?

Dr. FAILLA. That is right. This is per year. Then the war came and during the war this was the figure that we went by on the Manhattan Project, in radiology and in other applications of radiation. The next drop came in 1948, when the National Committee on Radiation Protection lowered the limit to 15. It was given in terms of three-tenths r per week, which makes it roughly 15 rems per year. The next drop came in 1956 and by a factor of 3 on the average, which brings it to 5 rems per year for whole body exposure.

Representative HOLIFIELD. This is occupational.

Dr. FAILLA. This is all for occupational exposure.

Representative HOLIFIELD. This is for men in the plants in occupations that use radioactive material?

Dr. FAILLA. That is right.

Representative HOLIFIELD. What is the comparison to the general population?

Dr. FAILLA. These committees, both international and national, at first were reluctant to make any recommendations that applied to the general population.

Representative HOLIFIELD. There are three fields. There is the occupational worker, the people in the neighborhood of the plants, and the world population.

Dr. FAILLA. That is right.

Representative HOLIFIELD. Will you cover that as near as you can?

Dr. FAILLA. I will. The first mention of a permissible dose applying people nonoccupationally exposed was made in our "Handbook 59," which was published in 1954. In that case the reference was an indirect one. It was stated that in order to avoid the exposure of children, so that by the time they had reached age 18 they would not have accumulated a large dose, or a significant dose in comparison to that which they might receive by occupational exposures, it was necessary to reduce the dose that these children in the neighborhood would receive to about one-tenth of what was permitted for occupational exposure. That was the first time there was any reference to anybody outside of a radiation area.

Representative HOLIFIELD. That was mainly for genetic protection rather than somatic?

Dr. FAILLA. No; that was actually for somatic effects. The idea was that if you allowed a child who lives near an X-ray machine in a doctor's office (and the radiation goes out of the window and reaches the room where this child may be, or apartment where he may be for the first 18 years), if you allowed him the amount of radiation permitted for workers, by the time he was 18 and he could work with radiation, he would have accumulated a large dose and, therefore, it would not be right to have him accumulate more dose in his occupation.

In order to prevent that sort of thing, something had to be done and this was the way in which it was done. If you lowered his exposure to one-tenth of what he is permitted later, per year——

Representative HOLIFIELD. In other words, that would be 0.5 roentgens.

Dr. FAILLA. At that time we were in the 15-rem range for occupational exposure.

Representative HOLIFIELD. It would be 1.5.

Dr. FAILLA. It would be 1.5.

Representative HOLIFIELD. That would be in the environment near the plant and would not apply to the world population.

Dr. FAILLA. That is right.

Representative HOLIFIELD. Now the maximum permissible level is five-tenths, in the neighborhood, per year.

Dr. FAILLA. That is right. We maintain the same idea now in the ICRP report, but made it definitely for people in the neighborhood of a radiation installation. For the general population, the controlling factor is the genetic damage. The ICRP adopted the 10 roentgens recommended by the Genetics Committee of the National Academy of Sciences for the genetic dose. That is, an average dose of 10 roentgens in 30 years to the gonads for the general population. The ICRP adopted that value, but in addition to that made an apportionment so that no one group of individuals, industrialists, or whatnot could say we are going to use up the 10 roentgens and therefore we have lots of leeway. The apportionment was made in the following way:

Studies had been made in some countries indicating that medical exposure could amount to something like 4½ or 5 roentgens on the average for the whole population as the dose to the gonads in 30 years. Obviously if that is the situation we cannot do anything about it immediately, except to put pressure on the medical profession to reduce that. But we couldn't say from now on you can't use X-rays for medical purposes. It is not the purpose of the Commission to lay down any laws. The Commission only makes recommendations. We assumed then that in the advanced countries where X-rays are used extensively, about 5 roentgens in 30 years would be used up by medical procedures. Therefore, for other purposes, for the expansion of atomic energy, powerplants, and applications of isotopes, radioactive sources, and so on, and waste disposal, there would be available only 5 rems in 30 years.

We divided that up as follows: For occupational exposure we assigned 1 rem of the genetic dose of 5 rems; 0.5 rem for people in the vicinity of plants; 2 rems for the general population; and 1.5 rems (which is the balance of the 5) we kept as a reserve in the sense that these figures were given as recommendations to design plants and to work out methods of waste disposal, and so on, and there might be unknown factors.

Representative HOLIFIELD. Including fallout?

Dr. FAILLA. No; not including fallout at all.

Representative HOLIFIELD. That 1.5 reserve is not for fallout?

Dr. FAILLA. No. Assuming that perhaps some mistakes were made in these estimates and calculations, you still had 1.5 to go by, and would still remain below the 5. The ICRP has never considered the problem of fallout as coming within its province. It is a special problem, and if you bring it up in an international commission you immediately run into the difficulty of political angles, and so on. Actually, it has never been mentioned. These levels are set by the International Commission on Radiological Protection and are not intended to apply

to fallout at all. It is obvious that people are going to use these levels and make comparisons, and we have no objection to that. However, it should be borne in mind that the conditions envisaged by ICRP are quite different from those that apply to fallout. We are thinking of the very distant future and we make recommendations that are very conservative, according to our present knowledge, especially in the case of internal emitters, because there is not really a good way of determining how much an individual has in his body.

As far as the peaceful applications of atomic energy are concerned, the strontium problem would not be a serious problem at all because it would take a long time to accumulate from even waste disposal as much as there is on the surface of the earth now from fallout. So there is always a possibility of changing these values, and the Commission, as you see, has changed these values from time to time. When you proceed at a slow pace it is always possible to do this. Therefore, in the case of the internal emitters especially we were particularly careful to recommend low limits so that as we acquire more information we can raise the limits rather than lower them.

Representative HOLIFIELD. Would you tell us what the one-thirtieth number, which was refered to by the ICRP, means?

Dr. FAILLA. These values that I put on the board are for exposure of the whole body. When it comes to exposure of a single organ which can occur really only in the case of internal emitters that concentrate in one particular organ, such as strontium 90, we did not lower the permissible limit by the factor of three that is shown on the board. The reason for that is that obviously if you permit the whole body to be exposed to a certain dose in every one of its organs and tissues, the hazard is very much less if only one organ is being exposed. The factor of three doesn't mean anything in itself. But it was used simply because we lowered the value for whole body exposure by a factor of three and we left the other one as it was—that gives the base line for occupational exposure. Then the permissible dose per year would be about 15 rems for a single organ, where no other organ is irradiated. This does apply for ocupational exposure and includes any contribution made by whatever material concentrates in that organ and even includes radiation reaching that organ from the exterior.

Then we had to do something about the dose for the people in the vicinity and the dose for the general population. The dose for the general population, in the case of genetic effects, is fairly easy to define because there is pretty good agreement—at least there was before Dr. Russell's latest result—that it didn't make any difference whether a few people got a large dose or a larger number got a correspondingly smaller dose. But for somatic effects you can't very well get people to agree that there is no threshold and we do not know as much about the situation as perhaps we think we know about the genetic situation.

There was no way, really, of giving some average dose for the population that applies for the production of leukemia or bone tumors or some other type of effect. So the recommendtion was made in an indirect way, and what was suggested was that the value recommended for occupational exposure be reduced by a factor of 30, the value for occupational exposure being 15 rems per year, in a single

organ. Therefore, dividing that by 30 you get the annual dose of 0.5 rems at the permissible dose for a single organ in the average individual of the population.

I am sorry, I didn't mean to say that. That is the maximum for an individual and not the average. The distinction between what was done for the genetic dose and what was done for the somatic dose is exactly that. In one case you could give an average for the whole population and in the other case you couldn't. Therefore, that five-tenths is not for the average individual in the population but rather for a single individual in the population. Actually the wording of the recommendation is not very clear. As a matter of fact, Dr. Morgan has given it a different interpretation than I have given. This will be clarified at the next meeting of the Commission, which is going to take place in July.

Representative HOLIFIELD. Could I sum up what you said, as I understand it? Both the National Committee on Radiation Protection and the International Commission on Radiological Protection have within the past year revised their recommendations with regard to permissible internal doses and have come up with almost identical tables for strontium 90.

Dr. FAILLA. Yes.

Representative HOLIFIELD. Both of those have recommended that for external exposure one-tenth of the present occupational exposure for workers be used as the permissible level for populations living near atomic installations.

Dr. FAILLA. Yes.

Representative HOLIFIELD. This is mainly to take care of population genetic effects.

Dr. FAILLA. Not in the case of strontium, no. This would be for somatic effects.

Representative HOLIFIELD. In addition to those two levels on which they pretty well agree, the International Commission has for the first time this year recommended permissible levels for world population.

Dr. FAILLA. That is right.

Representative HOLIFIELD. And these are as follows:

Internal dose for internal permissible levels for the world population for water, food, and air, it is recommended that one one-hundredth of the internal permissible levels for workers be used for isotopes such as cesium, which will have genetic effects.

Dr. FAILLA. Yes, that is right.

Representative HOLIFIELD. For the internal dose it was recommended that for isotopes such as strontium 90 where the effects are mainly somatic as opposed to genetic, that one-thirtieth of the permissible occupational level for workers should be used.

Dr. FAILLA. Yes. May I interrupt?

Representative HOLIFIELD. Yes.

Dr. FAILLA. There is some confusion at the present time because the recommendation that was made is not very specific.

Representative HOLIFIELD. I noticed that.

Dr. FAILLA. Whether average refers to average among the people in the population or average with respect to time. It is necessary also to average with respect to time.

Represenative HOLIFIELD. They are studying that now and will come up later with a clarification, as I understand.

Dr. FAILLA. Yes.

Representative HOLIFIELD. Did I make a mistake when I said water, food, and air? Should food be eliminated from that in the internal dose of one one-hundredth?

Dr. FAILLA. Yes. Only two, water and air, are mentioned in the ICRP, or the NCRP report. The permissible concentrations are given only for water and air. What they are intended for is to limit the intake of the material into the body so that it will not exceed the body burden that is stated in the same table.

Representative HOLIFIELD. How can they exclude food from that?

Dr. FAILLA. They don't exclude food because if you have food containing this material then you have to go according to the intake of food, the kind of food, how much the food contains and so on. But you can do it in terms of either the air of the water because that gives you the amount of radioactive material that you can take in either through the lungs or through the digestive tract. The value for air and the value for water do not apply simultaneously because that will give you too much. So you always have to work it out in terms of the corresponding amount of radioactive material.

Let us say that the intake of water in man averages 3 quarts or a quart and a half, or whatever it is per day. Then if you allow so much radioactive phosphorous to be in a quart of water, it is assumed that the permissible limit will not be exceeded. That means that the same amount of phosphorous now can be taken through something else if you drink pure water.

Representative HOLIFIELD. Then the one one-hundredths level of internal permissible dose would be for anything that you either breathe or swallow, you might say.

Dr. FAILLA. Yes. It doesn't make any difference how you take it in, but you can't exceed a certain amount of material per day or per week.

Representative HOLIFIELD. Dr. Crow, do you have anything to add to Dr. Failla's discussion?

STATEMENT OF DR. JAMES CROW,[1] UNIVERSITY OF WISCONSIN

Dr. CROW. I have a prepared statement, but I don't think I will read it.

Representative HOLIFIELD. Will you summarize it?

Dr. CROW. Yes. I would like to say a few things.

Representative HOLIFIELD. It will appear in the record in full.

(The statement referred to follows:)

STATEMENT OF DR. JAMES F. CROW, UNIVERSITY OF WISCONSIN, MAY 7

In the past the National Committee on Radiation Protection and the International Commission on Radiological Protection have concerned themselves mainly with maximum permissible concentrations in occupationally exposed groups. Recently there has been increasing concern with the effects of wide scale distribution of small doses of radiation over entire populations. It is this problem that I wish to discuss.

[1] See biographical sketch, p. 1563.

It is generally agreed that for genetic effects there is no threshold—no dose that is completely "safe." Any radiation that reaches the reproductive cells is thought to entail a risk roughly proportional to the amount of radiation. The interval between the occurrence of a mutation and a specific harmful effect due to that mutation may be several generations. Therefore, the way in which the radiation is distributed over the population—whether a small number of persons receive a large amount or a large number receive a correspondingly smaller amount—makes little difference. The effect on future generations depends mainly on the total (or the average) dose received by the population.

For these reasons the recommended maximum permissible dose to the population is best given as an average amount. It is not thought to be serious if some individuals exceed the average, as must certainly be the case for example with some necessary medical procedures, as long as the overall average is low.

The National Academy of Sciences Committee on the Genetic Effects of Radiations, The National Committee on Radiation Protection, and the International Commission on Radiological Protection have made almost identical recommendations with respect to the genetic dose. The National Academy of Sciences Committee has recommended that the amount of radiation to the reproductive cells be restricted so that the *average* dose for the population be less than 10 roentgens in the period between birth and reproduction, a period of about 30 years on the average. This includes radiation from medical and dental sources. If this is taken as 5 roentgens, which is close to some estimates for the United States, then there remain 5 roentgens for radiation due to industrial and other applications of nuclear energy. This is not far from the background radiation level of 3 roentgens. The hope, of course, is that the actual levels of radiation received by the population will be considerably below this.

For somatic effects of radiation—effects on the individual receiving the radiation, not his descendants—there remains considerable uncertainty as to whether there is or is not a threshold below which no effect is produced. Many of the animal experiments at high doses have been interpreted as favoring a threshold interpretation. On the other hand, there is some evidence for production of leukemia by very low doses. I think at present it is best to regard the question as unsettled.

In this state of affairs, as long as there remains the distinct possibility that effects can be produced by very low doses, it is only prudent, I believe, to *assume* that there is no threshold and plan protective measures accordingly. If later it is discovered that this was in error, we have simply been overcautious; this is far preferable to the opposite error of under estimating the effect and discovering the mistake too late.

If we make this assumption, we must consider the possibility that very low doses do entail some risk of somatic damage. As with the genetic risk, we must eventually strike a balance between the risk associated with the radiation and the expected benefits to the individual or to society from the use of the radiation.

A maximum permissible dose is necessarily arbitrary. The level cannot be set low enough to remove all risk. Therefore, one tries to find a dose level which, though not absolutely safe, can be tolerated and for which there is reasonable expectation that the benefits outweigh the risk.

At present it is not possible to weigh benefits against risks for low-dose somatic effect. We don't know with any precision what the risks are, nor can we express the benefits quantitatively.

In the absence of this information, I believe the wisest course of action is to try to keep the population dose level somewhere near the background level of natural radiation.

There are several reasons for this. One reason is that this can be relatively accurately measured and does not change with time. More important, this is a level of radiation to which the human population has been exposed throughout its history. The further we get from this level, the less confidence we have that any effects will be similar in kind and quantity to those the population has experienced in the past and been able to tolerate.

The present background level differs somewhat with altitude and geology, but the average is approximately 0.1 roentgens per year.

I am not saying that the maximum permissible level should be set at 0.1 roentgens per year; it might be somewhat lower or higher. But I believe it should not be set much higher than 0.1 roentgen per year without a careful consideration of the validity of the reasons for having a higher level and the expected benefits to the individual or society from a higher dose.

I have not said anything about medical and dental radiation with respect to somatic effects. The main reason is that in this case the benefit and the risk occur to the same person. Furthermore, there are medical circumstances when it is clearly desirable to go beyond the maximum permissible dose. Of course, proper shielding and other precautions should be taken to keep the dose as low as is reasonable.

There are many difficulties in determining somatic doses. Some radioactive compounds such as cesium give quite a uniform radiation throughout the body. Others tend to concentrate in certain organs, such as strontium in bone and iodine in the thyroid. Some elements decay rapidly or are eliminated from the body so that all the radiation is given quickly, others remain and give off radiation for long periods of time. Some may be more effective in different age groups; for example, young children may under some circumstances take up larger amounts and, therefore, receive heavier radiation.

It is necessary to realize further that it is not practical to set a maximum permissible dose and have any effective way of monitoring this by measuring individuals. We obviously cannot measure the strontium content in everybody's bones. This means that any control measures will need to be directed at the environment. Maximum permissible doses would have to be determined for such factors as air, water, and milk such that the amount of radiation received by a person in this area will be below the permissible dose. Recommendations regarding levels in the environment will require revision as new information becomes available.

One reason for prompt determination of environmental standards is for purposes of planning for industrial applications of nuclear energy. It is important that no one source of radiation become large enough that other developments are restricted.

To summarize: The National Academy of Sciences Committee and the National Committee on Radiation Protection have made recommendations regarding the average genetically effective dose, based on the generally accepted assumption that no dose, however low, is entirely free of risk. The evidence for somatic effects (effects on the same individual as received the radiation, not his descendants) is far from clear as to whether there is or is not a threshold below which no effect is produced. Until the question is answered, the only prudent course, I believe, is to *assume* that there is no threshold. The natural background level of radiation is one that the human population has been exposed to presumably throughout its entire history and has been able to tolerate. I suggest that it be used as a standard to which a maximum permissible somatic dose is related.

Dr. CROW. In fact, I would rather read the pertinent parts. I am going to devote myself entirely to the population dose question, rather than the industrial.

Representative HOLIFIELD. Is that population near the plant or the world population?

Dr. CROW. Population at large.

Representative HOLIFIELD. The world population.

Dr. CROW. Yes. It is generally assumed, still assumed, I think, that for genetic effects there is no threshold. For this reason, the recommended maximum permissible dose to the population is best given as an average amount. It is not thought serious if some individuals exceed the average, as must certainly be the case with some necessary medical procedures as long as the overall average is low.

The National Academy of Sciences Committee on the Genetic Effects of Radiations, the National Committee on Radiation Protection, and the International Commission on Radiological Protection have made almost identical recommendations with respect to the genetic dose. The National Academy of Sciences Committee has recommended that the amount of radiation to the reproductive cells be restricted so that the average dose for the population be less than 10 roentgens in the period between birth and reproduction, a period of

about 30 years on the average. This includes radiation from medical and dental sources. If this is taken as 5 roentgens, which is close to some estimates for the United States, then there remain 5 roentgens for radiation due to industrial and other applications of nuclear energy. This is not far from the background radiation level of 3 roentgens. The hope, of course, is that the actual levels of radiation received by the population will be considerably below this.

The NCRP has become recently concerned with the question of setting permissible somatic doses for the general population. To this end it has appointed an ad hoc committee of which I am a member to make recommendations regarding the principles to be used in setting such a dose. This committee has not yet submitted its report in final form, but its general conclusions are agreed upon, and these I should like to briefly report.

For somatic effects of radiation, there remains considerable uncertainty as has been abundantly clear in this hearing as to whether there is or is not a threshold. Many of the animal experiments at high doses have been favoring threshold, and on the other hand, there is some evidence for production of leukemia at low doses. I think at present it is best to regard the question as unsettled in this state of affairs that there remains a possibility that some effects can be produced by low doses. It is only prudent to believe that there is no threshold and plan protective measures accordingly. If later this is discovered to be an error, we have simply been overcautious. This is far preferable to the opposite error of underestimating the effect and discovering the error too late.

If we make this assumption, we must admit the possibility that some low doses admit damage. We must eventually strike a balance between the risks associated with the radiation and the expected benefits to the individual or society from the use of radiation. A maximum permissible does is necessarily arbitrary. On this hypothesis the situation does not change suddenly from safe to harmful as the permissible level is passed. The level cannot be set low enough to remove all risk. Therefore one tries to find a dose level which though not absolutely safe can be tolerated and for which there is a reasonable expectation that the benefits outweigh the risk.

At present it is not possible to weigh benefits against risk for low dose somatic effects. We don't know with any precision what the risks are nor can we express the benefits quantitatively. In the absence of this information the committee believes that the wisest course of action is to try to keep the population dose level of manmade radiation exclusive of medical radiation somewhere near the background level of natural radiation. That is, keep the dose of manmade radiation on top of the preexisting background level.

There are several reasons for this. One is that this can be relatively accurately measured and does not change with time. More important, this is the level of radiation to which the human population has been exposed throughout its history. The further we get from this level, the less confidence we have that any effects will be similar in kind and quantity to those that the population has experienced in the past and has been able to tolerate.

Representative HOLIFIELD. Does this constitute a complete new approach to the formula for setting the maximum permissible concen-

tration? Heretofore you have worked back from an occupational hazard to the amount you allow the full population. Now, in the place of taking an occupational hazard and working back, you are taking the background radiation to which mankind is exposed and using that as your base.

Dr. CROW. That is correct.

Representative HOLIFIELD. Is this a completely new approach?

Dr. CROW. It is a new approach. I think it is unfair to the ICPR to say it is a totally new idea. This has been kicked around a great deal.

Representative HOLIFIELD. I don't say it is a new idea. This is the first time I have heard it expounded. To me it seems based on something that is real and tangible and has been in existence for many years, rather than an arbitrary situation that may exist in an industrial plant.

Dr. CROW. That is the committee's view as well. We are not saying that the maximum permissible level exclusive of natural radiation and that for medical purposes should be set at exactly one background. It may be somewhat higher. But we believe it should not be set much higher than the background level per year without a careful consideration of the validity of the reasons for having the higher level and the expected benefits to the individual or society from a higher dose.

I should like to note that this recommendation is an essential quantitative agreement with the ICRP, although derived in a different way, as you implied. Dr. Failla has told us that for strontium the ICRP recommends 0.2 r per year or 0.5 r per year, depending on which way you calculate it, which is of the same order as the background level.

Representative HOLIFIELD. Your background level is 0.7 of a roentgen per year.

Dr. CROW. That is right.

Representative HOLIFIELD. Or 1 roentgen over a 70-year lifetime period.

Dr. CROW. That is correct. We have not said anything about medical and dental radiation with respect to somatic effects. The main reason is that in this case the benefit and the risk occur to the same period. Furthermore, there are medical circumstances where it is clearly desirable to go beyond the maximum permissible dose. Of course, proper shielding and other precautions should be taken to keep the doses as low as feasible. There are many difficulties in determining somatic doses for the individual. Some radioactive compounds such as cesium give uniform radiation throughout the body. Others are concentrated in particular organs. Some may be more effective in the different age groups. It is necessary to realize further that it is not practical to set a maximum permissible dose and have any effective way of monitoring this by measuring individuals. We obviously cannot measure the strontium content in everybody's bones. This means that my control measures will need to be directed at the environment.

Maximum permissible doses would have to be determined for such factors as air, water, and milk, such that the amount of radiation received by a person will be below the permissible dose. Recommendations regarding levels in the environment will therefore require revision as new estimates become available.

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Representative HOLIFIELD. I think that is a very fine and clear statement. Are there any questions of Dr. Crow?

Representative WESTLAND. I would like to question Dr. Failla on the figures he has on the board. There is quite a change in the picture from 60 rems in 1934 to 5 rems today.

Dr. FAILLA. Yes.

Representative WESTLAND. I presume the reasons for this change were good, and that they were based on research and perhaps the study of subjects. I would like to ask you this. Are there any people or animals that were subject to the 60 rems in 1934, or 30 rems in 1936, which have been under study since then? It is now almost 25 years later. So that any estimate could be made of the damage that was done to those peeple and those animals, or was there any damage?

Dr. FAILLA. During the days of the Manhattan project, a great deal of work was done at levels in the neighborhood of 60 or 30 r per year. We used to talk in terms of one-tenth r per day at that time. Lorenz at the National Institutes of Health carried out extensive experiments with mice at levels in that neighborhood and higher. Also, in Rochester they carried out experiments on rats and guinea pigs and dogs in that level of exposure. Boche made an extrapolation that indicated that even at the low daily doses there could be a shortening of the lifespan, but experimentally there was no shortening of life span for the one-tenth r per day exposures. As a matter of fact, Lorenz' mice had a lengthening of the lifespan as compared to the controls. But the controls were not very good, actually. So the answer was not significant statistically. It simply meant that you could not tell the difference under the conditions in which the experiment was made. Whether one-tenth r per day did or did not decrease the lifespan of the mice was not determined.

Representative WESTLAND. But people were exposed to this 30 rems per year, were they not?

Dr. FAILLA. Yes.

Representative WESTLAND. Or 60 rems?

Dr. FAILLA. As I pointed out before, the effects about which we are worrying are latent effects that do not appear for maybe 25 or more years. Therefore, they could not be detected immediately, and, in fact, they cannot be detected except by statistical studies. There have been statistical studies carried out on radiologists and technicians who were exposed to X-rays and radium in the old days as to the lifespan, and as to the incidence of leukemia. In fact, in one study that was carried out by Dublin and Spiegelman, I think, the total incidence of cancer in radiologists was somewhat higher, but not significantly higher than in other medical specialists.

The difficulty is that you have to wait so long before you can find out what happens to the individual. In the meantime, because of the general feeling that radiation was bad, there was no such thing as a good dose of radiation unless you were sick and had cancer or something of that sort in which case you received very large doses of radiation. The tendency has been to increase the protection gradually. Therefore, there has not been any long period of time where indi-

viduals have been exposed at the same rate and that you have had the chance to observe what has happened to those individuals.

Representative WESTLAND. You have had since the time of the Manhattan project, Doctor, have you not, to observe people who were exposed to 30 rems.

Dr. FAILLA. We must distinguish now between a recommended limit and what people actually receive.

Representative WESTLAND. I am going back to your figures.

Dr. FAILLA. That is a very good point. That was the maximum permissible dose. That does not mean that the workers that worked in the plants during the days of the Manhattan project or at the present time get anywhere near that amount of radiation on the average. There may be a few that get the maximum permissible dose accumulated over a period of a year. On the average they receive less. In fact, the great majority receive less than one-tenth of that amount. In those individuals you do not see any changes whatsoever. The only way in which you can find out what is happening to those individuals is to wait 25, 30, or 40 years, and then make a statistical study and compare them with another group that have lived under similar circumstances but not exposed to radiation.

Representative WESTLAND. In other words, in another 10 years or so you will probably have some real figures to go on.

Dr. FAILLA. I don't think so. I don't think you will ever have any figures to go on any more than you have today because of the fact that the permissible limits are so low now that you can't possibly detect any changes. The statistical error involved is going to be so large in comparison to what you might expect to find, you will never be able to tell.

Representative HOLIFIELD. In other words, you think the permissible levels have now been set so low that for all practical purposes they won't be changed?

Dr. FAILLA. Yes. I think that we have essentially touched the bottom.

Representative HOLIFIELD. You have gotten them so low that you have a cushion of safety involved that you think is adequate.

Dr. FAILLA. Yes. As I pointed out before, when you set the maximum permissible limit that is for any individual. Obviously on the average the individual is going to get very much less than that. So you would never get a very large group that has received this amount of radiation.

Representative HOLIFIELD. This is very interesting. I think we can go on but it is 15 minutes after 4, so at this time I am going to ask the rest of the panel to come forward.

We will now have a roundtable on permissible exposure levels in relation to weapons testing. I think I will start off by picking up the Gamarekian article, and ask Dr. Morgan to give us an interpretation of that article of the Subcommittee on Internal Dosage. You are the chairman, I understand, of the Subcommittee on Internal Dosage of both agencies. Will you give us your evaluation of this article, and the methods of computation used in the article, Dr. Morgan?

ROUNDTABLE ON PERMISSIBLE EXPOSURE LEVELS IN RELATION TO WEAPONS TESTING. PARTICIPANTS: DR. G. FAILLA, DR. JAMES CROW, DR. CLINTON POWELL, DR. K. Z. MORGAN, DR. FORREST WESTERN, DR. JACK SCHUBERT, MR. LAURISTON TAYLOR

Dr. MORGAN. Mr. Holifield, I glanced at the first part of the article and didn't agree with the argument so I didn't read all of it.

Representative HOLIFIELD. Since that time have you had an opportunity to read the article?

Dr. MORGAN. No, I have not. If you like, I could give you my method of calculation of permissible dose and what is used in the international handbook.

Representative HOLIFIELD. I think that the record should show that. After you give the record of your computation from the formula that is put forth in the book that Mr. Gamarekian used, table 5 of the Health and Safety Laboratory, April 20, Atomic Energy Commission office, data on strontium 90 content in milk and other food, prepared for this hearing, give us your interpretation of it, please.

Dr. MORGAN. May I go to the blackboard?

Representative HOLIFIELD. Yes, sir.

Dr. MORGAN. I was not at the hearings earlier, but I have gotten hold of this reference (see p. 1252) that I believe was used, and picked out the largest figure of radioactive contamination of bread that I could find. I believe this ($198 \pm 4 \mu\mu c$ Sr^{90}/Kg) is the one that Mr. Gamarekian was referring to.

First of all, I would like to give the calculation and indicate how I as a member of the ICRP and as chairman of the Committee on Internal Dose (that has recently prepared this internal dose manuscript for publication) would apply it.

First of all, the ICRP value applies for continuous exposure—and I would like to underline this word "continuous" exposure—for occupational workers. It is given in the new handbook as 10^{-6} microcuries of strontium 90 per cc. of water. This assumes that a person consumes a total of 2,200 cc. of water per day.

The International Commission on Radiological Protection suggests the factor of $\frac{1}{30}$ as a multiplying factor to be applied as a guide when using the occupational values for the population at large. That is, except for the case of gonad seekers (and strontium is not a gonad seeker) we can multiply the continuous occupational MPC values by $\frac{1}{30}$. If we multiply this 10^{-6} by $\frac{1}{30}$ it gives us approximately 3×10^{-8} microcuries per cc. of the strontium 90 for water. But this again is for 2,200 cc.'s per day, continuous exposure for the population at large.

I don't know how much bread the average man consumes, but as Dr. Failla has already indicated, the values given in these internal handbooks take the figure of 2,200 cc.'s per day which is the average of all the consumption of water of the average man. The water contained in food, water contained in coffee and other drinks, the entire intake of water, even from what you inhale is included in this 2,200 cc. So if one were to assume that on the average man consumes—to make the arithmetic simple—220 grams of bread per day, and if we multiply this ratio of 2200/220 by 3×10^{-8} microcuries per cc. we get the figure of 3×10^{-7} microcuries per cc. of bread.

If one takes the highest figure I could find from this reference (Hasl 9519), for Brookview, Ill., bread, a figure of 198 micromicrocuries of strontium 90 per kilograms of bread with a very low probable error of ± 4 $\mu\mu$c and convert the units, this would be approximately equal to 2×10^{-7} microcuries of strontium 90 per cc. of bread. This figure, you see, then is 66 percent or is two-thirds of what I would calculate to be the maximum permissible value.

Representative HOLIFIELD. How does that compare with Mr. Gamarekian's figures?

Dr. MORGAN. I believe, sir, he found that the maximum in the bread exceeded the maximum permissible value suggested by the International Commission. According to my estimates the maximum value I can find is less than that recommended or suggested by the International Commission.

Representative VAN ZANDT. Did you say two-thirds of the maximum permissible dose, Doctor?

Dr. MORGAN. That is right.

Representative HOLIFIELD. That is somewhat different from four times the maximum permissible dose.

Dr. MORGAN. I would like to make this one further point that I think is even more pertinent to the issue—and here there is some variance of interpretation; Dr. Failla, as one member of the International Commission, gave his view of something that was not specifically stated in the International Commission report and I as another member, will give mine. I consider that these MPC values for the population at large are average values and not maximum values. This is what they would have to be. This is the way we have treated them in the internal dose text of the International Commission report which has gone to publication.

Representative HOLIFIELD. May I ask one question before you go to the next illustration? Does the panel substantially agree with the figures Dr. Morgan gave on the board, or is there a variance of opinion? If there is we would like to have it expressed at this time. I see a gentleman in the audience, Dr. Selove. We would like to have you come forward.

Dr. SELOVE. I had a hard time behind the board hearing what was going on. I have one question. I did not understand one calculation that Dr. Morgan made. Perhaps I could ask him this question. If I understood the calculation you made for the permissible level of strontium 90 in bread, you first had the number which represented the permissible number of microcuries or whatever units of strontium 90 in 2,200 cc's of water, and then said that since a typical bread intake was 220 grams instead of 2,200, you used a 10 times larger number of micromicrocuries per gram as the permissible level in bread, is that correct?

Dr. MORGAN. That is correct.

Dr. SELOVE. My understanding of this then would be that if a man obtains strontium 90 only from bread and ate 220 grams of bread and no strontium 90 from any other food, we would then in this interpretation obtain a maximum permissible amount of strontium 90, is that correct?

Dr. MORGAN. That is right.

Dr. SELOVE. It would seem to me that would not be the most reasonable way to set a permissible level for bread because one should consider the possibility that other foods also would contain strontium 90

Dr. POWELL. Mr. Chairman, I would like to say that I was about to make somewhat the same comment. This does not disagree with Dr. Morgan's calculation, but relates to its interpretation. I think what he has derived is the percentage of the total daily intake of strontium which could come from the bread eaten by an individual and therefore assumes, if one is talking about two-thirds of a maximum permissible amount, that one had no other strontium intake, which is perhaps not a likely situation.

Dr. MORGAN. This point is very well taken and has been considered in the international reports. What we do there is calculate the permissible concentration for each item of exposure, not only for bread and for water but also for external exposure to neutrons and gammas. The criterion is that no organ of the body must exceed the permissible level that we set from all these sources. If I could just make one little drawing on the board, I think it would further emphasize the point. If we were to plot a graph and let this [a straight line on the graph of dose rate versus time of exposure] be the average background dose rate which the International Commission has taken as 4.5 rems to the age of 30, then above this 4.5 line we have the maximum (a value of 8.5 rem/30 years) reached by eating for one's entire life the highly active bread at Brookview, Ill. We will mark on the graph this line as 8.5 rem/30 years reached after a lifetime exposure. At this level we have the maximum permissible value that is set for bone [pointing to a line on the graph at 10.5 rem/30 years, or 6 rem/30 years above background]. This figure would then be 6 rems above the background. Then up here, 15 rems above background, we have the average permissible level for bone marrow, which might actually be the more important tissue if we are concerned primarily with leukemia or with some other bone diseases. So what we actually have are variations of this sort [pointing to typical variation shown on the graph] around the background with one particular case that has gone up this high [pointing to the short period rise on the graph due to Brookview bread] while the maximum permissible is still up here [pointing to the 10.5 rem/30-year figure]. If these permissible dose rates for the population at large are interpreted to apply to averages, we have never even gotten near the maximum permissible average for the population at large and only one small segment of the population would reach two-thirds of it if it consumed the Brookview bread at the highest measured value for a lifetime.

Representative HOLIFIELD. In other words, the significance of the measurement of the bread was not an average over a long period of time, but was apparently one incident?

Dr. MORGAN. It is not only one incident applying to one small segment of the population, but it is a high exposure for a small interval of time. According to my interpretation, we have to integrate over both time and numbers of people in getting the average permissible value.

Representative VAN ZANDT. Doctor, could we apply the term "hot-spot"?

Dr. MORGAN. Are you referring to hotspot with reference to the bone or with reference to a locality?

Representative VAN ZANDT. Locality.

Dr. MORGAN. My own interpretation is that occupational exposure itself is a hotspot. You permit higher doses to smaller groups. The probability of running into trouble at doses even a hundred times this average value for the population at large is quite low. I believe the problem we are concerned with here is trying to keep the average dose as close to background as we can in reference to the population at large.

Representative HOLIFIELD. The thing that we are trying to do is to put this in proper perspective. The thing that alarms me about some of these newspaper articles is because it brings worry to people due to the fact that they do not understand the period of time involved, nor the very small measurements that are being used in this field, nor do they understand the cushion of protection below the actual permissible level and the level that it is usually considered that harm occurs.

Dr. MORGAN. Could I make one other statement?

Representative HOLIFIELD. Yes.

Dr. MORGAN. I would like to say as a health physicist our mission in life is to keep these exposures from fallout and all sources as low as we possibly can. On the other hand. I don't believe I can be a scientist or health physicist and not have some regard for medical exposure which is many, many times higher than this, and in many cases could be reduced materially to the population. How can we worry so much about the 1 percent due to fallout and ignore the 99 percent of the problem due to medical exposure? I wish that all medical groups would do as the radiologists have done recently, and try to reduce the exposures to their patients and others.

Representative HOLIFIELD. We will leave that now unless someone else wishes to comment on it, and let us get to the other points in our round table.

Dr. FAILLA. Mr. Chairman, may I make one comment?

Representative HOLIFIELD. Yes, Dr. Failla.

Dr. FAILLA. I think we have to consider these permissible limits in terms of what the tissues get and not what the earth contains or what the cow contains or some other thing. In other words, what we have to worry about is the concentration of radioactive material in a certain organ and how it gets there. The limit set in the table that Dr. Morgan has prepared, is based on estimates that if one takes in this radioactive material at a certain rate, for example in 2,200 cubic centimeters of water per day, the amount that will concentrate in a particular organ over a period of 50 years is not going to exceed what we consider today to be a reasonable dose.

Representative HOLIFIELD. Then it is a considered opinion that in one concentration of radioactivity of this type in bread in one area that does not necessarily mean that the body has absorbed more than the maximum permissible dose, because of the factor of discrimination, and because of the factor of time involved; is that right?

Dr. FAILLA. That is taken care of in arriving at a permissible concentration in water. All those factors are taken into account. I would say that whether you take the radioactive material in water

or in bread or vegetables or any other food is immaterial so long as the total amount that you take in does not exceed the amount that corresponds to the permissible concentration in water.

Representative HOLIFIELD. But that does not apply to 1 day or 1 week.

Dr. FAILLA. No.

Representative HOLIFIELD. It applies to a whole lifetime.

Dr. FAILLA. You have to average that over a lifetime. As a matter of fact, the assumption is that the maximum amount that the organ receives is not immediate, but after a long time of exposure. This depends on the half life of the material. If it has a short half life then that equilibrium amount is reached soon. But if it is a long half life material, such as strontium, it takes quite some time before you reach the so-called maximum permissible body burden at the rate that the intake occurs, if the permissible concentrations are not exceeded.

Representative HOLIFIELD. Is there any statement that anyone else wishes to make on the maximum permissible dose or do you want to rely on questions at this time?

Dr. WESTERN. In this very complex field, it is almost impossible to avoid oversimplifying the statements that we make. I think that almost every statement which has been made here this afternoon has been oversimplified and is subject to question. I would like to qualify some of the statements that have been made in the last few moments.

In the field of fallout, we have indicated quite frequently that at the present time our primary interest is in the hazard to young children. For one reason at least, if for no other, this is because it is in the young children that we find the highest concentrations of strontium 90. The length of time it takes for a young child to come into approximate equilibrium with his environment is not a period of many years. A child aged 2 or 3 years is in fair equilibrium with his environment already, to such an extent that we can use this term "equilibrium" accurately.

Perhaps some of my friends here will say that I am not using it accurately, but at least to a first approximation I think one can say this is true.

Second, following up the statement which I believe Dr. Failla made, that what we are interested in is the dose to the organ, it seems to me that we can bypass many more or less academic arguments by looking at what we are actually finding in children at this time. We have had reports, for example, of Dr. Kulp, showing that the concentrations in children as we now observe them are in the neighborhood of perhaps 3 percent of the more recent recommendations of the ICRP for the general population. Regarding the question of whether we are talking about averages or individuals, we find that if we look at individual specimens, they vary on the average by a factor of the order of two or three. So certainly there is one quite reliable answer, reliable to a greater extent than any answer that is based on a number of theoretical considerations to the question of whether or not the concentrations which we now have in the food supply are producing excessive doses to people. That answer is what we are actually seeing in the people.

Representative HOLIFIELD. Is there any comment on Dr. Western's statement? Dr. Schubert?

Dr. SCHUBERT. I am grateful to Dr. Western for putting so cogently some comments which I would like to read as part of a statement I had prepared in which I dealt exclusively with the problem of children in regard to general population exposure. I will skip parts of my statement because some of the points have already been made quite well this morning. Part of my statement is based on the contents of two articles attached to my statement, one on global radiation limits, which appeared in January 1958, and the other on fetal irradiation in relation to cancer deaths from fallout. The latter article will appear next month in the Bulletin of the Atomic Scientists.

I would like to emphasize that we are dealing with a new problem in the setting of permissible limits for large populations. This problem began in 1954. Until that time this was not a problem since we were only dealing with small numbers of people in occupations. The setting of a maximum permissible level must be based on that part of the population which is most likely to suffer from a given injury.

Insofar as cancer produced by radiation is concerned, the most susceptible segment of our population appears to be the unborn child. The MPL for large populations cannot be based wholely on average values of radiation exposure of the entire population, but attention should be given to that fraction of the most sensitive population whose radiation exposure exceeds the average values by two, three, and more as long as those who exceed the average constitute large numbers. In other words, if one person in a billion has more than the permissible limit, we cannot recommend permissible levels just for that one person. But if 10 percent of the population exceeds this limit, then this population is the limiting factor.

In terms of strontium 90, we have the problem that one can make a fair estimate of how many children—and I am concentrating on children since adults are, practically, not very important in the strontium 90 problem for populations—when you look at the values of strontium 90 in children, and utilizing the lognormal distribution as it is called, estimate quite closely from existing data how many children will have two times, three times and four times the average exposure. This was pointed out in a very nice article by a Norwegian scientist, E. Dahl, in 1957, and I have an English translation of his article which I would like to insert into the record.

Representative HOLIFIELD. Without objection, that may be done.

(The article referred to appears on p. 1706.)

Dr. SCHUBERT. According to this function, which I may add has nothing to do with whether we know anything about the mechanism of how strontium 90 gets into the bone, we have a way of estimating how many people exceed the average. If this function fits, then it works. According to the normal distribution which has been used hitherto, the number of children who would have twice the average exposure would be 1 in 700, and those who would have three times, would be 1 in 20 million. The lognormal prediction, using Dahl's figures, show that instead of 1 in 700 exceeding twice, we would have 5 percent—5 children out of 100—having twice the average value.

The number of children having three times the average value would be 0.4 of a percent or roughly 1 in 200, rather than 1 in 20 million.

This means that the limiting factor on which to base the permissible levels, when we are given average values of strontium 90 in children, is the fact that a good fraction of these children have three times the average. So if we take the rough value of 10 strontium units as the average value children will receive by 1967, according to Dr. Langham's figures, then we have to take into account that a good proportion of the children will have 30 strontium units, which is roughly half the present permissible level and some children, will have, according to the lognormal, 0.05 percent, or four times, the average, or 40 strontium units.

Representative HOLIFIELD. They would have four times what?

Dr. SCHUBERT. Of the average value.

Representative HOLIFIELD. But it will still be less than the permissible.

Dr. SCHUBERT. It depends upon how you define permissible.

Representative HOLIFIELD. If it is given as 66, which I believe is the figure, you are talking now about an occasional child having four units.

Dr. SCHUBERT. Not an occasional. I would say 1 child in 2,000 would have 40 according to this distribution.

It is possible to make an estimate, and I want to emphasize it is an estimate, of the cancer producing effects of fallout on children. This is based on the studies of Dr. Stewart and Mr. Hewitt, her statistician in England. In these studies roughly 2 roentgens total body dose in the fetus in the last 3 months of pregnancy resulted in a doubling of the amount of cancer that these children got before the age of 10. From these studies I have estimated that 20 milliroentgens of fallout radiation, which is roughly the present total dosage the fetus receives during the 9 months gestation period, will produce a 1 percent increase in numbers of children below the age of 10 who die from cancer each year. It is interesting that 2 roentgens would produce a doubling dose, for this reason: Before 1950 it was universally assumed that it took roughly 2,000 roentgens to produce cancer in humans. These data were based mainly on adults. Then in 1950 it was found that as little as 200 roentgens produced cancer in children who were irradiated in the neck region. Now, 7 years later, we find that two roentgens had produced cancer in children, admittedly on the fetus, which is the most sensitive age.

Representative HOLIFIELD. Of course, you are comparing three different things there. You are comparing different levels to adults and certainly very much smaller levels to the fetus.

Dr. SCHUBERT. Yes; that is exactly the point that I am making. The fetus, being the most sensitive to radiation, is the basis upon which the permissible levels for large populations must be based. At the present time the fetus is getting radiation from strontium 90 and other internal emitters and radiation that the mother receives just from walking on the sidewalk and on the soil from the fission products that are deposited on the ground.

In my technical paper that I have attached to my statement I have tabulated the sources from where the radiation from fallout comes. While the estimate given is subject to a wide uncertainty, the important point is that it now appears that radiation doses so low as to

approach the radiation levels in fallout and natural background may cause a significant increase in childhood cancer deaths.

As the amount of fallout increases, it is prudent to assume as Dr. Crow did, that the increase in childhood cancer deaths will increase proportionately. So that if the fallout dosage becomes 40 mr, we would have 2-percent increase in childhood cancer deaths, whereas the adults may not be affected to the same degree.

Another point I would like to mention is that it is sometimes not appreciated that the permissible level cited for a given radioactive isotope assumes that no other radiation exposure occurs except that from the specific isotope and the natural background. Thus, when the maximum permissible level of strontium 90 is stated to be 67 strontium units for the whole population, it must be kept in mind that it is impossible to receive radiation from strontium 90 alone. Since the maximum permissible levels are not additive, it is necessary to consider what I call a practical permissible limit which includes a rough approximation of the radiation received from all sources. The practical limit for strontium 90 for the entire population would be 34 strontium units, one-half of 67, half the present limit, since any person who receives 34 strontium units actually receives radiation from other fallout sources equivalent to roughly an additional 34 strontium units of radiation.

It appears reasonable that the maximum permissible level for large populations be related to the natural background radioactivity rather than to occupational. This is a point which I made in my January 1958 paper in which R. E. Lapp and I suggested that the maximum permissible level for large populations be set as some multiple of what we call a fixed natural background. This means that instead of having a baseline which varies from country to country as the occupational permissible limit, we would have a baseline which would be fixed for all time and in all countries would be identical and unvariant.

My final point is that in order to achieve the best possible estimates of the maximum permissible limit a new committee of scientists representing all the pertinent fields of science, such as genetics, biostatistics, medical science, physics, and so forth, should be created and the membership be chosen by some procedure recommended by an organization such as the National Academy of Sciences. I am pleased that Dr. Failla has made substantially the same recommendation in his printed statement. It is important such a committee, which must now deal with an entirely new problem, the radiation effects on the whole population, not be dominated by just a few fields of specialty. The National Committee on Radiation Protection has dealt many years with the problems of permissible levels before any of us, including myself, had ever considered that such a problem existed. However, in the past 5 years because of the newness of the problem that I just mentioned, it seems to me that the NCRP does not meet the scientific requirements which I have just stated, and its basis for selecting committee members is inappropriate to this new problem. Thank you.

Representative HOLIFIELD. Dr. Schubert, you have certainly given us some meat to chew on. I see several hands up. We will start with you, Mr. Taylor.

Mr. TAYLOR. I am interested in what Dr. Schubert has just said, because the committee on whose work Dr. Crow reported this afternoon meets every requirement that was just suggested by Dr. Schubert. This ad hoc committee was established last fall for the specific purpose of looking at population-exposure question, particularly from the somatic point of view. It does include all of the disciplines that were mentioned by Dr. Schubert and other disciplines. This committee has just completed its consideration of its report. In fact, I just received it this morning as I was starting down here. However, this will not be released until it has been subjected to review and approval by the NCRP as a whole.

I can't for a moment buy Dr. Schubert's suggestion that during the past 5 years the NCRP has lacked adequate disciplines among its membership. Its total membership is some 150 people. It includes geneticists, biologists, physicists, biophysicists, every discipline I can think of, including people with public health experience, atomic energy experience, and everything else. So I am quite willing to stand as far as I am personally concerned on the makeup and composition of that committee.

Dr. SCHUBERT. May I make a comment, sir?

Representative HOLIFIELD. Let us have some more comment from Mr. Taylor.

Mr. TAYLOR. I had some other remarks I wanted to make. If Dr. Schubert wants to remark on this point, this is all right with me.

Representative HOLIFIELD. Finish your remarks.

Mr. TAYLOR. My other remarks had not to do with the question Dr. Schubert just raised.

Representative HOLIFIELD. Go ahead.

Mr. TAYLOR. The other remarks have to do with the concern over some misunderstandings that appear to have developed partly as a result of these hearings and partly as a result of the hearings that were held a month or so ago, in which it has been implied that there are important disagreements between the recommendations and the work of the NCRP and ICRP. Some of the reports on these disagreements I think have been rather damaging.

I would like to make the categorical statement, although it has been brought out by many witnesses that I have heard during these hearings, that these disagreements do not in fact exist. It is a fact that, between these two groups working closely together, so far every anticipated need in the field of radiation protection has been met. These recommendations and developments by the committees have been made available to the public before the situation demanding them has even arisen. There has been an implication that this has not been the case.

In connection with the ICRP report upon which Dr. Failla reported, it has been implied that these population levels that were mentioned—these low population levels—were opposed by the representatives on the ICRP from this country. As a matter of fact, they were put forward by one of the U.S. representatives, principally Dr. Failla himself. They were supported by other representatives from this country.

There was also the implication that because the NCRP was making its own study of the population question, that it was in some way responsible for withholding the ICRP report from distribution in

this country. This is absolutely not the case. The only thing that withheld it is the slowness of sea transportation. As soon as it was published it was shipped by sea to this country and eventually arrived.

It was also implied that the NCRP opinions on this question were influenced by the possibility of adversely affecting our weapons testing position in this country. The NCRP and ICRP, as Dr. Failla explained, have never at any time considered those aspects of the radiation problem as such. They have never at any time considered problems of national security or individual security. The question of disagreement arises sometimes because these bodies necessarily work out of phase. One is an international body that meets once or maybe twice a year, either in part or in whole, as the ICRP. The NCRP in its various groups average 20 to 25 meetings a year. They are working at different rates. They are working on different problems. They are bound to be out of phase.

I outlined this in a letter to you a few days ago and with your permission I would like to include that material in the record of these hearings.

Representative HOLIFIELD. It will be included.

(The letter referred to appears in vol. 3, app. E, together with extracts from ICRP recommendations.)

Mr. TAYLOR. Also, because they bear on this general question, I would like to submit at a later time—I don't have them with me today—three other items. One is a copy of a lecture on this general subject, which I will give tomorrow night. (See p. 1737.)

Another is the news release that we put out at the time we completed our consideration of the NCRP and also ICRP versions of the new permissible dose standards. This is the report by Dr. Morgan's committee. (See vol. 3, app. K.) And if and when it is approved by the whole committee I would like to submit a copy of the report of the NCRP ad hoc committee which has been mentioned today.

Representative HOLIFIELD. It will be received.

(The report referred to had not been approved at the time of publication.)

Dr. POWELL. As a member of both the executive committee on the National Committee on Radiation Protection, and a member of the ad hoc committee which has been referred to, I would like to support Dr. Taylor in his statements. I think that much of the apparent misunderstanding as to relationships or disagreements between ICRP and NCRP has come about because of an unfortunate tendency to place too much significance on the exact number proposed. After all, these are judgmental decisions made within certain fairly broad limits. I think it is inevitable that there will be some minor differences in what different groups will decide. But these are minor and don't strike at the heart of the problem nor affect in my estimation the real significance.

It is important to emphasize this because I think the possibility is real that from time to time we will see individual samples of milk, food, or other environmental sources which carry contamination levels above the lifetime recommended maximum permissible levels. Perhaps, if weapons testing continues, we may even see an increase to a point where certain average levels in milk and food approximate the maximum permissible levels.

I would like to emphasize, for myself at least, that I don't feel it is possible for us to set our maximum permissible levels on the basis of circumstances or the need for weapons testing. I think we as scientists must make our recommendations. To use Dr. Dunham's excellent analogy of the automobile accident situation and its relationship to setting speed limits, one doesn't set a 50-mile-an-hour speed limit and then refuse to allow a policeman to chase a bank bandit at 60 miles an hour. Nor do you move the speed limit up because that situation arises occasionally. I think that the scientist has a responsibility to make recommendations concerning desirable maximum permissible levels, not directly related to the weapons testing problem.

Dr. CROW. I have a couple of comments. There are a few points in this committee report that I did not refer to for brevity of time but since Dr. Schubert has brought them up, I would like to mention them, because they are in complete agreement with his suggestion.

We, too, suggest an arbitrary definition of the background agreed upon. We suggested a tenth of an r per year with some multiple of this be used as the maximum permissible level. We have also suggested in this committee report that the permissible level be based on the most sensitive of the population rather than the total population iself. If there is a particular age group, e.g., infants or children, that are more sensitive, then that should be used as the basis.

I should also like to clarify that in this particular instance there is no ambiguity as to whether this is an average figure or an individual figure. Acting on the assumption that any maximum permissible dose for an individual person or for an organ cannot be determined directly by measurements of that individual but only indirectly by measurements of the environment, all one can do, until it is possible to make individual human measurements, is to set the environmental level in such a way that the average human being in that environment would not be expected to exceed the permissible level.

Furthermore, I think any recommendation should include the possibility of averaging dosages over a fairly long period of time so that a small episode that is soon to be over is not taken out of proportion to its importance.

Representative HOLIFIELD. Thank you. Dr. Failla.

Dr. FAILLA. Mr. Chairman, I would like to point out that the National Committee on Radiation Protection and the International Commission have not neglected the background as a basis for setting up permissible limits, because these factors of 30 or 10 or whatnot have been applied in order to bring the level down to something like background radiation. So while they have not been mentioned, actually the limits have been set on the basis of something comparable to background radiation.

Representative HOLIFIELD. If we can be pardoned for bragging a little bit, our analysis that was put out in June 1957 had this in it: "The reason natural background radiation levels are important is that they are used as a yardstick in evaluating the biological effects of radiation. Thus the National Academy of Sciences' summary report on the biological effects of atomic radiation and the report of

the Committee on Genetic Effects illustrates the use of natural background levels as a basis for estimating genetic doubling doses."

So we were on top of this subject 2 years ago.

Dr. Morgan.

Dr. MORGAN. I would just like to underscore what Dr. Taylor has said. First of all, as a member of the International Commission and the National Committee, both of these groups are made up of scientists that speak as individuals and not as representatives of countries or any companies, and the only disagreement is among individuals trying to arrive at decisions. As for one group disagreeing with another, I have never experienced that.

With reference to the way we arrived at this figure of 100, I would like to correct the record on that, and support what Dr. Failla said. I think as chairman of the Subcommittee on Internal Dose I was the first to actually use this in the printed reports. The way we arrived at it was this. The International Commission, as Dr. Failla has said, chose this figure of 5 rems in 30 years, which was essentially background radiation. They took one third of that for internal dose, 1.5 rems. They had also taken 5 rems per year as the permissible occupational level. If you multiply this 5 rems per year times 30, the only way to get this figure down to 1.5 rems, which is a third of background, is to divide it by a hundred. This figure of 100 or 1 percent did refer to background. That is the way it was gotten.

I would like also to submit my written testimony for the record, if I may.

Representative HOLIFIELD. It will be accepted.

(The statement referred to appears on p. —.)

Representative HOLIFIELD. Dr. Schubert, we have a few minutes for you to have a rebuttal.

Dr. SCHUBERT. The only thing I have to rebut is that the NCRP is a parent organization, and I think Dr. Taylor is referring to the numerous and generally well qualified subcommittees. I don't want to leave the impression that the individuals comprising the NCRP are not of the highest caliber. It is merely that a committee made up of all geneticists would be no more qualified than a committee made up of chemists or physicists. You have to have a wide variety. I think the parent organization should be the one that has the widest variety of disciplines represented since the subcommittees have to concentrate their scientific specialties on few fields.

Representative HOLIFIELD. Is it your position that the committee is not widely represented by all disciplines?

Dr. SCHUBERT. No, I pointed this out in 1957. At that time it was made up of 38 members. It is now made up of 41 members. At that time half the membership of the 38 roughly were in the medical field, mainly radiologists, and the other half physicists. Despite the unqualified competency of every individual on this committee, you simply cannot have a committee which represents only a few fields and not the wide variety of fields.

Representative HOLIFIELD. What fields would you advise should be added?

Dr. SCHUBERT. One of the most important fields is statistics. An appreciation of statistics is what we are dealing with in the problem of evaluating radiation of large populations. We need people who are specialists on the metabolism of radioactive elements. We need health physicists. We need geneticists. We need people who are expert in physiology, particularly as pertaining to excretion of radioactive elements. We need radiologists. In other words, a wide variety of disciplines is the point I want to make there.

Representative HOLIFIELD. As I am not aware of the spread of professions in the committee, I will ask Mr. Taylor what is the spread.

Mr. TAYLOR. The spread includes all of these people with the possible exception of a statistician. I am not sure that there is anybody who is a direct statistician.

Dr. MORGAN. Snyder.

Mr. TAYLOR. I am sorry. Dr. Snyder would certainly be regarded as an extremely competent statistician.

Representative HOLIFELD. I hope your organization will take these suggestions in mind, and if you feel you need a satistician that you don't have, obtain one, because we want a widespread representation.

Mr. TAYLOR. I forgot Dr. Snyder. I don't think we could get anybody that is any better.

Representative HOLIFIELD. Is he a statistician?

Mr. TAYLOR. Yes.

Representative HOLIFIELD. Is Dr. Morgan a health physicist?

Mr. TAYLOR. He certainly is. He was the first president of the Health Physics Society. In this committee, sir, we have the main committee of 41 people. These serve a double purpose. One, they tie to particular organizations having competence and interest in the field. The members are approved by the already existing members in order to meet needed qualifications on that committee. The executive committee of 10 people is chosen to represent what we consider the 10 most important disciplines. The subcommittees, numbering a total of something over a hundred people, are chosen as specialists in whatever particular area of work the subcommittee deals with. Any study or report gets reviewed all the way through those three channels before being released.

Dr. SCHUBERT. Mr. Chairman, I did not mean to imply that the NCRP has no representation from other disciplines. What I am saying is that they are overloaded with a couple of fields and have scattered representation from others. In the aggregate you might say they represent five or six disciplines. Mr. Taylor mentioned organizations represented. For example, there are I believe four radiologic organizations, each one having two members. I don't know how many genetic organizations there are in this country but I doubt if each one has a representative on the NCRP. So my point is simply that the NCRP should have wide representation with only a few members from each specialty, rather than 30 from one or two specialties, and eight scattered from others.

Representative HOLIFIELD. I think your position has been well presented for the record, Doctor. We will let it stand.

We are just about to adjourn, but I must ask one question. Is there a U.S. program to verify the results of the British study on prenatal X-rays in childhood malignancy such as Dr. Stewart is doing in England? Is there any such U.S. study going on?

Dr. POWELL. As far as I know, there is no single comprehensive study on the scale of Dr. Stewart's being done in this country today. I might say that part of this comes about because of the organization of the practice of medicine in this country, as distinguished from Great Britain. I know of at least three or four independent studies on a smaller scale. The total accumulation of data may well approximate that of Dr. Stewart.

Dr. SCHUBERT. There are two studies which are beginning to approach the scale of Dr. Stewart in this country. One is a study in Louisiana in connection with Tulane University on the same type of study Dr. Stewart made, except that in this case the questioning is done by professional sources so there is no chance of bias.

Then another study partially published by Miriam Manning of the Children's Cancer Research Association, and I will give the references for the record. This study, which is still continuing, is also on the same lines as the Stewart data. Both of these studies thus far have confirmed Stewart's findings. These are the only two studies that I know of of a scale which approach Stewart. Insofar as they have gone, they have confirmed what Stewart found.

Dr. POWELL. I know of at least one other which is on a rather large scale, and that is Dr. Henry Kaplan's. I have reason to believe there may also be two or three others.

Dr. SCHUBERT. Yes. There are several that are going on. There are a few mentioned in the statements today.

Representative HOLIFIELD. I wish we had more time, but we have to give up the room. We will call a halt to this roundtable discussion and the other one that we had planned for this afternoon we will have to take up in the morning. Dr. Neuman will be the first witness and then we will have a roundtable after that. Thank you very much, gentlemen, for your presentations on the panel.

I have several interesting papers and statements submitted by members of our panel, and will place them in the record at this point.

(The material referred to follows:)

MAXIMUM PERMISSIBLE EXPOSURE OF THE POPULATION-AT-LARGE TO SOURCES OF IONIZING RADIATION*

By

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It is not possible at the present time to state with absolute certainty that human exposure to ionizing radiation is harmful at very low dose rates that are of the order of natural background. There is always a probable error associated with a biological or physical measurement and at very low dose rates, i.e., background levels, it requires observations on an extremely large number of animals (or men) in order that the probable errors become less than the magnitude of the effects under observation or in this case to prove that there is any danger or in fact any detectable effect of exposure at the background level. Man has been subjected to this low exposure since the beginning of his existence and although background radiation probably has not been beneficial, certainly man has managed to evolve to his present state of development in spite of it. In order to determine with certainty the influence on man of exposures at the level of natural background radiation, observations will have to be made on many men that have been exposed at two or more levels, e.g., background and twice background, over many generations. Contrary to suggestions of a number of scientists and others the required information cannot be obtained satisfactorily by observing the biological effects of cosmic ray exposures at two or more elevations, e.g., Washington, D. C. and Denver, Colorado, because in this case there would be far too many

*Operated by Union Carbide Corporation for the U. S. Atomic Energy Commission.

uncontrolled variables; some of which would effect the well-being of man much more than his exposure to background radiation. In the first place, cosmic ray dose (about 0.07 mrad per day) comprises only about 20% of natural background exposure (0.36 mrad per day) to the average man and the total background dose would increase only a little more than 10% in going from sea level to 5,000 feet. In the second place, in going from one elevation to another there are many human and physical factors that cannot be taken into account adequately, e.g., change in effective lung volume, increase in solar intensity, changes in cosmic ray spectra, etc. In the third place about 1/3 of the total average background dose to man is the result of radiation (0.13 mrad per day) from the uranium, thorium, and actinium series of elements normally found in man's environment and many studies[1] have indicated that this component of the dose commonly may vary by a factor of 5 or more due to the building materials, e.g., granite, shale and concrete blocks, etc., of which our homes are constructed. In any case promising and worthwhile population studies that may be conducted for the purpose of determining the effect on man of doses of

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   Solon, L. R., W.M.Lowder, A.V.Zila, H. D. LeVine, H. Blatz, and M. Eisenbud, "External Environmental Radiation Measurements in the United States," HASL-25 (March 11, 1958).

   Effect of Radiation on Human Heredity, World Health Organization, Geneva, Switzerland (1957).

ionizing radiation of the order of natural background will require careful investigations of several large and comparable populations that have remained relatively fixed in single localities for generations. Although the prospects of obtaining the desired information by studying the effect of cosmic radiation on man seems difficult, if not impossible, there is fortunately some hope of obtaining useful information from a study of relatively closed populations such as those in the Travancore, India region, where in monazite sand areas, the background radiation may be as much as ten times that in other very similar areas that could serve as controls. We should like to encourage and help with such studies in every way possible because they seem to offer considerable promise of furnishing a firmer basis than is now available for establishing reliable maximum permissible exposure levels for the population-at-large.

Studies on animals and observations on man have indicated that there are two types of radiation damage -- one requiring threshold or minimum dose before radiation damage results, and the other increasing linearly with the dose[2] (Figure 1). Typical of the first type of damage is radiation sickness which probably does not occur or is not perceptible at doses less than 20 roentgens and radiation fatalities which are extremely unlikely at doses less than 200 roentgens. Until recently genetic damage has been considered to vary linearly with dose and likewise many observations at high dose rates have suggested that leukemic incidence may increase in direct proportion to the accumulated dose. Recent experiments of W. L. Russell et al[3] have indicated that the number of genetic mutations in mice is not independent of the dose rate.

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2. Morgan, K. Z.: "Health Physics", Chapter 7-2, pages 7-22 to 7-59, Nuclear Engineering Handbook, edited by Harold Etherington. Published by McGraw-Hill Book Company (1958).

3. Russell, W. L., Liane Brauch Russell and Elizabeth M. Kelly: "Radiation Dose Rate and Mutation Frequency", Science, Vol. 128, No. 3338, pages 1546-1550, December 19, 1958.

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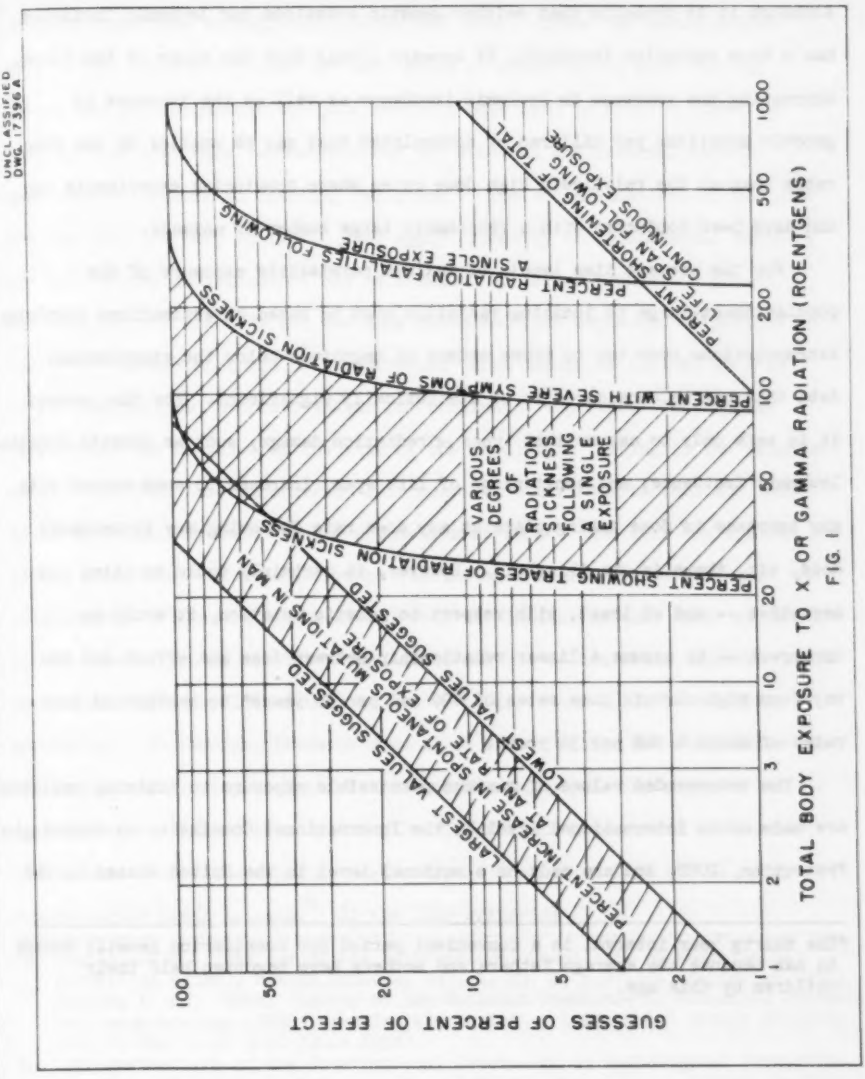
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PERCENT SHORTENING OF TOTAL LIFE SPAN FOLLOWING CONTINUOUS EXPOSURE
PERCENT RADIATION FATALITIES FOLLOWING A SINGLE EXPOSURE
PERCENT WITH SEVERE SYMPTOMS OF RADIATION SICKNESS
VARIOUS DEGREES OF RADIATION SICKNESS FOLLOWING A SINGLE EXPOSURE
PERCENT SHOWING TRACES OF RADIATION SICKNESS
LOWEST VALUES SUGGESTED
PERCENT INCREASE IN SPONTANEOUS MUTATIONS IN MAN AT ANY RATE OF EXPOSURE
LARGEST VALUES SUGGESTED
1000
500
200
100
50
20
10
3
2
1
100
50
20
10
5
2
1
TOTAL BODY EXPOSURE TO X OR GAMMA RADIATION (ROENTGENS)
GUESSES OF PERCENT OF EFFECT

FIG. 1

Although it is probable that neither genetic mutations nor leukemic incidence has a true radiation threshold, it appears likely that the slope of the curves expressing the increase in leukemic incidence as well as the increase in genetic mutations per millirad of accumulated dose may be smaller at low dose rates than at the relatively high dose rates where convincing experiments can and have been conducted with a reasonably large number of animals.

For the present time levels of maximum permissible exposure of the population-at-large to ionizing radiation must be based on estimations involving extrapolations over two to three orders of magnitude below the experimental data that can be considered to be statistically significant. For the present it is safe only to assume that types of radiation damage, such as genetic mutation leukemic incidence, and shortenings of life span, increase to some extent with any increase in dose taking place at any dose rate following any accumulated dose, viz. there is no threshold. However, it certainly would be ultra conservative -- and at least, with respect to genetic mutation, it would be incorrect -- to assume a linear relationship between dose and effect all the way from high chronic dose rates of 400 rad per 30 years* to background dose rates of about 4 rad per 30 years.

The recommended values of maximum permissible exposure to ionizing radiation are made on an international level by the International Commission on Radiological Protection, ICRP, and are made on a national level in the United States by the

*The thirty year interval is a convenient period for considering genetic damage to man because the average fathers and mothers have begotten half their children by this age.

National Committee on Radiation Protection, NCRP.[4] These two organizations have worked closely together for many years in establishing safe working levels and both organizations just recently have sent to press a number of publications that make recommendations for external and internal maximum permissible exposure to ionizing radiation.[5] The recommendations of the ICRP and the NCRP are very similar. They apply primarily to occupational exposure but with proper modifications may be extended to larger population groups. For example both the ICRP and the NCRP recommend that exposures to persons living in the neighborhood of a controlled* area should be limited to 1/10 of the occupational exposure values for continuous exposure. This raises immediately the question of the size of the population "living in the neighborhood". The NCRP has not answered this question other than to indicate that it is to be a small fraction of the population-at-large. The ICRP has applied the factor of 1/10 to the special groups (including primarily those living in the neighborhood of the controlled areas) with the limitation that these groups are not to exceed 1/30 of the total population and the occupational exposure levels are limited to a population that is not to be more than 1/150 of the total population. The Medical Research Council of Great Britain has recommended that the factor of 1/10 may be applied to a neighboring population provided it does not exceed 1/5 of the total population of Great Britain and the maximum permissible occupation exposure levels may be applied to 1/50 of the total population of Great Britain.[6] If the ICRP criterion

4. Taylor, L. S., "History of the International Commission on Radiological Protection (ICRP)," HEALTH PHYSICS, Vol. 1, No. 2, p.97-104 (September 1958). Taylor, L. S., "Brief History of the National Committee on Radiation Protection and Measurements, (NCRP) covering the period of 1929-1946," HEALTH PHYSICS, Vol. 1, No. 1, p. 3-10 (June 1958).
5. "Recommendations of the International Commission on Radiological Protection," Committee II Report on Internal Dose, Pergamon Press, Inc., London, England (1959). Report of Committee II of the National Committee on Radiation Protection, "Maximum Permissible Amounts of Radioisotopes in the Human Body and Maximum Permissible Concentrations in Air and Water," National Bureau of Standards Handbook 69. Available from the Superintendent of Documents, Washington 25, D. C. (1959).
6. "The Hazards to Man of Nuclear and Allied Radiation," Report of Medical Research Council. Published by Her Majesty's Stationery Office, London (1956).

* A "controlled area" is defined as an area in which radiation exposure is under the supervision and guidance of a health physicist.

were applied in the U. S., the number of radiation workers who would be permitted to receive the full maximum permissible occupational dose would be limited to about 1,200,000 people while those "in the neighborhood" of these operations who were permitted to receive 10% of the maximum permissible occupational dose would be about 6,000,000 people. At the present rate of expansion of the nuclear energy industry it will be many years before either of these conditions could impose serious boundary limitations. It is evident that nuclear energy plants must not be operated near our large cities, e.g., New York, Chicago, and Los Angeles, in such a way that these large populations would be considered to be "living in the neighborhood" of a controlled area and allowed to receive 1/10 of the maximum permissible occupational exposure values. However, it may be permissible to operate nuclear energy plants in or near such large cities provided these populations are limited to exposures specified for the population-at-large.

The ICRP as well as the National Academy of Science (NAS)[7] have recommended that the maximum genetic exposures to the population-at-large from man-made sources of ionizing radiation should be of the order of natural background. The NAS specified that the average exposure of the population's reproductive cells from radiation above natural background should be limited to 10 roentgens (or rem) from conception to age 30. The NCRP has not as yet set a specific figure for the exposure of the population-at-large, but has a special committee working on this problem and in the near future it probably will recommend a value that is not far different from "the order of natural background." The ICRP has suggested that for planning purposes the genetic dose to the population-at-large from all man-made sources of ionizing radiation other than medical

7. "The Biological Effects of Atomic Radiation", National Academy of Sciences, National Research Council, Washington, D. C. (1956).

sources should be set at 5 rem to the age of 30. This is equivalent to the 10 rem specified by the NAS if we assume the average medical exposure to age 30 to be 4.5 rem. To serve further as a guide the ICRP has apportioned this 5 rem as indicated in Table I.

The question is often asked, "Why do the ICRP and the NCRP permit much larger exposures to the occupational workers than to persons living in the neighborhood of the controlled areas or to the population-at-large?" There are many reasons for this. First of all the occupational workers in the nuclear energy industry are a select group of adults who receive routine medical examinations and are under the constant surveillance of health physicists. In spite of the fact that the nuclear energy industry is one of the safest of American industries, as indicated by Table II, the occupational workers recognize that there is some radiation exposure and they accept it as a part of the risk of employment just the same as is done with other hazards by the workers employed in all other industries. Persons living outside the controlled area, on the other hand, include children and pregnant women who probably have no knowledge of the radiation risks and may have no choice other than to live in their homes that are in the neighborhood of the controlled area. Unlike the occupational workers who work 40 hours per week and may move on to other types of work after a few years, some of the neighboring population may be exposed 168 hours a week and live close by for many years. Perhaps the principal reason for reducing the maximum permissible dose to persons living outside the controlled area is that a person engaged in any occupation must insofar as possible confine the hazards of his occupation to his own working group. Each industry has its own unique types of hazards. The construction worker, the lumberman, the miner or even the housewife has a particular brand of occupational hazard. As shown[8] by

8. Morgan, K. Z. "Current Status of the Internal Dose Problem", Health Physics, Vol. 1, No. 2, p. 125-134, September 1958.

Table I

MAXIMUM PERMISSIBLE GENETIC DOSE TO THE POPULATION-AT-LARGE SUGGESTED BY THE ICRP TO SERVE AS A GUIDE

(RHE Dose in Rem to Age 30)

(4.5 Medical)*	2.0	1.5 Internal 0.5 External 2.0 General to population-at-large
5.0 Other		
(4.5 Background)*	3.0	1.0 Occupational 0.5 Special groups 1.5 Reserve (TV, watches, etc.)
14 Total	5.0 Total	3.0

*The medical and background values are probable average values in the U. S. and are not values specifically recommended by the ICRP.

Table II

Year	Hanford*		Oak Ridge National Lab.		Chemical Industry Accident Frequency Rate
	Av. Annual Exposure (r)	Accident Frequency Rate	Av. Annual Exposure (r)	Accident Frequency Rate	
1944		2.71			
1945	0.9	0.90		1.86	10.08
1946	0.6	1.09		1.49	10.09
1947	0.4	----	0.1	1.74	8.86
1948	0.3	----	0.2	2.42	7.51
1949	0.2	----	0.2	1.54	5.72
1950	0.1	2.90	0.2	1.56	5.82
1951	0.1	2.76	0.3	2.09	5.48
1952	0.2	2.97	0.4	1.39	5.10
1953	0.2	3.15	0.3	1.20	4.53
1954	0.2	2.12	0.3	0.79	4.12
1955	0.2	0.79	0.3	0.59	3.77
1956	0.2	0.76	0.3	0.55	3.98
1957	0.2	0.45	0.4	0.70	3.55

Frequency is the number of injuries per million hours worked.

*Parker, H. M., "Radiation Exposure Experience in a Major Atomic Energy Facility", Peaceful Uses of Atomic Energy, Vol. 13, page 266, United Nations, N.Y., 1956.

Figure 2, our chances of survival as members of the population-at-large would be extremely poor if each industry were permitted to impose a full measure of its hazards on the neighboring population.

Close inspection of Table I indicates that the 5 rem allocated by the ICRP to man-made sources of ionizing radiation other than medical exposure is broken down into a number of components. Occupational workers (who of course are members of the population-at-large) are assigned 1.0 rem; special groups (i.e., persons living in the neighborhood of the controlled area, persons who visit inside the controlled area occasionally and persons that work in the vicinity of the controlled area) are assigned 0.5 rem and 1.5 rem is set aside as a reserve. The 2.0 rem general exposure to the population-at-large is the dose of particular interest for this discussion. Of this 2.0 rem, internal dose is assigned 1.5 rem and external dose is assigned 0.5 rem. A very small fraction of this 0.5 rem is used up already by exposures to shoe fitting machines, TV sets, radium dial watches, etc. This 2.0 rem comprises the dose received by the population-at-large over a 30 year period. It includes the dose received from radioactive waste discharged from nuclear energy plants[9] such as those at Oak Ridge and Hanford and radioactive waste from hospitals and research laboratories. No allowance was made specifically for fallout from the tests of nuclear weapons, although this might logically be included as part of the 2.0 rem. These radioactive materials are released originally in the air and in the water and some may become widely distributed in the air, soil, rivers, lakes, and oceans. Eventually some of the radionuclides become deposited in all the various forms of life, including man. Thus man is irradiated from without by man-made radioactive material in his environment and from within by man-made radioisotopes

9. Morgan, K. Z., "Summary and Evaluation of Environmental Factors that Must Be Considered in the Disposal of Radioactive Waste." Paper presented to the Joint Committee on Atomic Energy, Special Subcommittee on Radiation, Waste Disposal Hearings, Jan. 29-Feb. 4, 1959, Washington, D. C.

Morgan, K. Z., "Waste Management Program at Oak Ridge National Laboratory". Paper presented to the Joint Committee on Atomic Energy, Special Subcommittee on Radiation, Waste Disposal Hearings, Jan. 29-Feb. 4, 1959, Washington, D. C.

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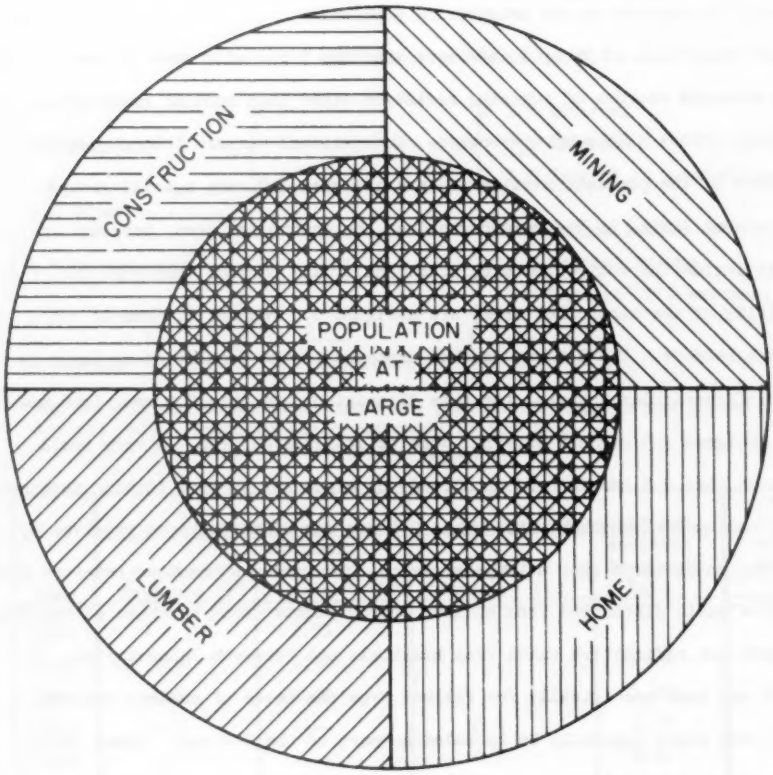

THE AMOUNT OF SHADING IS INTENDED TO SYMBOLIZE THE HAZARDS.

THE CENTER SECTION SHOWS THE DOSE TO THE POPULATION AT LARGE IF EACH OF THE FOUR PROFESSIONS WERE PERMITTED TO CONTRIBUTE ITS UNIQUE HAZARDS TO THE ENVIRONMENT.

Figure 2

as they move through the passageways of the body (i.e., into the respiratory and gastrointestinal tracts) from which certain fractions are deposited and incorporated into the various organs of his body. It should be emphasized that the values suggested by the ICRP in Table I apply only to the average man and in no case are they to be interpreted as limiting the exposure of an individual or even a group of individuals in the population. Likewise, it is permissible that there should be a considerable variation in the rate at which this dose is accumulated by the gonads of members of the population. Perhaps, less than 5% of this suggested 5 rem is being used by the average man of the population in 30 years at the present rate of exposure, but as the nuclear energy industry expands and if nuclear weapons tests are resumed we can expect a considerable increase in man's exposure. The detailed apportionment by the ICRP of this suggested 5 rem is of no significance, and in fact if one component can be reduced, others may be increased correspondingly. For example, during some periods and in certain areas of the earth the external environmental exposure may exceed the internal exposure or the average occupational exposure to the population-at-large may exceed 1 rem. This is quite acceptable provided the total average 30 year gonad dose from all man-made sources (including medical exposure) does not exceed 9.5 to 10 rem. Thus, if the average medical exposures are reduced presumably the other man-made exposures can be increased.

The values of 4.5 rem to age 30 for the average medical exposure is larger in some countries and especially in the large cities but is much smaller in countries that are less advanced economically. There is no question but that many benefits are derived from the medical use of X and γ radiation; on the other hand, there is no doubt that many medical exposures can be reduced with equally good or better medical results. Table III indicates average values of some of the common medical exposures many of which appear to be unnecessarily

TABLE III

Common X-Ray Exposures**

Chest X-Rays at Oak Ridge National Laboratory using full size X-ray film in direct contact with intensifying screen	20 mr(c)
Average chest X-ray exposure found from a survey in England	160 mr(d)
Average chest X-ray of majority in United States	200 mr(a)
Chest X-ray taking photograph on fluorescent screen	100 to 1,000 mr(d,e)
Dental X-rays	1.5 to 300 r(a,b,e)
Lower GI examination	27 r(a)
Upper GI examination	35 r(a)
Fluoroscopic examinations	5 to 270 r(a,e,f)
Average routine X-ray examination during 1st half of pregnancy*	46 r (a)
Average routine X-ray examinations during 2nd half of pregnancy*	56 r(a)
Treatment of malignant tumors	3,000 to 7,000 r(e)
Fluoroscopy in shoe-fitting (per 20 sec exposure)	7 to 14 r(e)

(a) Laughlin, J. S. and I. Pullman, "Gonadal Dose Produced By the Medical Use of X-Rays," Section III of Report, The Genetically Significant Radiation Dose Received by the Population of the United States. Preliminary Edition, published by National Academy of Sciences - National Research Council, Washington, D. C. (1957).

(b) Nolan, W. E. and H. W. Patterson, "Radiation Hazards from the Use of Dental X-ray Units." UCRL-1882, November 26, 1952; or Radiology;61, p.625-629, October, 1953.

(c) Memo to J.C. Hart from D. M. Davis and E. D. Gupton, "Radiation Dose in Diagnostic X-ray Procedures," March 26, 1957.

(d) Stanford, R.W. and J. Vance, "The Quantity of Radiation Received by the Reproductive Organs of Patients During Routine Diagnostic X-ray Examinations," British Journal of Radiology, Vol. 28, No. 329, p.266-273, May 1955.

(e) Moeller, Dade W., James G. Terrill and Samuel C. Ingraham, "Radiation Exposure in the United States," Public Health Reports, Vol. 68, No. 1, p.57-59, January 1953.

(f) Bell, A. L., "X-ray Therapy in Fluoroscopy," Radiology, Vol. 40, p.139-144, February 1943.

(*) Precautions should be taken to reduce these exposures as they are sometimes excessive. See: Russell, Liane B., "The Effects of Radiation on Mammalian Prenatal Development," Chapter 13, pp.861-918, Radiation Biology, Vol. 1, part II. A. Hollaender, editor, Published by McGraw-Hill Book Company, (1954).

(**) These doses are measured on the body surface of the incidence side.

high. When it is observed that the foetus (man in his most radiosensitive period of life) receives a very large exposure from the average routine X-ray examinations,* we can't help but wonder why more effort is not made to improve the equipment and techniques in order to materially reduce this exposure. Perhaps we should ask the question of why there is so much concern about the few thousandths of a rem received each year from fallout material and so little effort is made to reduce this medical exposure to our unborn children? It is to the credit of many of the leading radiologists in the U. S. and to their organizations that such an effective effort has been applied to this problem during the past two to three years that very recent preliminary surveys indicate a marked reduction in some of the exposures listed in Table III. It is only to be hoped that all users of sources of ionizing radiation, e.g., dentists, general practitioners, chiropractors, etc., will reduce medical exposures -- the principal source of human exposure to ionizing radiation -- to the lowest possible values consistent with good medical practice and for the accrual of the most overall good to the patients and their children. Why is it that many chest X-ray machines deliver 1000 mr per chest X-ray when just as good and even better X-ray pictures can be obtained from exposures of 20 mr using the best equipment and improved techniques? Why is there no concern that several surveys in some of the cities of the U. S. have revealed that many dentists deliver up to 300 roentgens to the jaw in a single series of dental X-rays and spray rather large doses to other parts of the body (including the eyes and gonads) when better X-ray pictures can be obtained with an exposure of 1.5 rem using properly adjusted equipment? As health physicists, I and my colleagues would like to keep all radiation exposures -- including those from fallout and radioactive

*A private communication from G. Failla, Dept. of Radiology, Columbia University, New York, N. Y. May 12, 1959, states, "In the first trimester of pregnancy perhaps one case in 500 would have an x-ray examination for possible genito-urinary disease. In the second trimester perhaps one in 200 would have an x-ray examination for genito-urinary to gastro-intestinal diagnosis. In the third trimester about 12% would have x-ray examinations for pelvimetry or complications, and these examinations would be made during the last few days of pregnancy."

waste disposal -- to an absolute minimum, consistent with the overall good of man but it is difficult sometimes for some of us to focus our attention completely on less than one percent of the radiation exposure problem while completely ignoring 99 percent of it.

In order to apply the values given in Table I and specifically the value 1.5 rem per 30 years of internal exposure to the internal dose problem resulting from fallout, waste disposal, etc., the ICRP has suggested that the Maximum Permissible Concentration, MPC, values for continuous occupational exposure that are given in the ICRP and NCRP Handbooks on Internal Dose[5] should be multiplied by 1/100 when the radionuclides deliver most of their dose to the gonads or localize in them, i.e., when the gonads are the critical organs. Since the MPC values for occupational exposure to the gonads are calculated on the basis of an average annual dose of 5 rem, it follows that 1/100 x 5 rem/yr x 30 yr = 1.5 rem, as was given in Table I.

Many radionuclides, such as Sr^{90}, do not deliver an appreciable portion of their dose to the gonads so they must be limited by other considerations. One of the most important radionuclides in nuclear weapons fallout material from the standpoint of gonad exposure is Cs^{137} which concentrates in the soft tissue (including the gonads) of the body. Fortunately the biological half-life* of Cs^{137} is only 70 days compared to the biological half-life of 50 years for Sr^{90}; otherwise, fallout would present a much more serious genetic problem. The limiting values given in the new ICRP and NCRP Handbooks on Internal Dose[5] for Cs^{137} and Sr^{90} and that are applicable to occupational exposure

*The biological half-life is the time required for half of the material to be eliminated from the critical body organ by normal biological processes.

are given in Table IV. From the above discussion it can be seen that if Cs^{137} were the only man-made radioactive material in the human environment that gives an appreciable dose to the gonads, the MPC for the average exposure of the population-at-large to Cs^{137} would be 2×10^{-6} μc/cc (2 ÷ one thousand billion curies per cc or 0.00000000000002 grams per cc) of water or 2×10^{-10} μc/cc (2 ÷ ten million billion curies per cc or 0.00000000000000002 grams per cc) of air. Of course there are some other radionuclides besides Cs^{137} that are present in the fallout material that deliver dose to the gonads so the maximum permissible concentration of Cs^{137} suggested by the ICRP as a guide for exposure to the population-at-large is somewhat smaller (perhaps 50 percent smaller for the average age of the fallout material) than the above values. Also the values for MPC in water and MPC in air would be reduced by a factor of two if the gonad dose from inhaled Cs^{137} were the same as from ingested Cs^{137} -- actually the inhalation dose from Cs^{137} in fallout is probably less than 10 percent of that from ingestion of Cs^{137}. The values for water can be applied to food as well as water since a large fraction of food is water and the calculations were based on the total water consumption (food and drink) of man. In cases where organs other than the gonads receive the principal exposure, the ICRP has suggested that for planning purposes the MPC values for continuous occupational exposure should be multiplied by 1/30 to obtain suitable values for the population-at-large. Thus the MPC values for the average exposure of the population-at-large to Sr^{90} would be 3×10^{-8} μc/cc (0.0000000000000002 grams per cc) of water or 3×10^{-12} μc/cc (0.00000000000000000002 grams per cc) of air. Again, other bone-seeking radionuclides besides Sr^{90} are contained in the fallout

TABLE IV

VALUES FOR OCCUPATIONAL EXPOSURE
AS GIVEN IN THE ICRP AND NCRP HANDBOOKS ON INTERNAL DOSE

Radionuclide	Critical Organ	Maximum Permissible Amount in Total Body (μc)	Maximum Permissible Concentration (μc/cc for 168 hr/wk exposure)	
			In Water	In Air
Sr^{90} (soluble)	Bone	2	10^{-6}	10^{-10}
(insoluble)	Lung			2×10^{-9}
(insoluble)	GI Tract		4×10^{-4}	
Cs^{137} (soluble)	Total Body	30	2×10^{-4}	2×10^{-8}
(insoluble)	Lung			5×10^{-9}
(insoluble)	GI Tract		4×10^{-4}	

material (e.g., Sr^{89}, Ce^{144}, Zr^{95}, etc.) so the values given above for Sr^{90} should be reduced by a factor (we might take 50 percent as an average value) which depends upon many things such as the type weapon, yield, elevation of shot, time since detonation, distance from shot, etc.

In like manner the maximum permissible body burdens of Cs^{137} and Sr^{90} as suggested by the ICRP as an average for the population-at-large are 0.3 μc Cs^{137} (0.000000003 grams of Cs^{137} in body) and 0.07 μc Sr^{90} (0.0000000005 grams of Sr^{90} in body).

In conclusion it should be emphasized that the above levels suggested by the ICRP for application to the population-at-large are average values that do not apply in the individual case. Also they are not firm figures but guides for planning purposes. However, even though these values are very small they do represent an appreciable exposure as indicated by the Summary in Table V. At the present time none of these values are firm. Although the NCRP has not as yet suggested specific dose values for the population-at-large, it may be anticipated that in the near future it will recommend values to serve as guides; values that are not much (if any) different from those in Table V. All values for occupational exposure in the Internal Dose Handbooks as well as the above values serving as guides will be revised from time-to-time as more data becomes available. It is to be hoped that as more data are obtained in the future, specific values applicable to the population-at-large can be determined for each of the radionuclides. For example the Health Physics Division of the Oak Ridge National Laboratory has underway small programs for the study of the localization of the radionuclides in the gonads and in the other body organs as well as ecological studies to determine the indirect effects

of ionizing radiation on man and his environment. Ultimately we must establish more firm MPC values applicable to the population-at-large; values that are based on large numbers of experiments that are normalized to man whenever possible. In the long run these values must be set in terms of the overall good to mankind. They must not be set so low as to deprive man of the many peacetime benefits of nuclear energy and at the same time they must be low enough that the benefits will exceed the damage not only with reference to ourselves but with respect to our environment and to generations of men yet unborn.

TABLE V

VALUES OF ABSORBED DOSE* TO VARIOUS ORGANS OF THE BODY FROM INTERNAL EMITTERS

Organs Exposed	Absorbed Dose (average rem in 30 yrs)	Remarks
Gonads	1.5	
Bone	6	From radionuclides emitting α, β, or e radiations
Bone	30	For the case of radium isotopes
Bone Marrow	15	For radioisotopes emitting X or γ radiations
Thyroid and Skin	30	
Total Body	5	Excluding the gonads
Individual All Other Organs	15	

*These values correspond to the dose that would be received by members of the population-at-large in 30 years if the average man were exposed for 30 years at the MPC values suggested by the ICRP for planning purposes.

STATEMENT OF DR. JACK SCHUBERT, SENIOR CHEMIST,
DIVISION OF BIOLOGICAL AND MEDICAL RESEARCH,
ARGONNE NATIONAL LABORATORY[1]

My statement is based in part on the contents of two articles[2,3] which are submitted for the record.

The points I stress are listed below. Additional specific information and extension of the comments listed are found in the aforementioned articles.

1. A maximum permissible level (MPL) of radiation is equivalent to a probability of risk. It involves the estimation of numbers which are related to the probability that exposure of a population to the MPL will produce no intolerable incidence of injury or death. That part of the population which receives less radiation than the MPL will have a lower probability of sustaining injury while that part of the population which receives more radiation than the MPL will have a higher probability.

2. The setting of a MPL must be based on that appreciable part of the population which is most likely to suffer injury from a given MPL. Insofar as cancer produced by radiation is involved, the most susceptible segment of our population appears to be the unborn child.

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[1] Biography is attached to this statement.

[2] "Global Radiation Limits," by J. Schubert and R. E. Lapp, Bulletin of the Atomic Scientists, Volume 14, pages 23-26 (1958).

[3] "Fetal Irradiation in Relation to Cancer Deaths from Fallout and Natural Background Radioactivity," by J. Schubert, Bulletin of the Atomic Scientists, June, 1959.

3. The MPL cannot be based wholly on average values of radiation exposure of the entire population, but attention should be given to that fraction of the most sensitive population whose radiation exposure exceeds the average values by two, three, and more, as long as those who exceed the average constitute significantly large numbers.

4. The best possible present-day estimate of the cancer-producing effects of fallout is derived from studies of the carcinogenic effects of unusually small amounts of irradiation (about 2 r) to the human fetus. From these studies I have estimated that every 20 milliroentgens (mr) of fallout radiation received by a child before birth may produce a 1% increase in the numbers of children below the age of ten who die from cancer each year.

While the estimate given here is quantitatively subject to a wide uncertainty, the important point is that it now appears that radiation doses so low as to approach the radiation levels from fallout and natural background cause a significant increase in childhood cancer deaths.

As the amount of fallout increases it is prudent to assume that the increase in childhood cancer deaths produced will increase proportionately.

5. It is sometimes not appreciated that the permissible level cited for a given radioactive isotope assumes that no other radiation exposure occurs except that received from the specific isotope and the natural background. Thus, when the maximum permissible level of strontium-90 is stated to be 100 strontium units it must be kept in mind that it is impossible to receive radiation only from strontium-90 in the case of fallout.

Since the MPL's are not additive it is necessary to consider a "practical permissible limit" which includes an approximate evaluation of

the radiation received from all fallout sources. The practical radiation limit for strontium-90 should be set at 30 strontium units, half the present MPL, since any person who receives 30 strontium units actually receives radiation from other fallout sources equivalent to roughly an additional 30 strontium units.

6. It appears reasonable that the MPL for large populations be related to the natural background radioactivity. Once a value of the natural background radioactivity has been agreed upon - I have suggested 10 rem as the lifetime full-body dose - then the MPL can be set as a multiple of this "fixed natural background".

At present the MPL for large populations is pegged to the occupational permissible level which has the disadvantage that it varies from country to country, and within a given country it has been subjected to constant revision. By relating the MPL for large populations to a more logical baseline, namely the natural background, the MPL would, for all time and in all countries, have an identical and unvarying baseline.

7. In order to achieve the best possible estimate of the MPL a new committee of scientists representing all the pertinent fields of science such as genetics, biostatistics, physics, medical science, etc. should be created and the membership be chosen by some procedure recommended by an organization such as the National Academy of Sciences. It is important that such a committee which must now deal with the possible effects of very low levels of internal and external radiation not be dominated by a few fields of specialty.

The National Committee on Radiation Protection (NCRP), which for many years has been setting the MPL's, does not meet the scientific requirements stated above and its basis for selecting committee members is inappropriate.

8. Once a qualified scientific committee has set maximum permissible levels of radiation exposure for large populations it is desirable that another type of committee apply this MPL to specific situations and occupations such as in industry, laboratories, and naval vessels. Such a committee should have representatives not only from science and engineering but from labor, management, the military forces, and the public as well.

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ADDITIONS TO THE STATEMENT OF DR. JACK SCHUBERT, SENIOR CHEMIST, DIVISION OF BIOLOGICAL AND MEDICAL RESEARCH, ARGONNE NATIONAL LABORATORY

I wish to extend some of the comments made in my prepared statement which has already been given to the Congressional Committee. In addition I would like to insert some scientific papers which have an important bearing on the evaluation of potential hazards from radioactive fallout.

Point 1: I think it is necessary to extend my comments which take it as fact that the National Committee on Radiation Protection (NCRP) is unqualified as a group to provide the American public with a well-balanced and technically competent evaluation of maximum permissible limits of radiation. The NCRP was originally organized to deal with a limited problem, namely the protection of technicians working with radium and X-rays in hospitals and a few limited industries. Consequently the committee has consisted predominantly of physicians, usually radiologists, and radiological physicists. However, since 1954 we are faced with a new problem which had never been visualized by the old NCRP. This new problem--the world-wide dispersal of man-made radioactivity--involves the evaluation of the potential harmful effects of radioactive isotopes and low-level radiation to the world's population.

The new problem of evaluating the effects of low-level radiation is no longer a problem for which the medical profession and their associated physicists alone are capable of judging. Scientists from numerous scientific disciplines are necessary--geneticists, epidemiologists, meteorologists, biostatisticians, biochemists, geologists, physiologists--to name a few. Yet, even at this late date, the NCRP still remains nearly unchanged from its original setup. The NCRP, namely, the main or parent committee, consists of 41 members of whom at least 33 are physicians and physicists. In recent years

they have added some scientists from other disciplines. However, the representation is still predominantly from radiological organizations, the military, and industry.

Dr. Taylor in his original remark on my testimony stated that the NCRP consists of 150 members. He was of course referring to the membership of all the subcommittees as well. He agreed, however, that the main or parent committee, the NCRP does consist of only 41 members. He overlooked the difference between the main committee and the subcommittee later in his remarks when he asserted that the NCRP does have a statistician on its staff. Dr. W. S. Snyder, a competent statistician, was named as being a member of the NCRP. Dr. Snyder is <u>not</u> a member of the NCRP but a member of Subcommittee 2 on permissible internal dose, at least as of November 1, 1958.

In my prepared statement and in my testimony my criticism of the composition of the NCRP was not such as to imply that scientists from other disciplines were not represented but rather that the membership of the NCRP was topheavy with radiologists and physicists. Examination of the membership list of the NCRP--which I assume will be included in the record of these hearings--is sufficient to illustrate my point.

The deficiencies in the membership of the NCRP extend into the subcommittees which are chosen largely upon the recommendations of members of the NCRP. Two examples illustrate my thesis. One is that some individuals are members of three or more different subcommittees despite the fact that many other qualified scientists are available who do not belong to any subcommittee. Secondly, some of the subcommittees themselves reflect the inadequate scientific representation of the main committee. Thus subcommittee 2 on

permissible internal dose has ten members distributed as follows: six of the ten are physicists, two are physicians, one is a statistician, and one is a biochemist. As a minimum, one should expect such a committee to have fewer physicists and more individuals who are expert on such topics as calcium metabolism and the relationships between the chemical and physical state of radioisotopes and their metabolism.

Point 2: I feel that too many unwarranted conclusions have been drawn from the radium dial cases with respect to the carcinogenic action of radium. This is a very important point because the estimation of the permissible levels of strontium-90 have been tied to that of radium. Much has been made of the fact that no cases of bone cancer have been observed in those radium dial painters who have had about 0.4 microgram or less of radium in their bodies. This observation has been utilized in considerations which have led to the setting of 0.1 microgram of radium as the occupational permissible level. However, one should examine the incidence of bone cancer in the radium dial cases from a point of view analogous to that used by insurance companies. From the fact no bone cancers were found in exposed persons retaining body burdens of less than 0.4 microgram of radium does not prove in any manner that such low amounts may not cause an appreciable increase in bone tumor incidence as the following paragraph details.

Consider the number of bone tumors that would normally, that is, spontaneously, appear in a group of 100 girls beginning at age 15 and followed for 30 years. For this calculation I am inserting for the record of the hearings the vital statistics on bone tumor incidence published by Mr. Harry Auerbach of the Argonne National Laboratory.[1] From his tables it is found

[1]Harry Auerbach, "Bone Tumor Integrated Death Rates", Semiannual Report Number ANL-5841, July-December, 1957 from the Division of Biological and Medical Research, Argonne National Laboratory.

that normally in 100 females beginning at age 15 and followed for 30 years that the actual bone tumor incidence is 0.015 bone tumor during the entire period of observation. Now if some agent such as 0.1 microgram of radium caused even a tenfold increase in the bone tumor incidence it is still unlikely that even one tumor would or could be detected. Obviously the handful of radium dial painters having small amounts of radium in their bones are completely inadequate a sample on which to make a judgment as to the radiotoxicity of amounts of radium or strontium-90 in amounts corresponding to a 0.1 microgram of radium or less.

An analogous situation which has been utilized by some of the witnesses has involved the apparent fact that no bone cancers were observed in a group of radiologists. Consider a group of one thousand men beginning at age 25 and followed for 30 years. The number of spontaneous bone cancers which would occur in the thirty year period is 0.27. Consequently it would be highly uncertain that one tumor would be found even if the radiation exposure of a thousand radiologists caused a tripling of their normal bone cancer incidence.

Point 3: The distribution of strontium-90 from nuclear weapons tests is world-wide which means that one has to deal with a heterogeneous system operating in a heterogeneous population. I pointed out in my testimony that when dealing with a potentially harmful agent involving the world's population it can be misleading to use average values of Sr^{90} in the bones. It is important, especially in relation to the setting of permissible levels for a world population that we have some basis for estimating the degree to which appreciable fractions of the population accumulate two and more times the average amount.

Consider the histogram shown in Figure 1 which shows the experimental results giving the analyses of natural non-radioactive strontium in the bones relative to calcium. While it was stated that the data in Figure 1 approximate a normal distribution it is seen that the lognormal fits the data and is the proper one to use. Note that the use of the normal distribution arbitrarily disregards several percent of the population who exceed the average values by two or more. It might be added that as a purely technical point that the average values calculated on the basis of a non-applicable normal distribution are inappropriate. Prof. J. H. Gaddum has pointed out that if a distribution is not normal "it is unjustifiable even to assume that the arithmetic mean is the best estimate of a quantity that can be derived from a set of measurements of it", (Nature, 156, 463, 1945). The geometric mean is the suitable average value for the data illustrated in Figure 1.

It is possible to test whether a normal or lognormal distribution applies by plotting the experimental data on special probability paper. Depending on the type of distribution curve assumed, whether normal or lognormal, a reasonably good straight line is sufficient evidence to justify the use of a given distribution to describe the data.

A good example of the applicability of the lognormal to apply to strontium-90 content of human bones is taken from the recent paper by J. L. Kulp and associates (Science, 129, 1249, 1959). They analyzed the bones of 838 humans living in different parts of the world. They concluded that the distribution "is clearly not normal". When the data are plotted on probability paper assuming a normal distribution as shown in Figure 2 it is seen that no straight line is obtained. However, when a lognormal distribution is assumed one obtains an excellent straight-line fit of the data as shown in Figure 3. The

applicability of the lognormal to these necessarily heterogeneous world-wide data is very gratifying. In view of the results obtained shown in Figure 3 it is difficult to explain the statement of Kulp and associates that the distribution does not "correspond closely to a lognormal pattern".

Using the lognormal distribution it follows that 28% of the group represented in Figure 3 have three or more times the average (geometric mean) amount of strontium-90 in their bones. Over 4% will have seven or more times the average! These values are appreciably greater than those of Kulp's who used incorrect averages and an incorrect distribution curve and hence underestimated the fraction of the population which would exceed the average values. The assumption of a non-applicable normal distribution always gives values which are too low as can be seen from Figure 1.

I am preparing together with my colleague, Mr. Sylvanus Tyler of the Argonne National Laboratory, a detailed analysis of the applicability of the lognormal distribution to the levels of trace elements in the body. Two conclusions which we have arrived at are: 1) The type of data shown in Figure 3 will never become normal as some scientists have believed, and 2) Without exception, the distribution of trace elements such as radium, cesium, natural and radioactive strontium, etc., all are best described by a lognormal distribution. Only in those cases where too few samples are available or in some cases where the samples analyzed are restricted to a homogeneous group then either the lognormal or normal distributions appear to apply.

While it is of certain scientific interest to restrict analyses of human bones to individuals living in only one area and in a restricted age group, the world-wide distribution of strontium-90 is inevitably subject to wide

variations. The fact that the lognormal distribution applies even in these cases allows a realistic prediction of the deviations far from the mean and hence gives a better basis for setting permissible levels.

Point 4: For reasons given in detail in my technical paper on fetal irradiation and fallout, it is my opinion that the findings of Stewart, Webb, and Hewitt on the incidence of childhood cancer induced by small amounts of X-rays, is of the utmost significance insofar as estimating the possible effects of fallout and natural background radiation. I am submitting the Stewart paper for insertion into the record of these hearings and a copy is enclosed (A. Stewart, J. Webb, and D. Hewitt, "A Survey of Childhood Malignancies", British Medical Journal, June 28, 1958, pages 1495-1508).

One point from these studies that I would like to stress is its bearing on the threshold problem. They found that doses of the order of 2 roentgens produced a 100% increase in childhood cancer incidence including leukemia. This suggests, in a practical way, that the concept of threshold levels for carcinogenic effects, insofar as the human fetus is concerned, is not applicable. If a threshold does exist in these cases it lies too low to be included as a factor for evaluating the world-wide effects of radiation.

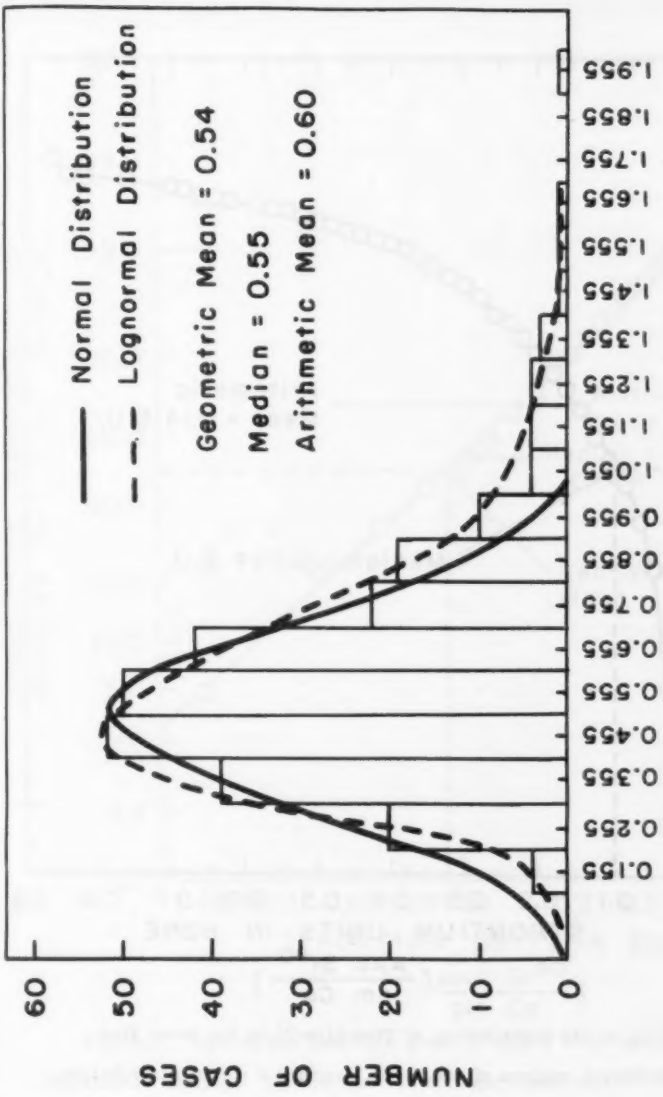

Figure 1. Histogram of $Sr/_{Ca}$ ratios for 277 human bone specimens. The histogram is presented in papers by K. K. Turekian and J. L. Kulp, Science, 124, 405 (1956); and W. F. Libby, Proc. Nat. Acad. Sciences, 43, 758 (1957).

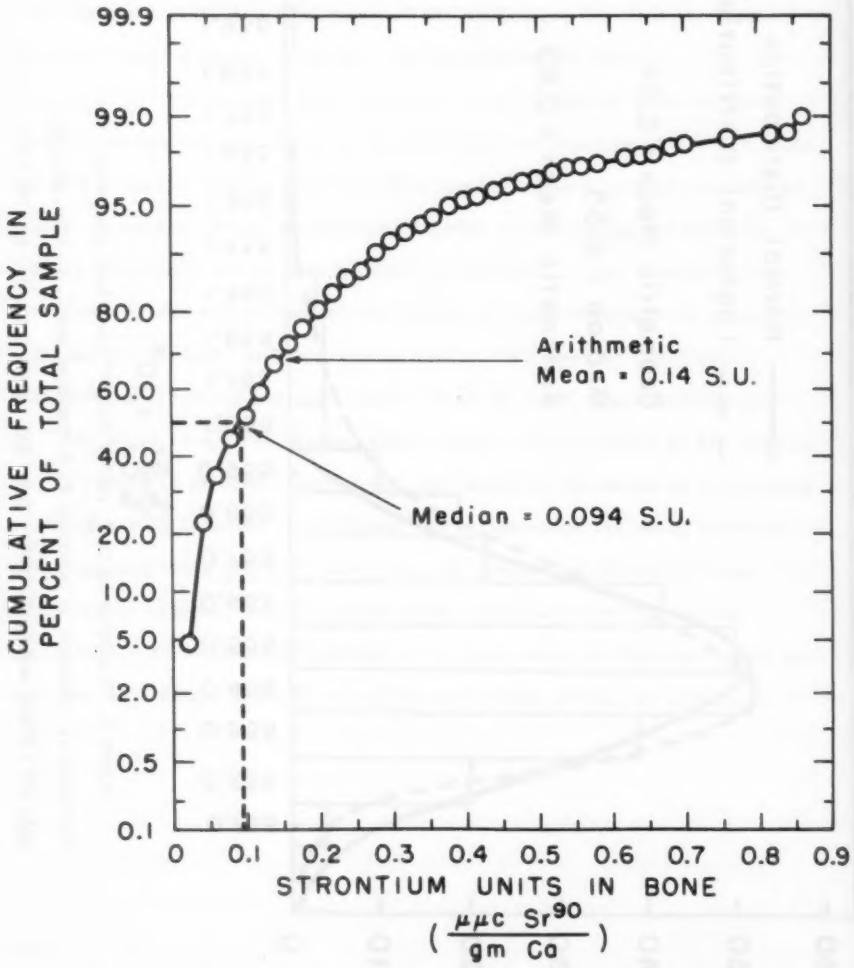

Figure 2. Plot of the distribution of Strontium-90 in the bones from 838 humans from different regions of the world, assuming a normal distribution. The data used are those of J. L. Kulp, A. R. Schulert, and E. J. Hodges, Science, 129, 1249 (1959). The pronounced curvature shows that the normal distribution is not applicable to the data.

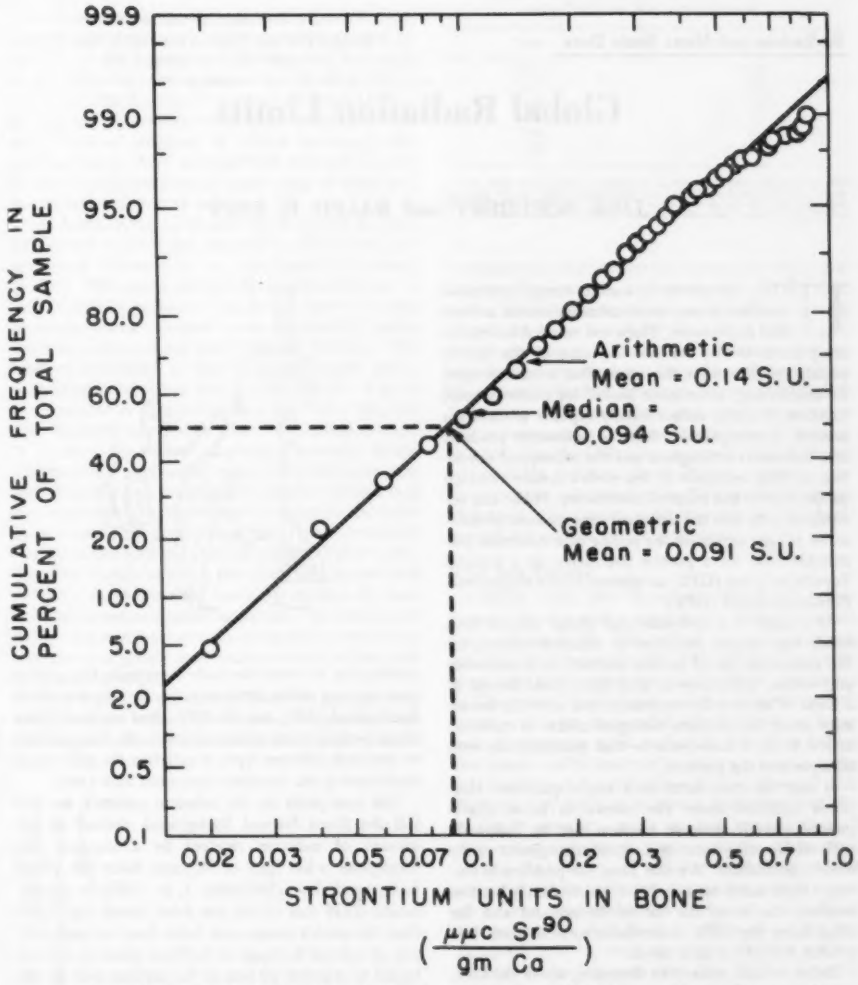

Figure 3. Plot of the same data given in Figure 2 but assuming a lognormal distribution applies. The abscissa is in logarithmic units.

Radiation and Man: Basic Data

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Global Radiation Limits

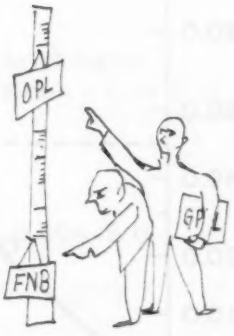
JACK SCHUBERT and RALPH E. LAPP

UNTIL the advent of nuclear energy one could consider all man-made radiation sources as localized in character. There was no need for considering limits to the radiation exposure of the world's population. It was for this reason that interest focused on establishing "permissible limits" for relatively small numbers of adults occupationally exposed to radiation hazards. The large-scale release of radioactive products into the earth's atmosphere and the subsequent deposition of these materials on the earth's surface attracted public interest and initiated controversy. Public and scientific anxiety over the biological consequences of radioactive fall-out argues for the setting of a maximum permissible level for a general population or a General Population Limit (GPL) as opposed to an Occupational Permissible Limit (OPL).

In a sense it is fortunate that global fall-out from bomb tests focuses attention on radiation hazards, for the expanding use of nuclear reactors, both stationary and mobile, will sooner or later force establishment of a GPL. What is to be regretted is that scientific knowledge about the ultimate biological effects of radiation at low levels is inadequate to deal quantitatively with all aspects of the problem.

It may be formulated as a single question: How much radiation above the natural radiation (background) which everyone receives may be "allowed" without incurring harm to a significant segment of the world's population? We may liken the problem to cutting a third notch on a yardstick which already has two notches, one being the natural background and the other being the OPL. Somewhere between these two notches the GPL is to be fixed.

Before we can proceed to discussing where the GPL



notch goes, we must obviously investigate the position and meaning of the levels corresponding to the natural background (NB) and the OPL. And we must define these levels in terms of appropriate units. Since one has to deal with different types of radiation the most meaningful unit is the *roentgen equivalent man* (rem).

The base point on the radiation yardstick we shall call the Fixed Natural Background, defined as that amount of radiation received by a standard man throughout a life span of 70 years. Since the annual background dose (Reference 1, p. 1240) is approximately 0.134 rem (0.030 rem from cosmic rays, 0.060 from the earth's surface and 0.044 from internal emitters, all referred to dosage of the bone marrow), it seems logical to stipulate 10 rem as the lifetime dose for the Fixed Natural Background (FNB). The authors propose that any General Population Limit for radiation be fixed as a multiple of the FNB rather than as a fraction of the OPL. We realize that the natural radiation background varies from region to region and for different heights above sea level, and so on. However, even though arbitrary, the fixing of an exact value for the NB provides many advantages as has been found in

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analogous situations as witness the "standard man" and the "curie."

Before proceeding further, we must examine the history and philosophy attending the relative position of the OPL (2, 3). Reports of radiation injury to dozens of individuals began to appear in the literature within a few years after Roentgen's discovery of X-rays (1895). As early as 1902 attempts were made to define a "tolerable" dose of radiation. In 1911 a monograph was published by O. Hesse who gathered statistical data on 94 cases of radiation-induced cancer some of which evidenced latent periods of a decade (4).

The International Committee on X-ray and Radium was formed in 1928 and renamed in 1937 as the International Commission on Radiological Protection (ICRP). This group defined Permissible Dose as "a dose of ionizing radiation that, in the light of present knowledge, is not expected to cause appreciable bodily injury to a person at any time during his lifetime." The "present knowledge" at that time was derived largely from human experience with a small number of gross overexposures to X-radiation; thus the word "appreciable" by-passed more subtle effects of radiation damage. In addition, the concept of damage occurring during a lifetime was admittedly weak since there were very little statistical data on long-term radiation effects. It is thus understandable that suggested values for the maximum permissible dose should have been constantly lowered throughout the past half century. Table 1 illustrates the manner in which the official and quasi-official estimates for permissible levels (occupational) have changed for external radiation sources. The most recent change apparently represents a strong recognition of the importance of genetic considerations and of others such as shortening of life.

With regard to permissible levels for internal emitters, principally radium, the record of the past half century begins with the curious notion—which persisted for many years—that radium had a beneficial effect upon humans. For example, as late as 1917, C. E. Field wrote: "Radium has absolutely no toxic effects, it being accepted as harmoniously by the human system as is sunlight by plants." (5). Figure 1 illustrates how the suggested permissible levels for radium in occupationally exposed individuals have been changed since the first limits were established. If we look upon radium retention in humans as a problem in toxicology, it is understandable that the OPL is not regarded as an absolutely safe limit but rather as a level which is subject to constant revision and re-evaluation. Because of the nature of the evaluation process, much depends upon the background and outlook of the evaluators.

TABLE 1

OCCUPATIONAL RADIATION LIMIT TO TOTAL BODY FROM EXTERNAL RADIATION SOURCES

Year	OPL (r/year)
1902	2500
1925	100
1931	50
1936	25
1948	15
1956	5*

* The dose rate is based actually on accumulated exposures received. The maximum absorbed dose to total body that could be accumulated from occupational exposure was set at 5 (A-18) in which A is the age in years. This limit on accumulated exposure amounts to an average dose rate of about 5 rem per year.

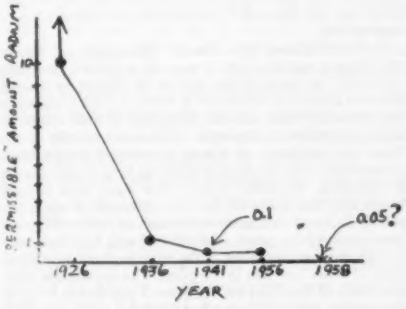

FIGURE 1

Who Sets Limits?

Initially, those who formulated the ICRP recommendations were either physicians or physicists associated with medicine. The outlook of most physicians was limited to short-term and visible effects of radiation; many even today regard the genetic effects of radiation as "theoretical." Radiologists accustomed to therapeutic radiation dosages measured in the hundreds or thousands of roentgens seem to find it difficult to believe that much smaller amounts of radiation may be harmful. Consider the membership of the influential U.S. National Committee on Radiation Protection (NCRP): of the 36 members, nearly one-half are physicians, 8 representing radiological groups, and the others representing the military and industry; while nearly all the rest are physicists, with a few representing manufacturers of radiation equipment. Essentially, no "pure" scientists representing such fields as cellular physiology, biochemistry, genetics, trace element metabolism, and biostatistics are on the committee. The NCRP, being the principal committee concerned with all aspects of radiation, should represent a wide spectrum of scientific fields—one made up of all geneticists, for example, is no more qualified than a committee consisting only of physicians, or statisticians. One of the underlying reasons why the recommendations of the report of the National Academy of Sciences (1956) clashed with

NCRP views is probably the different membership of the advisory committees involved (6).

A constant argument in radiation protection circles has been the question of how much radiation it takes to produce cancer in humans. Many radiologists and biologists subscribe to the contention given in many textbooks that a dose in excess of 1,000 r is necessary to induce cancer. Values of 1,000 to 3,000 r are stipulated as necessary for the induction of lung and bone cancer. Studies published in 1950, based upon late follow-up of children given thymic irradiation with X-rays, point to radiation dosages in the neighborhood of 200 r as inducing thyroid cancer. Subsequent publications have substantiated these initial results (7). British data, first published in September 1956, indicated that many types of cancer were induced in children exposed *in utero*, generally in the 8th or 9th month, as a result of pelvimetric X-rays (8). Intravaginal dose measurements yield a total average of 2.9 r for a series of 4 pelvimetric films (9). That such small amounts of radiation may induce cancer in human tissue has been questioned (10), but the completed study, not yet made public, bears out the preliminary reports. The data compiled by Dr. A. Stewart and her colleagues from a study of more than 1,500 children who died before the age of 10 from malignancy of all types shows that a history of irradiation *in utero* (i.e., a pre-natal abdominal X-ray) was obtained nearly twice as frequently from the mothers of children who had died from leukemia or cancer as from the mothers of live controls. If these observations are confirmed, the whole problem of the carcinogenic effects of radiation must be re-evaluated.

Factors To Be Considered

It may be hoped that future research will illuminate the induction of somatic effects by radiation in the range of a few roentgens. But the present data should make for extreme caution in the establishment of the GPL. The following considerations, however, also apply:

1. *The Nature of Relative Risk.*—In the past the numbers of people involved in occupational exposure to radiation have been relatively small, so that calculation of the risk to a total population raises new problems. A probability of one injury per 100,000 individuals exposed would be a small risk for a laboratory group but for the world's population it would involve harm to 28,000 individuals.

2. *Sensitive Groups.*—A large population includes a non-uniform sample in contrast to the selected sample in occupational exposures. The latter consists of healthy adults, educated in risks, voluntarily exposed to known hazards under administrative and medical supervision. Those establishing the GPL must recognize the sensitivity of the most critical age, health, or genetic group. The GPL must also reflect consideration of the individual *in utero* if this is the most sensitive phase of a person's exposure to radiation.

3. *Lifetime Radiation Effects.*—Delayed and subtle somatic effects of radiation become of great significance when one applies the concept of a radiation limit to a total population. The medical profession has only limited data on late effects of radiation, usually limited to follow-up studies of a few years or decades after irradiation. Very few data exist for half-century delayed effects and these involve individuals originally exposed as adults or in late adolescence. Information on delayed radiation effects on children is urgently needed in considering the GPL since many millions of persons are irradiated even before birth. For example, about 400,000 children are irradiated *in utero* in the United States each year as a result of X-ray pelvimetries—one woman in four has a routine pelvimetry before the birth of her first child (11).

4. *Genetic Effects.*—Obviously the genetic factors become of great importance when formulating a GPL. The NCRP suggested in January 1957 that: "The maximum permissible dose to the gonads for the population of the United States as a whole from all sources of radiation, including medical and other man-made sources, and background, shall not exceed 14 million rems per million of population over the period from conception up to age 30, and one-third that amount in each decade thereafter." Of the 14 million rems, 4 million rems are from the unavoidable natural background.

5. *Biospheric Variations.*—Deposition of air-borne radioactive particles shows irregular patterns depending upon the geographic source of original radiation, the meteorology, soil conditions, terrain, and the type of agriculture. These variations will be superimposed upon differences in diet and human factors which lead to retention of radioactive substances in humans.

6. *Ecological Factors.*—Upset to the ecological pattern of an area can result from changes in a single ecological link. Concentration of a radioelement by a species must be considered. The radioactivity of plankton average several thousand times higher than the surrounding sea water.

7. *Statistical Distributions.*—It is of importance to know what distribution pattern traces of radioactive substances exhibit on being retained in humans. One may fix the GPL on the basis of an average value, say for strontium-90 retained in bone, but the critical feature for a global population is the extent to which a segment of this population will retain twice, three, four or more times this average.

8. *Psychological Factors.*—Scientists may recommend certain values for a GPL, but acceptance of these recommendations by the general population is another thing. This has been demonstrated rather graphically on a number of occasions when the public was confronted with a radiation hazard. The Japanese response to contaminated tuna fish is a case in point (12). Radiation levels established by the American tuna industry clearly reflect the view that the consumer sets the standard for acceptability of radioactivity in canned tuna.

9. *Future Knowledge.*—Present knowledge of human physiology is not adequate to preclude scientists' "stubbing their toes" in extrapolating studies on chronic or delayed effects of radiotoxic substances in humans. This is especially true when one looks into the allied field of toxic chemicals such as beryllium compounds (13). Unexpectedly it was found that inhalation of minute amounts of certain forms of beryllium could produce a disabling lung disease known as berylliosis. The latter might follow many years of good health after the intake of the toxic chemical. It was a surprising discovery that many individuals in communities adjacent to beryllium plants were afflicted with berylliosis even though the concentrations of the toxic material were thousands of times less than in the plant. The permissible exposure levels of beryllium for the general population living in the vicinity of a beryllium plant has been set by the AEC as 200 times less than for occupational groups. The history

of toxicology is replete with many other examples of recognition of hazards and setting up of limits only *after* serious or lethal effects were observed.

10. *Adequacy of Biological Data.*—Extension of radiation levels from small populations to 2.8 billion people makes it mandatory that there be an adequate biological base for predicting the ultimate effect of radiation upon so many individuals. In the case of globally distributed strontium-90 the evaluation of the toxicity of this additive to the human internal environment is based upon some 80 published case histories of radium retention in humans. Only a fraction of these individuals were reported to have radium levels in the area of interest, i.e., chronic, low-level damage. Additional radium-burdened humans have been found in the past few years and the total may run to several hundred cases.

About 15 individuals in every million become afflicted with bone cancer each year. Now even if this rate were increased ten-fold by an amount of radium corresponding to the OPL this would still mean less than one case of bone cancer would be found in a thousand cases. It is obvious, therefore, that conclusions as to the toxicity or lack of toxicity of radium and hence of strontium-90 drawn from a handful of cases are statistically meaningless. While no bone cancer was found in patients having less than 0.4 microgram of radium, patients who had 0.5 to 10 micrograms of radium had a bone tumor incidence of 14 per cent. From these latter data it can be calculated that the probability of bone cancer from the retention of 0.01 microgram of radium (the GPL) is 1 in 2,000 cases in 30 years (assuming the worst—complete linearity between radium content and bone cancer induction). This would amount to nearly 100,000 additional cases of bone cancer in the U.S. population alone in a 30-year period.

It is apparent that the biological basis for evaluating the global strontium-90 hazard is by no means firm. Considering the shakiness of the base, it is difficult to understand the confidence with which some scientists, officials, and others have voiced optimistic predictions about the nature of this new hazard.

This tabulation does not necessarily end with the recital of these ten topics for there are other aspects of the global problem which could be added. When one comes to consider the over-all problem it is inescapable that the final decision on the GPL will consist of a value judgment and a calculated risk. A certain risk must be taken in order to obtain the great benefits which accompany the use of radiation. A humanist would argue strongly for allowing no radiation which could add to human misery; a geneticist would plead for no level which might add significantly to the genetic overload of future generations; and a somatologist, conscious of the limitations of present knowledge, might well argue for levels stipulated by the geneticist.

The present custom in stipulating a value for the GPL is to set it as some fraction, often one-tenth, of the OPL. An obvious disadvantage is that the OPL is arbitrary and has been subject to many downward revisions. Furthermore, values for OPLs vary from one country to another. Some extremists have suggested GPLs at such a fraction of the OPL that below-background values result. Setting the GPL as a multiple of the FNB (10 rem) would resolve these difficulties.

Within reason and practicality, the GPL should be based upon the harmful effects expected to result from the smallest dose of radiation to a significant segment of the world's population. If we have a choice between a lower and a higher GPL, within the bounds of present knowledge, we should choose the lower one, if it permits us to carry on our essential activities involving the use of radiation. Emphasis should be placed upon genetic factors, and on possible especially high sensitivity of the developing embryo, in stipulating the GPL. Of future importance is the point that the potential hazards from radiation must be discussed not only in terms of "average" dose but also in terms of the fraction of the population who will exceed the average by two, three, and more (14).

Summary

It is proposed that international agreement be reached on establishing radiation limits applicable to the world's total population. Such a General Population Limit (GPL) would be at some point intermediate between the Fixed Natural Background (FNB) to which everyone is exposed and the OPL (Occupational Permissible Limit) set for small numbers of individuals occupationally exposed to radiation. It is proposed that the GPL be taken as a multiple of the FNB which in turn is fixed at 10 rem. The factors which enter into consideration of the global radiation limits are enumerated and contrasted with those involved in setting the present occupational levels.

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FETAL IRRADIATION IN RELATION TO CANCER DEATHS FROM FALLOUT AND NATURAL BACKGROUND RADIOACTIVITY*

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Recent surveys (1) on the carcinogenic effects of unusually small amounts of irradiation (~2 r) to the human fetus during diagnostic x-ray pelvimetry of the mother make it possible to estimate that present levels of fallout (~20 mr to the fetus) may cause an increase of about 1% in the number of deaths from all types of cancer occurring in children in the 0-10 year age group. Similar estimates indicate that natural background radioactivity may be responsible for about 5% of all cancer deaths in the 0-10 year age group. These estimates assume: 1) that a proportionality or near proportionality for radiation effects on the fetus exist in the 20 mr to 2 r dose range since 20 mr = 0.02 r being 1% of 2 r. Likewise the 100 mr to the fetus from natural background radioactivity would produce a 5% effect. 2) that no dependency on dose-rate exists.

The validity and applicability of the aforementioned assumptions are discussed later. However, it can at once be stated that these estimates, derived entirely from human exposure data, are based on fewer assumptions

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and extrapolations than any others that have as yet been made. In short, they are the best that can be made at the present time on the important and pressing problems of the possible carcinogenic effects on children of low level radiation from fallout, natural background, and other sources.

The purpose of this paper is to discuss 1) the basis for the above calculations and 2) their implications as to the planning of experiments and statistical surveys in which it is desired to ascertain the carcinogenic effects of very low levels (< 10 r) of radiation in human populations. It is of further interest to investigate other consequences of fetal irradiation on such topics as threshold and the evaluation of accidents involving the release of radioactive material.

IN UTERO IRRADIATION FINDINGS

The estimates given in this paper derive from the findings of Stewart, Webb, and Hewitt (1). In their careful statistically controlled study an attempt was made to trace all children in England and Wales who died of leukemia or cancer before their tenth birthday during the years 1953-1955. These children were compared in their prenatal and postnatal experiences with an equal number of healthy children -- the control group. The necessary facts were obtained from the mothers by sending a specially appointed "survey doctor" to interview, first, the mother of a dead child, and secondly, the mother of a live child matched for age, sex, and locality but otherwise picked at random from the local birth register. By May, 1957, the mothers of 1,416 children (out of a total of 1,694) who died from cancers (677 leukemia, and 739 other cancers) had been interviewed as well as 1,416 mothers of control children.

Extreme precautions were taken to eliminate sources of bias and faulty selection of cases and controls. Such factors as contraception, illness, use of antibiotics by children, postnatal x-ray exposure, etc., common to the cases and controls were examined and evaluated.

In their retrospective study Stewart, Webb, and Hewitt found that children who have been x-rayed in utero and thus received a radiation dose of roughly 2 r are about twice as likely to die of a malignant disease before their tenth birthday as other children. The fetal radiation was delivered during diagnostic x-ray pelvimetry in the last trimester of the gestation period.

It should be appreciated that the exact amount of increase in cancer deaths produced in children from in utero irradiation received during diagnostic x-ray pelvimetry of the mother may be somewhat less or more than the factor of two reported by Stewart. Additionally, the exact radiation dosages received by the fetus are uncertain. It is possible, for example, that the fetus may receive as much as 10 r from some routine x-ray pelvimetries depending on the technique used. The important point, however, is, that despite the tentative nature of my estimates, and the wide margins of uncertainty, it now appears that radiation doses so low as to approach the radiation levels from fallout and natural background cause a significant increase in childhood cancer deaths.

It is first necessary to inquire into the validity of Stewart's findings. Similar studies still in progress in the state of Louisiana (2) and in Boston (3) thus far corroborate Stewart's findings. A large part of the criticisms which have appeared, e.g., (4) were directed at the preliminary communication of Stewart et al. (5) which necessarily omitted details of the

statistical procedures and controls which were adequately described in the final report. Some critics of Stewart's work, apparently unaware of the contents of her final study, have suggested the possibility that some or all of the excess cancer deaths are explained by better recall of abdominal radiation by bereaved mothers. Actually Stewart and her associates had definitely excluded this possibility in a carefully conducted statistical manner. Furthermore, in the similar study made in Louisiana (2) the data were gathered from professional sources, thus minimizing the risk from emotional bias or selective memory in the informants.

It must be concluded, unless a comparable weight of evidence to the contrary arises, that the carcinogenic effect of direct fetal irradiation found by Stewart et al. is not an artifact.

FETAL RADIATION DOSAGES

The second aspect of the Stewart findings to be considered is the dose received by the fetus during x-ray pelvimetry. From measurements of the total gonadal or intravaginal dose received by the mother during x-ray pelvimetry, estimates of the average corresponding fetal dosage have been made (6-9) but all must be considered uncertain. Berman and Sonnenblick (8) have pointed out that because of the position and location of the fetus, only partial body exposures occur during pelvimetry. As an estimate of the average total body dose received by the fetus, I have taken 2 r -- in approximate agreement with usually accepted estimates.

Two more estimates of dosage are needed for calculation. These are the total dosages received by the fetus from radioactive fallout and natural background radioactivity during the 9-month gestation period. The over-all

gamma and beta radiation reaching the fetus will be assumed equivalent to that received by the mother, despite differences in the localization and retention of some of the fission products and naturally radioactive elements.

First let us evaluate the average radiation dosage from natural background radioactivity. The best estimates of the total irradiation received by humans from all natural sources -- internal and external -- are those reported by the U. N. (9, p. 58) and Langham and Anderson (10). These sources give 125-134 mr per year as the average total background irradiation dose received by humans. We will assume, therefore, that the fetus receives 100 mr from natural background radioactivity during the 9-month gestation period.

The estimation of whole body radiation dosage received by the fetus from radioactive fallout can only be approximated. In my estimate I have taken into account the fact that during the past five years bomb testing has been especially heavy and frequent. The frequency of testing is important because a large fraction of our radiation exposure comes from gamma-emitting fission products deposited on the ground, especially the Zr^{95}-Nb^{95} isotopes where the Zr^{95} parent has a 65 day half-life. In the careful work by Gustafson, Marinelli, and Brar (11) it was found that relatively fresh fallout present in soil collected in October, 1957 delivers a radiation dose to the body of 22 mr per year from gamma emitters. Transient exposures from short-lived fission products such as I^{131} and Sr^{89} give important contributions to the fetus which are sometimes overlooked when averaged out over a year. It should be recognized that in the case of gamma-emitting fission products inside the body, the fetus receives a radiation dose from these isotopes deposited in the mother as well.

From data on internal emitters (10) in conjunction with the external radiation doses from fallout on the ground I estimate that the radiation received by the fetus during its 9-month gestation period is at least that given below:

Iodine-131	1 mr
Strontium-90	2 mr
Cesium-137	1 mr
Others (Ru^{103}, C^{14}, Ba^{140}, Sr^{89}, Zn^{65}, etc.)	1 mr
External from ground (Weathering and shielding factor of 4 is assumed.)	5 mr
Total	10 mr

It is necessary to take into account the fact that several percent of the world's population have received at least twice the average exposures listed above and that the weathering and shielding factors in many heavily populated areas are less than 4 (9, p. 119). These factors plus a "safety" factor lead me to assume that the fetal dosages from fallout for an appreciable fraction of the world's population lie in the range of 10 to 30 mr during the 9-month gestation period. For the estimations given below I will take 20 mr as the basis for calculation.

From Stewart's findings and the above estimates of radiation doses from fallout and natural background, and including assumptions stated at the beginning of this article, it follows that:

1. Present levels of radioactive fallout are producing an approximate 1% increase in the death rate from cancer in the 0-10 year age group.
2. The natural background radioactivity accounts for approximately 5% of all cancer deaths in the 0-10 year age group.

THRESHOLD FACTORS

Stewart's findings have some general bearing on the question of the existence of a radiation threshold. The fact that a dose of about 2 r delivered *in utero* produces a comparatively large effect, namely a 100% increase in the probability of developing a fatal cancer, means that the number of r required to produce a 1% increase in the cancer death rate would be less than 2 r.

A threshold, if it exists, must necessarily lie below a doubling dose of about 2 r. There is no generally acceptable biological or physical argument to justify the *ad hoc* hypothesis that a threshold lies above 20 mr and below 2 r, therefore, it seems prudent for the present to conclude that no radiation threshold above 20 mr for the fetus exists. A threshold concept should not now be a factor in the setting of permissible levels for the world's population.

DOSE-RATE EFFECTS

One factor which under certain conditions diminishes the magnitude of radiation effects is a lower dose-rate. The radiation received by the fetus from x-ray pelvimetry is practically instantaneous, while the dosage from fallout or natural background is delivered over a 9-month period. However, it is sometimes not appreciated that the evidence cited to support the contention that radiation delivered at low dose rates is less carcinogenic than that delivered at high dose rates, is based on high total doses of ~200 r or more. There is at present no direct evidence for or against the existence of a dose-rate effect for chronic effects such as cancer or life-shortening in the region of doses of the order of a few r or less (12).

Experimental evidence with regard to life-shortening (13) and certain genetic effects (14) indicate that dose-rate effects which are considerable for large doses and comparatively high dose rates, tend to disappear with decreases in both dose and dose-rate.

Another factor which could influence the magnitude of the risk is the relationship between the radiosensitivity of the fetus relative to time after conception. The data of x-ray pelviustry are derived from human fetuses irradiated in the last trimester. However, the somatic irradiation produced by fallout and natural background begins at conception. It is universally assumed that the fetus is most radiosensitive in the first trimester and, indeed, the entire uterine period is assumed to be more vulnerable to radiation injury than the postnatal period (9, chapter V; 15,16,17).

CONSEQUENCES OF FETAL IRRADIATION

Specific examples of the possible consequences of irradiation to the fetus from fallout and natural background are of interest. According to available vital statistics (18), about 3000 children in the 0-10 year age group now die from all types of cancer per year in the U. S. A., of which leukemias constitute about one-half. From the estimates presented in this paper it follows that of these 3000 childhood cancer deaths roughly 30 could be caused by fallout radiation (1% of 3,000) and 150 by natural background radiation (5% of 3,000). It also means that ionizing radiation itself is probably not a major cause of leukemia and cancer in childhood -- a conclusion cited by Stewart et al. (1) and Burnet (19).

It is interesting to note, however, that diagnostic medical radiation from x-ray pelvimetry may be an appreciable factor in childhood cancer deaths in many countries. At the present time, for example, an estimated 11% of U. S. children are subjected to irradiation *in utero* from diagnostic x-ray pelvimetry (7, p. 14). Consequently of the 3,000 cancer deaths annually in the U. S. A. in the 0-10 year age group, about 400 can be attributed to x-ray pelvimetry.* While some radiologists claim that the frequency of x-ray pelvimetry examinations is being reduced (e.g., 20)

*The estimated number of childhood cancer deaths from x-ray pelvimetry is arrived at as follows: In any one year there are about 4,000,000 births in the U.S.A. Of these, 4,000,000 x 0.11 = 440,000 have been irradiated *in utero* from x-ray pelvimetry. The normal death rate from childhood cancer is ten per hundred thousand per year which means that of these 440,000 children 440 would die within the next ten years. However, because the fetal irradiation doubles the normal death rate, the total deaths resulting from a single year of x-ray pelvimetry would be 440 x 2 = 880 in ten years, i.e., 440 additional deaths or 44 per year. Since a new group of children are irradiated each year, there would be a "piling up" of childhood cancer deaths from x-ray pelvimetry. X-ray pelvimetries have been a relatively common practice for many years -- and still is as far as is known. At equilibrium, the average number of excess deaths in a population of 4,400,000 fetally irradiated children in the 0-10 year age group, would be 10 x 44 = 440 per year. An approximate correction for the fact that the normal death rate includes children irradiated *in utero* reduces the latter figure to 400 per year. The approximate nature of this estimate does not warrant further corrections.

such assertions have no substance until such time as supporting evidence is produced. Information on the frequency of any diagnostic x-ray examination must be based on surveys of large numbers of individuals and hospitals over a wide area as was reported in (7).

We can extend the numerical estimates of cancer deaths produced by fallout and natural background to the children in the world's population by using a multiplication factor of only ten which takes into account the smaller fallout levels in the southern hemisphere. Consequently, radioactive fallout could cause, taking the U. S. A. rate as a rough measure, at least 300 additional deaths annually in children in the 0-10 year age group while natural background radioactivity could account for 1500 deaths annually.

The contribution to cancer deaths from fallout and natural background could be greater in certain areas. For example, in the areas near atomic testing sites, radiation dosage from local or nearby fallout can reach relatively high levels (21). Bauer (22) has estimated that up to the summer of 1957 some 10,000 persons in southern Utah have been subjected to fallout from the Nevada test site equaling a full-body gamma-ray dose of about 3-4 r. He further estimates that the average full body dose received by the million people nearest the atomic test site for the first six years of testing was 0.1-0.2 r. Among a heterogeneous population, about 2% are pregnant at any one time. It is, however, practically impossible to link present levels of fallout radiation directly to an increase in childhood cancer deaths in this population since in the 0-10 year age group the cancer death rate is normally about 1 per 10,000 per year.

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There are several other important inferences which can be drawn from Stewart's findings:

They provide a more sensitive population - probably the best - to which attention must be directed for evaluating radiation effects on whole populations by a consideration of the carcinogenic effects on the fetus. The writer and R. E. Lapp have already pointed out that the setting of permissible levels for global populations should be based on the radiosensitivity of the most vulnerable age, health, or genetic group as long as they contribute an appreciable fraction of the total population (23).

Also to be considered is the evaluation of the effects of momentary high exposure as a result of accidents such as in the release of I^{131} in the Windscale reactor accident. In addition to averaging out the temporary high exposures over the whole lifetime for the entire population, it would be desirable to evaluate separately the potential effects from the total number of children who received a given dose in utero.

In medicine, the effect of administration of radioisotopes for diagnostic purposes to pregnant women should be re-evaluated in terms of fetal dosage, keeping in mind that as little as 20 mr may produce an increase of 1% in the probability of childhood death from cancer.

The design of experiments for ascertaining the effects from irradiation of both very low dosages and low dose-rates could very well focus on a program such as one involving momentary radiation to the embryo corresponding to the last trimester of the human gestation period. Such an approach, at least for exploratory purposes, would involve far smaller animal populations than one in which relatively mature animals are irradiated.

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BONE TUMOR INTEGRATED DEATH RATES

Harry Auerbach

The death rates available from Vital Statistics Offices are generally the crude death rate and the age and sex specific death rates. A specific disease crude death rate such as the male bone tumor death rate for the United States, 1950, would be recorded as 1.73 per 100,000 population. This is derived by dividing the total number of male bone tumor deaths in 1950 (1295) by the mid-year estimate of total male population (74,855,000), and means that for every 100,000 males alive during the year an average of 1.73 bone tumor deaths occurred. Age specific death rates are based on the number of deaths that occur in a specific age group of the population. In 1950 the U.S. male bone tumor death rate for age 35-44 was 0.77 per 100,000 and for age 65-74 was 8.19 per 100,000. The crude death rate is based on all deaths in the total population and because the age specific death rates increase with age, the crude death rate can increase, without any change in the age specific death rates, by an increase in the proportion of older people in the population. This may be falsely interpreted as a real increase in the incidence of bone tumors.

In studying bone tumor deaths among populations working with or subjected to environments of radium or Sr^{90}, a decision as to an increase in the number must be based on the number of tumor deaths normally expected in an unexposed population. The computation of this number is based on use of life tables for the population and on age specific bone tumor death rates for the population. Table 44 shows the calculation of bone tumor deaths to be expected in following 100,000 females from age 15 to the death of the entire group. This shows that of 100,000 fifteen-year-old females, a total of 117 would eventually die of bone tumors. This can also be used to show that if 100,000 fifteen-year-old females are followed for thirty years to age 45, 5160 would die of all causes and 15 would die of bone tumors.

Table 45 shows the total deaths and bone tumor deaths to be expected in populations of 100,000 males and females observed for 30 years.

The implication of these integrated death rates is that unless large populations are observed for bone tumors, manyfold increases in bone tumor death rates due to environmental or other factors may go undetected. By constructing series of these integrated bone tumor death rates, it can be estimated that of the 170,000,000 people now alive in the United States approximately 200,000 will die of bone tumors. Similar integrated death rates can be derived for any cause of death or different populations.

TABLE 44

Bone tumor deaths in female populations*

Age group	Population at start of period	Man years at risk in period	Death rate per 100,000 man years	Bone tumor deaths in period	Cumulative bone tumor deaths
15-24	100,000	996,000	0.62	6.18	6.18
25-34	99,120	984,262	0.32	3.15	9.33
35-44	97,683	964,131	0.59	5.69	15.02
45-54	94,840	921,845	1.35	12.44	27.46
55-64	88,846	833,375	3.00	25.00	52.46
65-74	76,843	666,229	4.58	30.51	82.97
75-84	54,028	377,656	6.34	23.94	106.91
85+	21,428	104,997	9.79	10.28	117.19

*Based on U.S. life tables 1949-1951 and U.S. 1949-1951 Female Bone Tumor Death Rates.

TABLE 45

Total deaths and bone tumor deaths in populations of 100,000 followed for 30 years*

Age at beginning of observation period	Males		Females	
	Total deaths	Bone tumor deaths	Total deaths	Bone tumor deaths
15	7,921	21	5,160	15
25	16,011	27	10,365	21
35	32,907	55	21,334	44
45	58,146	96	43,032	72
55	85,084	127	75,882	89

*Based on U.S. 1949-1951 life tables and U.S. 1949-1951 Bone Tumor Death Rates.

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A SURVEY OF CHILDHOOD MALIGNANCIES

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SECTION I. BACKGROUND TO THE SURVEY

The present survey is based on an earlier study of the vital statistics relating to leukaemia (Hewitt, 1955). This had revealed an unusual peak of mortality in the third and fourth years of life which indicated that the subsequent survey should, in the first instance, be restricted to children. The earlier investigation had also led to the suggestion that it might be particularly worth while to study modern innovations, such as radiology.

Method

An attempt was made to trace all children in England and Wales who had died of leukaemia or cancer before their tenth birthday during the years 1953 to 1955 (case group) and to compare their pre-natal and post-natal experiences with those of healthy children (control group). Details of control selection and method of recording data are given later, but the basic idea was to obtain the necessary facts from the mothers by sending a specially appointed " survey doctor " to interview, first, the mother of a dead child, and, secondly, the mother of a live child matched for age, sex, and locality but otherwise picked at random from the local birth register.

Available Cases.—The total number of deaths in the category required was 1,694, of which 792 were ascribed to leukaemia and 902 to other cancers (Registrar-General, 1954–6). By May, 1957, the mothers of 1,416 of these children had been interviewed (677 leukaemia

*In receipt of a grant from the Medical Research Council.

and 739 other cancers). The lost cases represented 16.4% of the total, and included 6.8% who belonged to families that had moved abroad or to an unknown address and 3.1% where the interview could not be arranged in time. The remainder represented refusals to co-operate, 4.5% by parents and 2.0% by doctors.

Appointment of Survey Doctors.—It was a basic principle of the survey that the same doctor should interview both mothers of a given case/control pair, but that pairs in different local authority areas might be seen by different doctors. The 90 pairs belonging to the London County Council area were seen by one of us (J. W.) and the remaining 1,326 pairs by "survey doctors" appointed by the principal medical officer of health to the areas. All local authorities co-operated, so that all parts of the country are represented in the survey. To ensure uniformity in the recording of data and the selection of controls one of us (A. S.) visited each one of the health departments to give detailed instructions about field-work procedures.

Collecting of Data

Each survey doctor was given a list of the cases in his area. If the mothers or foster-mothers were still living in the area he was to see them and the corresponding control-mothers : if a mother had left the area he was to find and interview a control, but return the case papers to Oxford. These were eventually sent on to the new area, but in this way a central record was kept of all "transfers." No case/control pairs seen by different doctors have been included in the analyses making direct comparison between cases and controls, but they may feature in other analyses—for example, incidence of mongolism. Since the records obtained from foster-mothers contained no information about the pre-natal environment they too have been excluded from the case/control comparisons.

The questionaries for recording the interviews were the same for cases and controls, and were distributed in pairs bearing the same serial number and a so-called final date. This was the date of death of the case, and was a reminder that the medical history of the control child should cease at the so-called onset date—that is, the date when the corresponding child fell ill. The first half of the questionary described the children's medical experiences before the onset date—that is, illnesses, *x*-ray

exposures and antibiotics—their feeding habits, and their exposures to non-medical ionizing radiations (television, luminous toys, and pedoscopes) during this period, The second half described the mother and other relatives. The mother's illness and *x*-ray histories ceased at the date of birth of the survey child, but the family histories continued to the date of the interview (this allowed the total number of children in the family, and the number of relatives dying from cancer, to be the number up to the time of the interview). The mother's illness and *x*-ray histories were recorded separately for three periods of her life: (1) the first period, before marriage; (2) the middle period, between marriage and the relevant conception; and (3) the final period, during the relevant pregnancy. If the child was illegitimate the first period extended to the relevant conception.

Control Selection

It was clear that some "matching" of the live and dead children was needed, but it was decided to restrict this to three features of cancer deaths which it was not intended to study—namely age, sex, and locality. The distribution of the dead children in respect of these three influences could be readily obtained from official mortality statistics, but as yet nothing was known about, for example, their parity or social class distribution.

Accompanying the papers for each case/control pair was a so-called control selection list. On this was entered the name of the dead child, its sex, when it was born, and the home address at the time of death. Space was provided for half a dozen names of mothers who, in the stated locality, gave birth to a child of the same sex in the same month or half-year. These names were to be obtained from official registers of births. When completed, the list might be passed to a health visitor with instruction to visit the houses in the order in which they were listed. If it proved impossible to arrange an interview with the first mother the reason was to be entered on the list and the second house visited. In this way a record was kept of all first and later choices, also the reasons for not obtaining the control of choice.

Survey Objectives

The survey doctors were told that the purpose of the survey was to compare the medical and social histories of the children before and after birth, and that the promoters were interested both in the nature of all "previous"

illnesses and in the investigations and treatments associated with them (the schedules had separate headings for diagnoses, antibiotics, and *x*-ray histories).

Interviewing began in December, 1955, and by the following August 547 case/control records had been completed and returned to Oxford. These formed the basis of a preliminary report, which showed that abdominal *x*-ray films in the relevant pregnancy—that is, direct foetal irradiation—had been reported in 85 cases and 45 controls (Stewart, Webb, Giles, and Hewitt, 1956). For records completed after this date the corresponding numbers were 107 and 58. There is therefore nothing to indicate that awareness of an important finding has affected these records.

Preliminary Findings.—The 1,416 case/control pairs completed by May, 1957, included 89 transfers and 28 adoptions. After removing these 117 pairs the remaining 1,299 (on which most of the findings are based) included 619 leukaemias and 680 other cancers. The controls were represented by 775 first choices and 524 later choices. Only 60% of first choices may seem a low proportion, but the birth registers from which the names were taken had been compiled, on average, six or seven years before the survey began. With the use of a register which is revised annually the Survey of Sickness obtained 84% of their first choices (Logan and Brooke, 1957). A quarter of the children wanted as controls had definitely left the district and a further 5% (marked "no reply") may have done so. Only 6% of the mothers either refused to co-operate or were dead, and 3% were deliberately rejected because the child was dead or had always lived away from its mother. However desirable it may be in theory to include such children in the sample, in practice the records would have been so defective that they would have been of very little use for case/control comparisons.

Failure to obtain 100% of first choices has led to deficits in the control group of three types of children—namely, first-born children, migrants, and twins.

First-born Children and Migrants.—The cases included 510 first-born children and the controls 427. By national standards the first figure is nearer expectation (approximately 500) than the second, it is therefore reasonable to suppose that there is a genuine deficit on the control side and that it is related to the fact that one-child families tend to move house more often than larger families. Because of this deficiency we have, wherever indicated, done separate analyses of the first- and later-born children of primiparae and multiparae, and of migrant and static families.

Twins.—The birth registers from which the controls were taken were, in effect, lists of maternities; hence the con-

trol group should contain approximately half the normal proportion of twins. The actual number of control twins was 15 and the "expected" number in the region of 28. By this calculation the number of twins in the case group, 33, is higher, but not significantly higher, than the expected number. Though numerically small, the twins are from the point of view of foetal irradiation important. For this reason they are considered separately in Section II.

In spite of the larger number of first-born children in the case group there was no excess of small families. By the time of the survey most of these children had acquired younger brothers and sisters, and the average family size was the same in both groups (see Section VI).

Demographic Characteristics

The following information about the age, sex, and locality distribution of the cases is based not on the survey findings but on death certificate data.

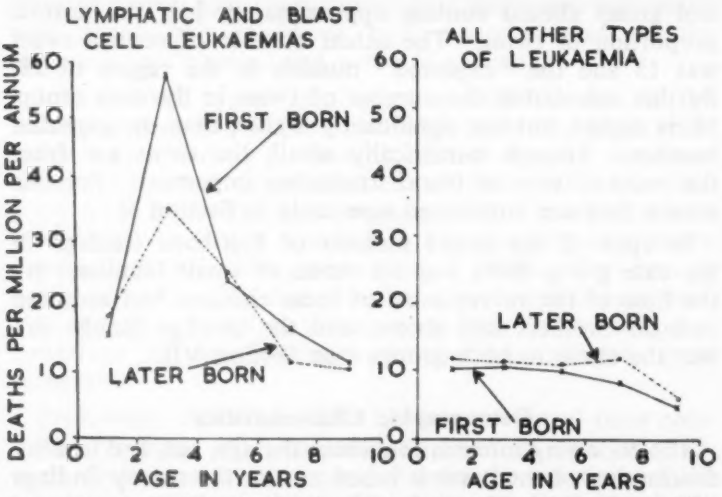
Age and Sex.—The death rates were higher for males than for females and for children under 5 than for children aged 5 to 10. In the leukaemia series the early peak of mortality was shown by a higher death rate for children between 2 and 4 years of age than for younger or older children.

Locality.—Only two regions—Surrey and Manchester—had a suggestive excess of leukaemia deaths, but in general these and the other cancer deaths showed a remarkably even geographical distribution. Classification by size of town revealed greater contrasts, but the highest death rates were in medium-sized towns, not in the largest (see Table I).

TABLE I.—*Comparative Mortality Ratios for Age Group* 0–14 *in Five Density Aggregates,* 1953–5 (*England and Wales*=100)

	Leukaemia	Other Malignant Disease
Conurbations	108	100
Towns with over 100,000 inhabitants	98	100
,, 50,000 to 100,000 inhabitants	124	110
,, under 50,000 inhabitants	92	96
Rural areas	86	100
$\chi^2_{(4)}$	12·372	1·451
Value of P	<0·02	>0·80

Birth-rank Distribution.—Since the control group was deficient in first-born children the birth-rank distribution of the cases has been compared with national figures for the years which corresponded to the births of these children—that is, 1943–55 (Registrar-General, 1945–56). According to these statistics, the birth-rank distribution of childhood cancers, other than leukaemia, is typical of the population at large, but among leukaemic children there appears



Approximate leukaemia death rates of first- and later-born children, 1953–5. These rates are based only on 663 leukaemia cases of known parity The inclusion of 14 adopted cases and 115 cases not followed up would give somewhat higher rates per million but would be most unlikely to change the appearance of the graphs.

to be a 10% excess of first-born children. Division of these children into two groups—(1) lymphatic and blast-cell leukaemias, and (2) other leukaemias—revealed further peculiarities (see Chart). Thus, between the ages of 2 and 4 years the distribution of lymphatic and blast-cell leukaemias indicates a 70% higher risk for first-born children than for other children. Whether this risk is related to the antenatal or post-natal peculiarities of first-born children we do not know. It is, however, deaths in this narrow age group which are largely responsible for the remarkable post-war increase in childhood deaths from leukaemia both in this country and in the U.S.A. (Hewitt, 1955). Our own data on post-natal *x*-ray exposures (see Section IV) also suggest that some of the children who survive to the age of 2, but are dead of leukaemia before the age of 4, belong to a separate aetiological group.

Maternal Age.—The average age of the mothers at the date of birth of the survey children was 28.91 years for the leukaemia series and 28.43 for the other cancer series—that is to say, the group which contained the higher proportion of primiparae was, on average, 25 weeks *older* than the other group. Further examination of the maternal ages showed that this greater average age was determined by a small group of mothers who were over the age of 40 when the leukaemic child was born. These mothers represented 6.9% of the leukaemic series and only 3.5% of the other cancer series. To avoid confusion with the high incidence of mongolism in the leukaemia series (see Section IV) the mothers of mongols were excluded from the next analysis

and primiparae were separated from multiparae. In the latter group (which contained the bulk of the women over 40) the average age was 52 weeks higher for the leukaemia than for the other cancer series, a difference which is statistically significant ($P<0.01$). Assuming that age of the mother has not affected the prevalence of other childhood cancers, this suggests that the risk of dying of leukaemia before the age of 10 years is twice as great as usual if the mother is over 40 years of age at the date of birth (this estimate is in no way dependent on the slightly higher incidence of obstetric *x*-ray examinations in elderly women). The independent findings, first, of a large number of mongols in the leukaemia series, and, secondly, of a relatively large number of "old" mothers in the leukaemia series suggest that childhood leukaemia and mongolism are influenced by a common factor, rather than that one disease predisposes to the other (see also Section IV). In a recent American survey the ages of the mothers in a smaller group of children with leukaemia and others cancers also show these contrasts (Manning and Carroll, 1957).

Social-class Distribution.—The basis of the social-class distribution was the father's occupation. A statement of family income was also obtained for 87% of cases and 88% of controls. Since it is common knowledge that professional families move house more often than working-class families, the social and economic distributions have been calculated separately for children who moved house after they were born (migrant series) and children who did not (static series). On this basis there are virtually no social-class or economic distinctions as between cases and controls or between cases of leukaemia and cases of other cancer. (National mortality rates suggest that leukaemia deaths at later ages tend to have an upward social gradient, and other cancer deaths a slight downward gradient.)

SECTION II. MOTHERS' X-RAY HISTORIES

Although the mothers were questioned first about their children and only later about their own health, we shall deal first with the mothers' *x*-ray histories and illnesses. These were recorded separately for (1) the first period, before marriage; (2) the middle period, between marriage and the relevant conception; and (3) the final period, during the relevant pregnancy. Three categories of exposure were distinguished—therapeutic, occupational, and diagnostic. The first two need not detain us. Only 16 mothers (7 cases, 9 controls) reported any radiotherapy and the treatments never coincided with the relevant pregnancy. A further 19 mothers (12 cases, 7 controls) may have been exposed to *x* rays in the course of their work, and for three —all mothers of cases—the work was continued during the final period. The occupations in question were: nurse in

radiotherapy department, nurse in a radiological department, and a metal-tester.

Diagnostic X-ray Histories

A summary of the mothers' diagnostic *x*-ray histories is given in Table II. Figures are given for the three periods, separately and jointly, and for two main types of *x*-ray examination (abdominal and other). The figures are numbers of *mothers*, not numbers of films or examinations—that is to say, no woman has been counted more than once

TABLE II.—*Number of Mothers Reporting X-ray Examination of Different Sites (Abdomen and Other) in Three Periods (see Text)*

Period	*X*-ray Examinations		
	Abdominal	Other	Any
Before marriage	*44/26=*1·69*	335/275=*1·22*	361/296=*1·22*
Between marriage and relevant conception ..	109/121=*0·90*	213/184=*1·16*	304/285=*1·07*
During relevant pregnancy	178/93=*1·91*	117/100=*1·17*	273/184=*1·48*
Any period	296/215=*1·38*	531/456=*1·16*	692/593=*1·17*

The case/control ratio of 1·91 for abdominal *x*-ray exposures during the relevant pregnancy (1) differs from the "expected" ratio (1·00) at the level of $P < 10^{-6}$; (2) differs from the contemporary ratio for other *x*-ray exposures (1·17) at the level $P \simeq 0{\cdot}011$ and from the ratio for other *x*-ray exposures in any period (1·16) at the level $P < 0{\cdot}001$; and (3) differs from the ratio for abdominal *x*-ray exposures in any period (1·38) at the level $P \simeq 0{\cdot}012$.

* 44/26, etc., represent ratio of case mothers to control mothers.

in any cell of the Table (consequently the figures for "any period" and "any *x*-ray exposure" are rather lower than the sum of the figures for separate periods and separate types of examination). The figures in italics give the ratios between the numbers of case mothers and the corresponding numbers of control mothers.

It will be seen that 692 mothers of cases reported at least one diagnostic *x*-ray exposure compared with only 593 mothers of controls, a case/control ratio of 1.17. Though this case excess is significant, it is not very large, and might perhaps be attributed to relative under-reporting of *x*-ray exposures on the control side. If, however, under-reporting by control mothers was the only reason for the case excess one would expect to find similar case/control ratios for each type of examination and for each period of life. In fact the ratio is higher for abdominal (1.38) than for other types of examination (1.16), and higher for all examinations during the relevant pregnancy (1.48) than during earlier periods (1.22 and 1.07). In particular, the ratio for abdominal *x*-ray examinations during the relevant pregnancy is outstandingly high (1.91). The chance probability of obtaining so high a ratio is less than one in ten million. Moreover, as the footnote to Table II shows, this ratio is significantly higher than the ratio for *x*-ray examinations of other

sites in this period, and than the ratio for abdominal *x*-ray examinations as a whole. Hence there is prima facie evidence that abdominal *x*-ray examinations during pregnancy—the only type of examination involving direct exposure of the foetus—can contribute to the aetiology of malignant disease in children.

Most of the remainder of this section is devoted to further analysis of the apparent association between foetal irradiation and malignant disease in childhood, but it is necessary first to say something about the figures for the other maternal *x*-ray histories.

One group of maternal *x*-ray histories—those relating to *x*-ray exposures of the abdomen during the middle period—shows a small excess on the *control* side. This was at first thought to be due to the fact that on the control side there were more previous children, and hence more occasions for obstetric *x*-ray exposures in the middle period. However, when the obstetric *x*-ray examinations were separated from the other abdominal *x*-ray examinations belonging to this period and related to the number of pregnancies at risk (see Table III) there was still a slight excess on the control side. This strengthens the view that the control mothers were as efficient as the mothers of cases in recalling their abdominal *x*-ray examinations.

TABLE III.—*Comparison Between Cases and Controls in Respect of Obstetric X-ray Examinations in the Middle Period (i.e., Between Marriage and the Relevant Conception)*

	Cases	Controls
Total No. of previous pregnancies	1,623	1,742
No. of women reporting obstetric (abdominal) *x*-ray examinations between marriage and relevant conception:		
(*a*) Actual	79	96
(*b*) Expected	84·41	90·59

In the pre-marriage period there was a case excess for abdominal *x*-ray exposures which has to be accepted as technically significant ($P<0.05$). The possibility has therefore to be considered that damage to the maternal gonads may increase the risk of childhood malignancy. But even if this is the correct explanation such damage is of minor interest, for, as judged by the numbers involved, its importance can only be one-quarter of that of direct foetal irradiation.

Unlike the case excess for abdominal *x*-ray examinations, that for other *x*-ray examinations showed no tendency to concentrate in the period of the relevant pregnancy. This seems to dispose of the notion that scatter from *x*-ray exposure of sites other than the abdomen had been harmful to the cases. The meaning of the steady case excess in all periods is not clear. To be on the safe side we will

assume (see Table VII) that it is an average measure of the relative under-reporting of *x*-ray exposure by control mothers compared with the case mothers. However, even this group is not completely homogeneous as regards case/control comparisons. On further breakdown it was found that mothers reporting chest *x*-ray examinations were in substantial excess on the case side (ratio of 1.20), but that the numbers reporting skull or limb exposures were virtually identical (133 and 132).

The first step in pursuing the association between foetal irradiation and malignant diseases was to check all reports of abdominal *x*-ray examinations during the relevant pregnancy against hospital records. This revealed a few inaccuracies. Five mothers of cases and five of controls were found to have reported an examination which in fact had been performed during a different pregnancy. In a further 14 instances (9 cases and 5 controls) no hospital record of the alleged examination could be found. The figures shown in Table II do not include any of these faulty records (the effect of removing them was to alter the case/control ratio from 1.86 to 1.91).

Twins.—As already explained there are only 15 twins in the control group compared with an expected number of 28 (see Section I). Since seven of these 15 children had been *x*-rayed *in utero* it is reasonable to suppose that with a full quota of twins there might have been six more control mothers reporting abdominal *x*-ray examinations in the final period.

First-born Children.—Separate analysis of the maternal *x*-ray histories in groups defined by parity (Table IV) showed that the proportion of first-born children who were *x*-rayed

TABLE IV.—*Histories of Direct Foetal Irradiation, Distinguishing Three Birth-rank Groups of Cases and Controls*

Position in Family of Child (Birth Rank)	Cases		Controls		Ratio
	No.	%	No.	%	
First	85/510	16·7	36/427	8·4	1·99
Second	47/393	12·0	28/448	6·3	1·90
Later	46/396	11·6	29/424	6·8	1·71
All ..	178/1,299	13·7	93/1,299	7·2	1·91

in utero was approximately 2% higher than the proportion for other children. This slight difference is equivalent to about two "extra" records of direct foetal irradiation on the case side.

Migration (see Table V).—Independent case/control comparisons based on the "migrant" and "static" series (see Section I) showed no significant differences between these two groups.

TABLE V.—*Frequency of Direct Foetal Irradiation of Cases Belonging to "Migrant" and "Static" Families and of Corresponding Controls*

Foetal irradiation	Migrant Series		Static Series	
	Cases	Controls	Cases	Controls
Yes	57	23	121	70
No	325	359	796	847
Total	382	382	917	917
χ^2	15·205		14·610	
P	<0·001		<0·001	

The last point to be considered is whether the high case/control ratio for abdominal *x*-ray examinations in the relevant pregnancy could be due to the existence in the case group of a condition or conditions which only incidentally caused the child to be *x*-rayed *in utero*. In this connexion it is clearly appropriate to draw a distinction between *x*-rays taken for purely obstetric reasons and other abdominal *x*-rays which happened to coincide with the pregnancy (see Table VI). According to this analysis over 90% of the *x*-ray examinations were taken for obstetric reasons, other abdominal *x*-ray examinations representing 9% of cases and 6% of controls. Inasmuch as these other abdominal *x*-ray examinations tended to take place earlier and to involve heavier exposures than the obstetric *x* rays, they might be expected to concentrate on the case side if there were a causal relationship between direct foetal irradiation and childhood malignancies. Hence the slight

TABLE VI.—*Numbers of Mothers Reporting Obstetric and Other Abdominal X-ray Examinations During the Relevant Pregnancy*

X-ray Category and Reasons for *X*-ray Examination	Cases		Controls	
	No.	%	No.	%
Obstetric:				
Position	61	36·3	29	34·5
Size (? twins)	53	31·5	29	34·5
Routine pelvimetry	35	20·8	20	23·8
Diagnosis of pregnancy	4	2·4	1	1·2
Total ..	153	91·0	79	94·0
Other abdominal:				
Barium meal	6	3·6	1	1·2
Intravenous pyelogram	3	1·8	—	—
Injury	4	2·4	1	1·2
Other	2	1·2	3	3·6
Total ..	15	9·0	5	6·0
Total of all known indications ..	168	100·0	84	100·0
Indication for *x*-ray examination unknown	10	—	9	—

excess of other abdominal *x*-ray exposures on the case side in no way disturbs this hypothesis.*

We are now in a position to judge the effect of all the factors which might possibly have contributed a spurious element to the case/control ratio for abdominal *x*-ray examinations in the relevant pregnancy. To do this we have made three assumptions which are unfavourable to an explanation in terms of a causal link between foetal irradiation and childhood malignancies. These are: (1) that the excess of twins in the case group is wholly spurious; (2) that ill-health during pregnancy is of such overruling importance that any associated *x*-ray exposure should be ignored; and (3) that the case mothers were 16% more efficient than the control mothers in reporting *x*-ray exposures (on the grounds that the case/control ratio for other *x*-ray examinations was 1.16) (see Table II). These three assumptions have been worked into Table VII, which excludes all twins,

TABLE VII.—*Actual (and Expected) Numbers of Cases and Controls With and Without a History of Direct Foetal Irradiation. Revised Basis of Comparison (see Text)*

	Cases	Controls	Total
Irradiated	141 (118·44)	81 (103·56)	222
Not irradiated	1,125 (1,147·56)	1,204 (1,181·44)	2,328
Total ..	1,266	1,284	2,550

$\chi^2_{(1)}$ with continuity correction is 9·644, equivalent to a normal deviate of 3·10; $P < 0·002$.

counts all abdominal *x*-ray examinations in the final period, other than purely obstetric *x*-ray examinations among the non-irradiated totals, and gives "expected" numbers which are calculated not on the basis of equality but on an "expected" case/control ratio of 1.16 to 1.00. In spite of these drastic modifications there is still an excess of abdominal *x*-ray examinations on the case side which is so large that it would occur by chance in less than one of 500 repeated trials.

In the remainder of this section it is assumed that there is a causal relationship between irradiation *in utero* and childhood malignancies, and four aspects of the risk involved are considered: (1) its relationship to *x*-ray dose, (2) its relationship to the maturity of the foetus at the time of exposure, (3) its relationship to the type of malignant disease, and (4) its absolute size.

Dose-Response Relationship

Court-Brown and Doll (1957) have studied the incidence of leukaemia and aplastic anaemia in adults given deep

*Judging by the number of deaths among the children of case and control mothers, the older and the younger children of the case mothers were just as healthy as the older and younger children of the control mothers, nor was there any difference in the stillbirth rates for the case and control groups (see Section VII).

x-ray therapy to the spine. Although they had only 37 cases at their disposal they were able to demonstrate a strong relation between dose and risk of disease. The present series of children includes no fewer than 178 in whom malignant disease had been preceded by antenatal irradiation, and therefore appears to offer ample material for studying dosage effects. There is, however, an important difference which nullifies the advantage of larger numbers. Court-Brown and Doll had an almost "pure" series of radiation-leukaemias, since they gave evidence that over 90% of them would not have occurred but for the preceding *x*-ray exposures. In the present series a high proportion of the cases *x*-rayed *in utero* developed the fatal disease for *other* reasons. The existence of these cases is bound to hinder recognition of a dose-response relationship.

Attempts to collect data for the study of dosage effects met with great difficulties. Over 200 different hospitals had to be asked for records, many of which related to events of more than five years previously. Radiologists supplied all the technical details available, but these were rarely sufficient to support a calculation of dose. Finally, we were reduced to using, as an extremely crude index of dose, the number of films believed to have been taken, though even this was often no more than a reasoned guess on the part of the radiologist and did not include films discarded by the radiographer. Table VIII shows a classification of the cases

TABLE VIII.—*Distribution of Cases and Controls Irradiated in Utero According to the Numbers of Abdominal Films Reported Taken During the Relevant Pregnancy*

No. of Films	Cases	Controls	Ratio
0	1,121	1,206	0·93
1	37	27	1·37
2	60	26	2·31
3	23	18	1·28
4 or more	32	10	3·20
(Unknown number)	(26)	(12)	(2·17)
Total:	1,299	1,299	1·00

and controls on this basis. Except in the group with no films the numbers are small and therefore the sampling errors of the case-control ratios are large. The group representing three films breaks the rising sequence of ratios, but at least the lowest and highest ratios (0.93 and 3.20) correspond to the lowest and highest number of films (nil and four or more films). A small group of mothers, not distinguished in the table, who had five or more abdominal *x*-ray examinations during the relevant pregnancy, included 15 cases and only 2 controls. Thus the figures are consistent with, though they do not establish, a relationship between increasing dose and increasing risk.

Timing Effects

The precise date of exposure was ascertained in most cases, and on this basis each mother was classified according to the month of the first exposure (see Table IX). The

TABLE IX.—*Distribution of Cases and Controls Irradiated in Utero According to Month of Pregnancy*

Gestation Period in Months	Cases	Controls	Case/Control Ratio
1st to 5th	18	2	(9.00)
6th, 7th	15	9	1·67
8th	20	19	1·05
9th	55	30	1·83
10th	58	25	2·32
Total known	166	85	1·95
Unknown	12	8	(1·50)
Total ..	178	93	1·91

A statistical comparison of the cases and controls in the five "known" categories above yields $\chi^2_{(4)}=9{\cdot}656$, for which $P<0{\cdot}05$.

dated series did not include a single mother examined within 28 days of conception, and only 20 mothers were certainly *x*-rayed in the first half of pregnancy. However, in this small group there were 18 cases and only 2 controls, which represents a much higher case/control ratio than the one for exposures in the second half of pregnancy (148 cases, 83 controls). The variations in the second half of pregnancy all lie within the accepted range of chance variation.

Tissue Sensitivity

Previous work suggests that, in adults, leukaemia is more likely to be produced by ionizing radiations than other malignant diseases (Court-Brown and Doll, 1957; Faber, 1957). But our own data suggest that in a foetus there may be other tissues which are at least as sensitive to *x* rays as the reticulo-endothelial system (see Table X). In this table

TABLE X.—*Comparative Incidence of Direct Fœtal Irradiation in Eight Diagnostic Groups*

Diagnosis	No. of Cases	Irradiated *in utero*		
		%	Actual No.	Expected No.
Lymphatic leukaemia	292	14·4	42	40·31
Myeloblastic ,, ...	124	7·3	9	16·24
Blast-cell and other leukaemias ..	203	13·8	28	27·93
Lymphosarcoma, other reticuloses ..	109	7·3	8	14·24
Malignant tumours of C.N.S. ..	212	12·7	27	28·75
,, ,, kidney	120	15·8	19	16·68
,, ,, suprarenals (incl. *all* neuroblastomas)	87	18·4	16	12·68
Malignant tumours of other sites ..	152	19·1	29	21·16
Total ..	1,299	13·7	178	177·99

In an 8 × 2 table the comparison of actual and expected numbers irradiated and not irradiated yields $\chi^2_{(7)}=11{\cdot}832$, compared with 12·017 at the level $P=0{\cdot}10$.

the cases have been divided into eight diagnostic groups, and the number of irradiated cases in each of these is compared with the number which would be expected if the *x*-ray exposures were evenly distributed among the cases. In calculating these expected numbers, we have standardized them in ten-yearly age groups, in order to allow for changes in *x*-ray habits during the years in which the survey children were born. On this basis the differences between the actual and expected numbers are of a magnitude which, under chance conditions, would be expected to occur in about one out of ten samples. The group labelled " other sites " had the highest percentage (19.1%). This included three retinoblastomas with three *x*-ray exposures, and 12 teratomas with seven *x*-ray exposures. We hesitate to draw any conclusion about the retinoblastomas, because in an independent series of 39 children who were said to have survived surgical removal of a retinoblastoma we found there had been only seven pre-natal *x*-ray examinations, which is no more than average for all forms of cancer. However, the chance of obtaining as many as seven *x*-ray exposures in 12 cases is only one in a thousand, so it may be that the nests of embryonic tissue contained in teratomas are exceptionally radiosensitive.

Delayed Effects of Foetal Irradiation

The survey was deliberately restricted to children under 10 years of age because it was thought that this was a convenient limit to the time that mothers might be expected to recollect events. But by so doing we may have missed some of the consequences of direct foetal irradiation. If the effects of pre-natal irradiation are exhausted by the age of 10 years one would expect the case/control ratio for foetal irradiation to be higher for children who were under 5 years of age at the onset date than for children who were 5 or over. But, as Table XI shows, this ratio is 1.73 for the younger children and 2.50 for the older children.

TABLE XI.—*Case/Control Ratio for Direct Foetal Irradiation. Comparison of Cases and Controls Defined by Age at the " Final Date "*

	Cases	Controls	Ratio
Deaths at ages 0 to 4	123	71	1·73
" " 5 to 9	55	22	2·50
" " 0 to 9	178	93	1·91

Estimates of Risk

The following estimates are based on the figures shown in Table II and necessarily represent a rough appraisal of the situation. In the case group 13.7% of the children were

x-rayed before birth and 86.3% were not. The corresponding percentages for control children were 7.2 and 92.8. On this showing children who have been *x*-rayed *in utero* are $\frac{(13.7 \times 92.8)}{(7.2 \times 86.3)}$ times, or twice as likely to die of a malignant disease before their tenth birthday as other children. Since at the present time about one in every 1,200 children in Britain die in this way, it follows that less than one in a thousand of the pre-natal *x*-ray examinations performed in recent years have led to death from malignant disease before the age of 10 years. An alternative way of expressing this estimate is to say that abdominal *x*-ray examinations of pregnant women have in recent years been responsible for (13.7−7.2) % or between 6 and 7% of all deaths from malignant disease before the age of 10 years.

To sum up: there appears to be no doubt that there is a causal relationship between pre-natal exposure to *x* rays and the subsequent development of malignant disease, and there are indications that the risk is related both to the dose of *x* rays and to the date of exposure. There is nothing to suggest that irradiation *in utero* explains the early peak of leukaemia mortality (see Section IV for data on post-natal *x*-ray exposures) and it may even cause deaths from malignant disease after the age of 10 years. The rough estimates of risk which are given here apply only to one period of time and to the *x*-ray doses in use at that time. Evidently radiotherapy of pregnant women is an extremely rare event in this country, and contributed nothing to the cases we have considered.

[ADDENDUM.—After this report had been written we received the results of an independent study (Paterson, 1958) which had been designed specifically to test the conclusion announced in our preliminary communication. This study concerned children who had died under the age of 10 in the State of Louisiana during the years 1951–5. Although conducted on a rather small scale it had one important advantage over our own inquiry: all the information was obtained direct from professional sources, thus minimizing the risk from emotional bias or selective memory in the informants. The incidence of irradiation *in utero* discovered by Paterson was as follows: among 77 children who died of leukaemia, 27.3% ; among 70 children who died of other cancers, 28.6% ; and among 293 control children who died of other causes, 18.4%. These percentages are much higher than those found in England and Wales, but show two important resemblances: (1) the significantly higher incidence of foetal irradiation among children with malignant disease than among controls, and (2) similar figures obtained for children with leukaemia and those with other forms of malignant disease. Using Paterson's figures in the same way as our own to derive an estimate of the relative risk of malignant

disease associated with irradiation *in utero*, we have $\frac{27.9}{18.4} \times \frac{81.6}{72.1} = 1.72$, which is in reasonable agreement with our own estimate.]

SECTION III. MOTHERS' ILLNESSES

The mothers were asked whether they had ever had a serious illness or injury before the survey child was born. Although these events were actually recorded separately in the three periods (see Section II), only two periods are distinguished in the following analysis—namely, before and during the relevant pregnancy. Only information of a reasonably objective and definite type has been considered, and unless a common childhood infection—for example, measles—coincided with the relevant pregnancy it has been omitted from the following analyses. After omission of these items and such vague conditions as nervous breakdowns, influenza, anaemia, and injuries other than fractures, approximately half the mothers were left with no diseases before the birth of the survey child. The remaining records were classified according to the *International Classification of Diseases* (World Health Organization, 1949), and the results are shown in Table XII.

TABLE XII.—*Illnesses of Mothers Arising Before and During the Relevant Pregnancy*

International Classification Main Category	Arising Before Relevant Pregnancy		Arising During Relevant Pregnancy	
	Cases	Controls	Cases	Controls
I. Infective	86	82	13	1
II. Neoplasms	9	11	2	2
III. Allergic, etc. ..	24	22	1	3
VI. Nervous system ..	32	37	1	0
VII. Circulatory	53	44	0	4
VIII. Respiratory	219	207	12	13
IX. Digestive	74	91	12	4
X. Genito-urinary ..	62	44	0	0
XI. Pregnancy	32	32	113	77
XII. Skin	33	43	5	5
XIII. Bones and muscles ..	15	20	3	0
XIV. Congenital	5	7	—	—
XVII. Accidents	48	57	0	0
Total No. of illnesses	692	697	162	109
,, ,, women	565	554	155	104

Illnesses Originating Before the Relevant Pregnancy.—The total number of illnesses in this category was 1,389 (cases 692 and controls 697) and the number of mothers affected was 1,119 (cases 565 and controls 554). Unlike the series reported by Manning and Carroll (1957) there were no case/control differences for allergic conditions (category III), but compared with this American series the incidence of these conditions was low. The only group of illnesses with a

substantial difference between cases and controls was category X (diseases of the genito-urinary system) with 62 records for cases and 44 for controls. The difference was confined to disease of the renal tract as such (49 cases and 30 controls), which represents a significant case excess (P approx. 0.04).

Illnesses Arising During the Relevant Pregnancy.—The total number of illnesses recorded in this period was 271 (162 cases, 109 controls) with 259 mothers affected (155 cases, 104 controls). Further analysis of the records showed that this case excess, which is statistically significant, was restricted to two categories—namely, infective disease (13 cases, 1 control) and direct complications of pregnancy (113 cases, 77 controls). The latter group (see Table XIII) is

TABLE XIII.—*Illnesses of Mothers Specifically Associated With the Relevant Pregnancy*

I.C.D. Numbers	Illness	Leukaemia		Other Cancers		Total	
		Cases	Controls	Cases	Controls	Cases	Controls
642	Toxaemias of pregnancy ..	29	31	38	26	67	57
640–641	Infections of genito-urinary tract during pregnancy..	8	3	11	7	19	10
648·0	Threatened abortion ..	14	4	13	6	27	10
Total	.. ..	51	38	62	39	113	77

dominated by toxaemias of pregnancy (67 cases, 57 controls), but there were also 29 urinary infections (19 cases, 10 controls) and 37 threatened abortions (27 cases, 10 controls). The difference between cases and controls is not significant for toxaemia of pregnancy, and the difference for urinary infections is complicated by the fact that six of the mothers on the case side had also had an abdominal *x*-ray examination during the relevant pregnancy (mostly intravenous pyelograms). This leaves threatened abortions and infective diseases as the only ones with a significant

TABLE XIV.—*Infections During the Relevant Pregnancy*

Maternal Infection	No. of Cases	Corresponding Children
Rubella	3	Congenital sarcoma of mediastinum Neuroblastoma at 12 months Lymphoblastoma at 15 months
Mumps	2	Congenital sarcoma of the meninges Leukaemia at 5 years
Herpes zoster ..	4	Healthy control Leukaemia at 7 years Leukaemia at 8 years Cerebral tumour at 6 years
Infective hepatitis ..	2	Leukaemia at 4 years Cerebral tumour at 6 years

and unambiguous excess on the case side. The infective disease group included 10 virus infections on the case side and one on the control side. These undated infections were associated with the childhood diseases shown in Table XIV.

To summarize: there is nothing in the illness records to suggest that the case mothers were, before the relevant pregnancy, less healthy than the control mothers. During the relevant pregnancy they appear to have suffered more from threatened abortions and virus infections than the control mothers, but the numbers involved are small.

SECTION IV. CHILDREN'S X-RAY HISTORIES

As already stated, the survey doctors had been told to record the illnesses of the children up to but not beyond the onset dates, these being defined as the dates on which the corresponding children first showed signs of the fatal disease. (In the following analyses the years before this date are referred to as the *pre-onset* period.) Since it was more likely that the mothers of the living children would mistakenly include events after this date than the mothers of dead children, we have in the following analyses discriminated carefully between accurately dated *x*-ray exposures and illnesses and those in which there was a margin of error. We will deal first with non-medical *x*-ray and then with medical *x*-ray exposures, but pedoscope exposures will be mentioned in both parts.

Non-Medical X-ray Exposure.—Three non-medical sources of ionizing radiations were considered—television sets, luminous clocks or toys, and pedoscopes. Only one fact relating to these (undated) exposures has been analysed—namely, whether the child was ever exposed during the pre-onset period. For each of the three sources there was a slight excess on the control side. In the case of television sets (278 cases and 301 controls) and luminous objects (328 cases and 333 controls) the differences were negligible, and even for pedoscopes (212 cases and 242 controls) they were not significant (P approximately 1 in 7). As, however, the exposures were undated, it is possible that the few extra cases in the control side represent exposures which occurred after the onset date. The possible effects of this type of mistake on the pedoscope records are considered again at the end of this section.

Medical X-ray Exposure.—Records of diagnostic and therapeutic *x*-ray exposures were comparatively uncommon, but a slight excess was shown on the case side (see Table XV). The excess was confined to 11 children having radiotherapy (8 cases, 3 controls) and 167 children who had been radiographed on more than one occasion (90/77). For single radiographic examinations there was a slight deficiency of cases (88/100).

TABLE XV.—*Numbers of Children With Post-natal Medical X-ray Exposure*

Type of Exposure	Leukaemia		Other Cancers		Total	
	Cases	Controls	Cases	Controls	Cases	Controls
Diagnostic:						
Once*	43	42	45	58	88	100
More than once	47	38	43	39	90	77
Therapeutic ..	5	1	3	2	8	3
Total ..	95	81	91	99	186	180

* Includes some children for whom the number of examinations was not ascertained.

Tissue Sensitivity.—When a child is *x*-rayed *in utero* it is safe to assume that the whole body has been exposed; but after birth this rarely happens. It was therefore appropriate to examine the records for an association between the parts of the body *x*-rayed after birth and subsequent developments. We have, however, searched the records in vain for any sign of this.

Estimate of Dose.—No attempt was made to discover the number of films taken at each *x*-ray examination or to check the doses used in radiotherapy. The latter were all of the type to treat minor skin conditions, and no examples of deep *x*-ray treatments of the kind recently followed up by Simpson and Hempelmann (1957) were recorded. The 11 children with histories of radiotherapy were all girls treated for naevi. The cases included five leukaemias, one glioma, one neuroblastoma, and one sarcoma of the umbilicus (not the site exposed to *x* rays). At least eight of these children (possibly nine) were treated before their first birthday, and two (both cases) had already been *x*-rayed *in utero*.

Time Relationship

In view of the evidence already presented that a foetus may be specially radiosensitive, the post-natal *x*-ray exposures (diagnostic and therapeutic combined) were analysed by age at first exposure (see Table XVI) to discover whether there were any time relationships. Included in this table are 67 children (27 cases, 40 controls) who have been classified as "age at first exposure unknown." These children were definitely *x*-rayed before the onset date, but the possible dates covered a period of more than 12 months. The remaining children included 128 where the month and year of the first *x*-ray exposure was known, and 171 where the date was given to within 12 months. In the latter group we have assumed that the actual date of exposure was midway between the two possible extremes and have placed the child accordingly. The last two columns of the table (where all cases and all controls are compared) show a significant excess on the case side of children exposed before their third birthdays. This excess is greater for the leuk-

TABLE XVI.—*Distribution of Children With Post-natal Medical X-ray Exposure According to Age at First X-ray Exposure*

Age at 1st Exposure	Leukaemia		Other Malignant		Total	
	Cases	Controls	Cases	Controls	Cases	Controls
0–	19	12	23	13	42	25
1–	21 } 57	10 } 31	12 } 50	15 } 43	33 } 107	25 } 74
2–	17	9	15	15	32	24
3–	6	4	10	13	16	17
4–	5	14	9	8	14	22
5–	5	9	5	6	10	15
6–	5 } 25	3 } 33	2 } 27	3 } 33	7 } 52	6 } 66
7–	3	2	1	3	4	5
8–	1	1	0	0	1	1
9–	0	0	0	0	0	0
Not known	13	17	14	23	27	40
Total ..	95	81	91	99	186	180

aemic children than for the children with other cancers. For first exposures between fourth and tenth birthdays the balance is on the control side, but the absolute and relative difference is small and may well be due to chance. In the interpretation of these figures much depends on the 67 children whose first exposures were inadequately dated. If the first exposure dates for these children were distributed in roughly the same way as in the dated series it would strengthen the impression of a genuine difference between early and later exposures, but if the distribution were markedly different it might have the opposite effect.

No such reservations need apply to the figures in Table XVII. Here the children have been placed according to their age in years at the onset date, and only three children have had to be relegated to the "age unknown" category. In three of the five age groups there were more cases than controls, and in two there were more controls than cases, but the most striking case/control difference relates to children who were aged between 2 and 3 years at the onset date (58 cases, 27 controls).

TABLE XVII.—*Distribution of Children With Post-natal Medical X-ray Exposure According to Age of the Child at the Onset Date*

Age at Onset Date (Years)	Leukaemia		Other Malignant		Total	
	Cases	Controls	Cases	Controls	Cases	Controls
0, 1 ..	6	7	14	10	20	17
2, 3 ..	35	11	23	16	58	27
4, 5 ..	23	25	21	32	44	57
6, 7 ..	17	22	17	27	34	49
8, 9 ..	14	15	15	13	29	28
Not known	0	1	1	1	1	2
Total ..	95	81	91	99	186	180

* The onset date is defined as either the date of onset of the fatal illness (case children) or the date when the corresponding case developed its fatal illness (control children).

A similar arrangement by age at the final date (which was usually within a few weeks or months of the onset date) also picked out children who died between the second and fourth birthdays (51 cases, 24 controls), and again the difference was more marked for leukaemia than for other cancers. This is particularly interesting, since it is precisely this age group which has borne the brunt of the recent increase in childhood leukaemia mortality both in this country and in the U.S.A. (Hewitt, 1955).

Thus far the evidence in favour of a link between childhood malignancies and post-natal *x*-ray exposure has depended on case/control comparisons of arguable validity. We have therefore attempted to assess the evidence by means of comparisons within the case and the control series, using the following argument. In a "normal" group of children, such as our own controls, the percentage who have ever been *x*-rayed should steadily increase with age, for each year adds to the number of children who are *x*-rayed for the first time. For the sake of simplicity we shall assume that the percentage of "first" *x*-ray exposures is normally the same in each of the first ten years of life. In order to test this assumption we have calculated for the control children the "expected" percentages of children ever *x*-rayed in each of the five age groups shown in Table XVII. In the first two columns of Table XVIII these expected figures are shown in italics together with the actual percentages and numbers. It will be seen that the correspondence between the two sets of figures is not exact but

TABLE XVIII.—*Distribution (by Age at Onset Date) of Children With Post-natal X-ray Exposure Compared With the Distribution Expected on the Basis of a Regular Increase with Age of the Percentage of Children Ever X-rayed Since Birth*

Age of Children at Onset Date (Years)	Controls		All Cases		Leukaemia		Other Cancers	
	Actual / *Expected*		Actual / *Expected*		Actual / *Expected*		Actual / *Expected*	
	%	No.	%	No.	%	No.	%	No.
0, 1	4·9 *3·5*	17 *12·28*	5·8 *3·7*	20 *12·76*	4·4 *3·8*	6 *5·25*	6·7 *3·6*	14 *7·43*
2, 3	7·2 *10·6*	27 *40·03*	15·4 *11·0*	58 *41·61*	17·9 *11·4*	35 *22·37*	12·7 *10·7*	23 *19·30*
4, 5	19·7 *17·7*	57 *51·15*	15·2 *18·4*	44 *53·16*	16·1 *19·0*	23 *27·20*	14·4 *17·8*	21 *25·95*
6, 7	25·9 *24·8*	49 *46·83*	18·0 *25·8*	34 *48·67*	18·3 *26·6*	17 *24·77*	17·7 *24·9*	17 *23·89*
8, 9	32·2 *31·9*	28 *27·71*	33·3 *33·1*	29 *28·80*	31·1 *34·2*	14 *15·41*	35·7 *32·0*	15 *13·44*
Total	13·8 *13·8*	178 *178·00*	*14·4* *14·4*	185 *185·00*	15·4 *15·4*	95 *95·00*	13·4 *13·4*	90 *90·01*
$\chi^2_{(4)}$ Value of P	7·622 >0·10		19·414 <0·001		12·478 <0·02		11·015 <0·05	

lies well within the limits of chance fluctuation ($P>0.1$). It follows that the original assumption is a reasonable one. Now, if the post-natal *x*-ray exposure had *not* influenced the risk of the children dying later of malignant diseases, the correspondence between the actual and expected figures for the case series should be much the same as in the control series. But the discrepancies are much greater in the case series than in the control. Subdivision of the cases into leukaemias and other cancers shows that in the second age group of the leukaemia series (which includes children who developed leukaemia between their second and fourth birthdays) the proportion of "*x*-rayed children" is actually higher than the proportion in the third age group, although for these children the period during which they had an opportunity to be *x*-rayed was half as long again. For children with other cancers the largest discrepancy is in the first age group, where the actual number of children *x*-rayed (14) is nearly twice the expected number (7.43).

In earlier parts of this paper (and again in Section IV) we discuss evidence which indicates that the decisive factor in some of the cases, particularly in the leukaemia series, dates back to a period well before birth. If this is so then there must be, in the newborn population, a number of children in what may be loosely called a "pre-malignant state." The present analysis of post-natal *x*-ray exposures suggests that if such children are *x*-rayed during infancy overt signs of the disease may appear relatively quickly.

This inference can be drawn from the tables already given, but is perhaps better illustrated by the distribution of cases and controls according to the interval between the first post-natal *x*-ray exposure and the final date. For children *x*-rayed in the first three years of life this was :

Interval, First Exposure to the Final Date	Cases	Controls
Up to 11 months	12	11
12 to 47 ,, ...	62	26
Longer intervals	33	37

This interpretation would also explain the deficiency on the case side of *x*-rayed children who developed the fatal disease between their fourth and eighth birthdays (see Table XVII). For, if irradiation in infancy hastens overt signs of the disease, then there must be some "vulnerable" children who survive a comparatively long time partly because they have not been *x*-rayed. If this is so, we would expect the percentage of *x*-rayed children in the older half of the case series to be below the level for healthy children.

Pedoscopes

We must now reconsider the records for shoe-shop *x*-ray exposures. The same rays are emitted from pedoscope

machines as from diagnostic *x*-ray sets. If, therefore, the medical *x*-ray examinations have had an effect, one would expect the pedoscope records to show the same signs. A possible reason for there being no such excess has already been mentioned—namely, inclusion on the control side of children whose first pedoscopic examination was later than the onset date. Another reason should now be apparent. The data on medical *x*-ray examinations suggest a risk only when the first exposure takes place before the third birthday—that is to say, during a period when very few children will have had shoe-shop *x*-ray exposures. The relevant data cannot be analysed by age at first exposure, but if the cases with pedoscope exposures are examined in relation to the age at death some suggestive figures emerge. Only seven children dying before the age of 2 had been *x*-rayed in a shoe-shop, but six of them had leukaemia. The other pedoscope histories included 37 children who developed leukaemia between 2 and 4 years of age (18% of such cases), but only 17 (or 10%) of the children who developed other cancers between these ages. Thus there is no inconsistency between the figures for medical and shoe-shop *x*-ray exposures.

Comment

To sum up: there are two reasons for thinking that *x*-ray films taken shortly after birth influence the distribution of childhood deaths from malignant disease. In the first place, there is a significant case excess for *x*-ray exposures during infancy. Secondly, children who died of leukaemia at the age of 2 and 3 years were concentrated within the group of cases exposed to *x* rays in infancy. The effect of post-natal *x*-ray exposure appears to be more marked in respect of leukaemia than other cancers and to be much weaker than the effect of pre-natal *x*-ray exposure. It may in fact be restricted to speeding the date of death in children who are already predisposed to leukaemia.

The lack of any association between the parts of the body exposed to *x* rays and subsequent developments may be related to the fact that in infancy the reticulo-endothelial system is so widespread that it is likely to be involved in any exposure.

SECTION V. CHILDREN'S ILLNESSES AND THEIR TREATMENT

In this section we consider: (1) congenital defects, (2) acquired diseases during the pre-onset period, and (3) two classes of drugs—sulphonamides and antibiotics. Again we discriminate between accurately dated illnesses and ones which might have happened after the onset date.

Congenital Defects

These were mentioned in 75 of the case and 46 of the control records, but only two conditions, mongolism and naevi, were more common among the cases.

Mongols.—There were 17 mongols in the leukaemia series, one among the other malignant disease, and none in the control group, though one child placed first on a control selection list was said to be a mongol and to have been in an institution since infancy, and was for this reason not chosen (see Section I). Until two years ago only four cases of leukaemia associated with mongolism had been reported. Since then Krivit and Good (1956), Merrit and Harris (1956), Carter (1956), and Paterson (1958), have between them reported 13 cases. With the present series this makes a total of 34 reported cases. Since the incidence of mongolism in the present leukaemia series (2.6%) is nearly 20 times as high as the incidence of mongolism in 14,000 consecutive births (Malpas, 1937), the association is evidently not a fortuitous one. We have already shown that excessive maternal age at the time of the child's birth (which undoubtedly predisposes to mongolism) also predisposes to leukaemia (see Section I). It is therefore more likely that the two diseases are influenced by a common factor than that the antecedent condition, mongolism, predisposes to the later condition, leukaemia. If this is so then for some of the cases of leukaemia the decisive event must date back at least to the onset of the mongolism; that is to say, at the latest to the second month of gestation. A number of facts relating to the mongols in the survey are shown in Table XIX. The mongol who developed a glioma had been *x*-rayed 71 days before birth, but none of the mongols who died of leukaemia had been irradiated *in utero*. Four mothers had had previous abortions, but

TABLE XIX.—*Features of the* 18 *Mongols Included in the Survey*

Cause of Death	Cell Type (Leukaemias)	Sex	Birth Rank	Age at Death	Age of Mother*
Cerebral glioma	—	M	4	3 years 5 months	40
Leukaemia	Myeloid	M	7	2 ,, 11 ,,	44
,,	,,	M	2	1 year 2 ,,	44
,,	Lymphatic	F	8	1 ,, 6 ,,	43
,,	,,	F	4	5 years 0 ,,	42
,,	Myeloid	M	5	2 ,, 1 ,,	40
,,	N/R	F	2	2 ,, 8 ,,	39
,,	Lymphatic	M	3	1 year 11 ,,	38
,,	,,	M	2	4 years 9 ,,	36
,,	Monocytic	M	2	5 ,, 11 ,,	34
,,	Lymphatic	F	1	2 ,, 0 ,,	34
,,	Myeloid	F	4	2 ,, 6 ,,	34
,,	Lymphatic	F	1	2 ,, 0 ,,	32
,,	,,	F	2	1 year 5 ,,	32
,,	Aleukaemic	F	3	8 years 4 ,,	29
,,	Lymphatic	M	1	6 ,, 5 ,,	26
,,	Monocytic	M	1	4 ,, 6 ,,	26
,,	Lymphatic	M	1	6 ,, 11 ,,	19

* Age at birth of survey child.

there were no threatened abortions in the relevant pregnancy and no previous stillbirths. One mother was married to a second cousin.

Naevi.—These were reported 34 times on the case side and 21 times on the control side. As previously stated, eight of the cases and three of the controls were treated with *x* rays. The overall difference between cases and controls is not statistically significant, and might be due to under-reporting on the control side of a condition which is only cosmetically important. For girls there were 18 cases and 14 controls, and for boys 16 cases and 7 controls.

The other congenital defects took various forms and were evenly distributed between the two series, with 25 records on the case side and 25 on the control side.

Other Diseases of Childhood

It was clearly important to exclude all illnesses which happened after the onset date. This raised two problems: what to do with an illness which appeared to coincide with the onset of the fatal disease; and what to do with an inaccurately dated event which might or might not have preceded this date. An illustration of the first problem is provided by a case record which stated: "Following measles the child was always ailing. The measles was complicated by pneumonia then . . ." In this case we should have placed the onset date immediately after the measles and excluded all subsequent events, even the so-called pneumonia.

The second problem arose when an illness was not more precisely dated than the year in which it happened or the age, in years, of the child at the time. In such cases we have assumed that the event occurred midway between the limiting dates. It follows that an illness dated, say, "1953" would have been counted in the pre-onset period if the onset date was July 1 or later, but not otherwise. Similarly an illness occurring "at 4 years of age" would be counted in the pre-onset period if the onset date was 4 years and 6 months or over, but not otherwise.

It follows that, though some illness must have been wrongly placed, the errors in placing are likely to have cancelled out, leaving an approximately correct total in the two series; where there was possible confusion between a previous illness and an early but unrecognized manifestation of the fatal disease, the illness has not been included in the following analyses.

In Table XX the illnesses allocated to the pre-onset period have been classified as *recent* if within two years of the onset date, and *remote* if two or more years before this date. Events which could not be placed in one or other of these subgroups have been relegated to a separate column and are not included in the total of dated illnesses.

The table deals with illnesses, not injuries, and is restricted to relatively acute episodes which could be more or less precisely dated. A number of chronic diseases, injuries, operations, and ill-defined conditions not included in the table are mentioned in the text.

The first part of Table XX deals with the six infectious diseases which comprise nearly 80% of all dated illnesses and of all illnesses in the *recent* period. The exclusion of

TABLE XX.—*Childhood Illnesses in* 1,299 *Pairs of Survey* (*M*) *and Control* (*C*) *Children*

	Illness Within 2 Years of Onset Date		Illness More than 2 Years Before Onset Date		Total of Dated Illnesses		Additional Illnesses of Uncertain Date	
	M	C	M	C	M	C	M	C
Measles	208	235	236	219	444	454	60	70
Chicken-pox ..	131	173	125	102	256	275	46	55
Whooping-cough..	105	136	154	158	259	294	48	36
Mumps	57	61	45	38	102	99	10	22
Rubella	47	46	34	34	81	80	12	23
Scarlet fever ..	20	24	19	15	39	39	4	6
Total	568	675	613	566	1,181	1,241	180	212
Bronchitis.. ..	38	24	19	19	57	43	6	3
Bronchopneumonia	39	17	37	32	76	49	2	1
Total	77	41	56	51	133	92	8	4
Acute tonsillitis ..	30	22	14	12	44	34	1	4
Acute otitis media	20	25	15	9	35	34	0	2
Total	50	47	29	21	79	68	1	6
Other infections ..	88	54	65	47	153	101	12	9
Other illnesses ..	29	12	29	12	58	24	1	3
Total	117	66	94	59	211	125	13	12
Grand total ..	812	829	792	697	1,604	1,526	202	234

M=Survey children (all malignant diseases). C=Controls.

cases of uncertain date has still left an excess on the control side, but this is slight and confined to three diseases —measles, chicken-pox, and whooping-cough. As this comparison might have been affected by the deficit of first-born children in the control group, a rough standardization for birth rank was made. This had very little effect, particularly on the comparison between illnesses which happened within two years of the onset date, where the difference between cases and controls was mainly concentrated. Hence the popular idea that an infectious illness occasionally initiates a malignant process (or provokes a pre-malignant state) receives no support from the present survey. To argue the other way and say that the survey findings suggest an antagonism between exanthema and malignant diseases would also be wrong. Only in the *recent* period and for one disease (chicken-pox) was there a significant excess of controls, and for this disease there was a deficiency of

82 controls), and frequent colds (already mentioned). The even distribution of cases and controls for three of these four conditions does not suggest under-reporting on the control side ; it is therefore possible that the figures in the fourth section of Table XX and the figures for recurrent attacks of bronchitis indicate a genuine excess on the case side.

Finally, some reference must be made to injuries and operations. A vast number of minor injuries, including bruises, were recorded, but only two types were regarded as important enough to merit analysis, and then only if they happened within two years of the onset date. In this period there were 44 fractures (26 cases, 18 controls) and 28 burns or scalds (16 cases, 12 controls). The case excess lay entirely within the leukaemia series, and for the two items combined these children produced 28 records, compared with 14 for the children with other cancers. For recent operations, other than tonsillectomy, the figures were 16 cases and 17 controls.

In summary: Neither in their lifetime as a whole, nor in the two years immediately preceding the fatal illness, did the children with leukaemia and other cancers have an excessive number of the common infectious diseases of childhood. If anything they experienced less chicken-pox and fewer throat and ear infections than usual. On the other hand, the children with leukaemia had a noticeable heavy incidence of acute pulmonary diseases and severe injuries during the two years before they showed signs of the fatal disease.

Sulphonamides and Antibiotics

There were three reasons why the interviewing doctors were specifically asked to record treatments with sulphonamides and antibiotics. In the first place, being new drugs, they might have contributed to the recent increase in leukaemia mortality. Secondly, intensive treatment with sulphonamides occasionally causes aplastic anaemia. Finally, it was thought that the mother would know, if not what exactly was given to the child, at least whether sulpha drugs or penicillin had been prescribed, and how these drugs had been administered.

In the following analyses we have omitted all local applications (drops, powders, and ointments) and made no distinction between oral and parenteral administrations. The conventions which were used to decide whether the drugs were actually given in the pre-onset period were the same as for the illnesses but, as Table XXI shows, there was a higher proportion of undated drugs than illnesses. This is due to the fact that in some of the records it was impossible to say which of the dated illnesses had been treated in this way. The table reveals an even distribution of adequately dated treatments with sulphonamides, but the corresponding

control records in the *remote* period, which reduces the overall excess to a non-significant level.

The second part of Table XX shows that acute pulmonary infections were relatively uncommon, but for every three cases with such infections there were only two controls. In the *recent* period there were nearly twice as many records of acute bronchitis or pneumonia on the case side, and in the leukaemia series (not distinguished in the table) the case/control contrast was greater than in the other malignant series. Compared with all controls, the excess in the leukaemia series was highly significant ($P<0.001$) and indicates in this series an " extra " 24 cases of acute bronchitis or pneumonia in the two years before the onset date. If this is accepted as evidence of a causal relationship, then about 4% of leukaemia cases might be ascribed to a recent acute pulmonary infection. The corresponding figure (less than 2%) for other cancers was of borderline significance. Since there is no excess on the case side of acute pulmonary infections in the *remote* period, the recent infections are more likely to have accelerated overt signs of malignant diseases in vulnerable children than to have initiated the malignant process.

These estimates all relate to single acute episodes. A much smaller number of children were said to have recurrent attacks of bronchitis (often associated with teething), and these were also in the ratio of approximately three cases to two controls. Such reports are necessarily suspect and difficult to date, so the excess is less certainly a genuine finding. Nevertheless for three infections of the respiratory tract, which were suspect in the same way, there was an excess on the control side: these were " frequent colds " (242/295), " recurrent sore throat " (27/39), and " sinusitis " (10/21).

Clear-cut acute attacks of tonsillitis and otitis media are shown in the third part of Table XX. The slight excess of tonsillitis on the case side is confined to the *recent* period, and the figures for otitis media are roughly equal on the two sides. Not mentioned in the table are 35 case children and 29 controls who had their tonsils removed within two years of the onset date, also 10 cases and 19 controls with chronically discharging ears.

The fourth part of Table XX includes a number of illnesses which were either too rare or too vaguely described to be considered separately. In both periods there was a marked excess on the case side, but since the risk of " inflated reporting " by bereaved mothers is probably at a maximum with minor or ill-defined conditions (which form the bulk of this section), we prefer to draw no conclusions from these figures. Trivial conditions not included in the table are " feeding difficulties " (254 cases, 198 controls), allergic conditions (28 cases, 25 controls), worm infections (89 cases,

sidered here. There may, however, be an indirect relationship between antibiotics and leukaemia. In recent years antibiotics have revolutionized the prognosis for acute pulmonary infections and have undoubtedly kept alive children who would otherwise have died. If these infections occasionally provoke a latent leukaemic process, then, by increasing the number of children who survive, antibiotics may indirectly increase the prevalence of leukaemia.

SECTION VI. FEEDING HABITS AND HOME BACKGROUND OF THE SURVEY CHILD

Concern is often expressed about the widespread use of new chemicals, particularly food preservatives and detergents, on the ground that these may be carcinogenic. In the present survey no attempt was made to collect information about exposure to specific chemicals, but the mothers were asked a number of questions about their own habits and what they allowed their children to eat and drink.

Feeding Habits

The survey doctors were told to ask whether the children had been given certain foods and drinks every day, less than daily but at least one a week, less often, or never. They were also asked to state when these foodstuffs were first given; and what the mother thought the child had consumed in the way of coloured sweets. The completed records showed that a few control mothers had reported foods which were so unlikely to have been given during the pre-onset period—for example, fish and chips before the age of 1—that the analysis has been restricted to case/control pairs for whom the age at the onset date was not less than an arbitrarily chosen "qualifying age" for each foodstuff (see Table XXIII).

TABLE XXIII.—*Feeding Habits. Percentage of Cases and Controls Who Had Ever Taken Certain Foodstuffs*

Item	Qualifying Age	% of Children Who Had Ever Taken This Item		$\chi^2_{(1)}$	P Approx.
		Cases	Controls		
Dried milk	3 days	69·6	67·1	1·718	0·19
Fruit juice	1 month	90·6	90·7	0·002	0·96
Tinned sieved vegetables	4 months	61·1	58·2	1·976	0·16
Other tinned vegetables	12 ,,	55·1	60·6	6·797	0·009
Highly coloured cakes		33·5	33·7	<0·001	>0·98
Highly coloured fruit drinks		47·4	52·6	5·729	0·017
Coloured sweets *		32·6	33·0	0·018	0·90
Shop-fried fish and chips		43·2	43·9	0·066	0·79
Total				16·306	0·05>P>0·02

* For this item "ever" = more than ¼ lb. (113 g.) per week.

TABLE XXI.—*Treatment of Children's Illnesses with Sulphonamides and Antibiotics*

Treatments	Leukaemia (619 Pairs)		Other Cancers (680 Pairs)		Total (1,299 Pairs)		Case/Control Ratios
	Cases	Controls	Cases	Controls	Cases	Controls	
Sulphonamides:							
Adequately dated ..	94	96	84	61	178	157	1·13
Not adequately dated	30	18	26	34	56	52	1·08
Total ..	124	114	110	95	234	209	1·12
Antibiotics:							
Adequately dated ..	109	83	95	77	204	160	1·28
Not adequately dated	36	36	29	41	65	77	0·84
Total ..	145	119	124	118	269	237	1·14

TABLE XXII.—*Sulphonamide and Antibiotic Treatments, Distinguishing Children Who Had an Acute Pulmonary Infection Within Two Years of the Onset Date*

	All Cases		All Controls		Ratio*
	No.	%	No.	%	
Sulphonamide:					
Children who had acute pulmonary infection within 2 years of onset date ..	22/77	28·57	10/41	24·39	1·17
Remainder	156/1,222	12·77	147/1,258	11·69	1·09
Antibiotics:					
Children who had acute pulmonary infection within 2 years of onset date ..	36/77	46·75	13/41	31·71	1·48
Remainder	168/1,222	13·75	147/1,258	11·69	1·18

* Case/Control incidence ratio.

figures for antibiotics show a small case excess. Since the high incidence of pulmonary infections on the case side was likely to have influenced these figures, Table XXII has been prepared in which the children who had such an illness in the two years before the onset date are shown separately. The effect of doing this is to reduce the case/control ratio for antibiotics from 1.28 (all treatments) to 1.18 (treatments other than those probably but not certainly related to a recent pulmonary infection). At this level it is within the conventional limits of chance fluctuation (P=approximately 1 in 15). The relatively high case/control ratio for both types of drugs in this table probably reflects the fact (not yet mentioned) that in the case series these infections were usually more severe, as well as more numerous, than in the control series, and for this reason were more likely to receive antibiotics or sulphonamides.

A record was also kept of treatments with ultraviolet light. These were reported for 50 cases and 40 controls. No conclusions can be drawn from these small numbers.

We conclude that there is no evidence for a direct relationship between childhood malignancies and the drugs con-

The figures shown in this table relate to children who had ever been given a food regardless of the reported frequency. In aggregate the case/control differences for the eight foodstuffs considered appear to be statistically significant ($P<0.05$), but only two foods—tinned vegetables and coloured drinks—showed a significant difference, and both were reported more often on the control than on the case side. For these two items the qualifying age may have been set too low, thus permitting some upward bias on the control side. If such bias exists the case/control ratios for other items may have been underestimated, but the reported percentages are so nearly level that it is unlikely that any important differences have been missed.

The foodstuffs listed on Table XXIII were also considered from the point of view of "dosage." Three of them showed a case excess in the highest consumption category:

Coloured Cakes.—Daily consumption of these was reported for 45 cases and 16 controls, but when this and the next-highest "dose" were combined the figures became 125 cases (11.3%) and 111 controls (10.0%).

Coloured Sweets.—At an estimated level of 12 oz. (340 g.) or more sweets per week there appeared to be an excess of cases (7.0%) over controls (4.4%), but this was balanced by an excess on the control side of children who usually ate between 3 and 12 oz. (85 and 340 g.) per week. As already shown, there was no deficiency of cases in the lowest consumption group.

Shop-fried Foods.—There was no difference in the proportion of cases and controls who had never had these foods, but consumption of fish and chips as often as once a week was reported by 191 cases and 140 controls. Once again the balance was made up by an excess of control children among those who occasionally had fish and chips.

In short, for none of the eight foodstuffs considered was there either a significant excess in the percentage of case children who had ever consumed them, or a consistent association between the amount consumed and subsequent events.

Other Factors

Contraception.—An attempt was made to discover whether there had been any attempt at family planning and if so what methods had been used. The proportion of completed records for contraception (89%) was lower than for most items, but the numbers of case and control mothers who gave satisfactory answers were similar (1,158 and 1,143). Chemical contraceptives were used by 124 case mothers (10.7% of those giving full information) and 107 control mothers (9.4%). The small difference arose principally in the leukaemia series, but these mothers did not differ sig-

nificantly either from their own control pairs (P=approximately 0.21) or from the mothers of other cases (P= approximately 0.37).

Detergents.—The percentage of case mothers who stated that they used synthetic detergents was 61.3%—very similar to, but slightly below, the figure of 63.6% for mothers of controls.

Smoking.—Under this heading interviewers were asked to place the mother and father in one of the following categories: heavy, moderate, or light smokers, and non-smokers (the instructions gave quantitative definitions for these four categories). The figures quoted here relate only to "smokers" (at least one cigarette or pipe a day) and "non-smokers." The percentages of fathers and mothers in both series recorded as smokers were slightly higher than those recently reported for men and women of comparable age by Research Services Ltd. (Todd, 1957). For fathers of cases the proportion was 82.9% and for fathers of controls 80.9%, and for mothers 47.8% and 43.8% respectively. These percentages indicate a significant ($P \simeq 0.04$) excess among mothers of cases, but the difference is a small one (case/control ratio of 1.09), and we have not attempted to relate it to age, income, or number of children, as would be necessary before concluding that the mothers' smoking habits had affected their children. One factor which might well produce a genuine but irrelevant difference between mothers of cases and controls was, of course, the bereavement itself.

SECTION VII. FAMILY HISTORIES

Sibs—The records gave a complete tally of all the mothers' pregnancies up to the date of the interview, including those which ended with an abortion or stillbirth. As Table XXIV shows, there was no case/control difference in respect of stillbirths, but the case excess for abortions is at least suggestive, particularly as it is concentrated (like the case excess for threatened abortions during the "relevant" pregnancy) in the leukaemia series. The numbers of deaths among the liveborn sibs of the cases and controls were 132 and 129 respectively. This implies a very similar total

TABLE XXIV.—*Miscarriages and Stillbirths (Up to the Date of the Survey Child's Death)*

	Leukaemia		Other Cancers		Total	
	Cases	Controls	Cases	Controls	Cases	Controls
Mothers reporting abortions ..	129	93	119	118	248	211
Total No. of abortions ..	173	122	153	156	326	278
Mothers reporting stillbirths ..	26	23	27	23	53	46
Total No. of stillbirths.. ..	26	28	32	25	58	53

mortality, since the numbers of liveborn sibs in the two series were almost equal (2,119 and 2,155) and the average number of years at risk was only slightly greater on the control side. Although most individual causes of death were equally common in the two series, deaths from malignant disease among the case sibs numbered eight as against only two for the controls. On the basis of the relatively high national mortality rates in 1953–5, and without allowing for reduction of the population at risk by other deaths, the expected number of deaths from malignant disease among the sibs of the cases is 2.15. It follows that the total of eight malignant deaths on the case side is significantly higher than expectation (P=0.001) (see Table XXV). Even five cancer

TABLE XXV.—*Siblings of Cases With Death Attributed to Malignant Disease*

Serial No.	Index Case			Sibling		
	Sex	Diagnosis	Age at Death (Years)	Sex	Diagnosis	Age at Death (Years)
122	F	Lymphosarcoma ..	6	M	" Acute anaemia "	1
289	F	Acute lymphatic leukaemia	1	F	Leukaemia ..	1
290	F	Acute reticular leukaemia	0	F	" ..	0
347	M	Acute lymphatic leukaemia	5	M	Generalized lymphosarcoma	3
				M	Retroperitoneal sarcoma	2
476	F	" " "	5	M	" Brain tumour "	4
1293	F	Acute stem-cell leukaemia	1	F	" Growth in abdomen "	2
1656	M	Neuroblastoma ..	1	M	Lymphatic leukaemia	5

deaths—that is, after the exclusion of three cases which were not certainly cancers—namely, the " acute anaemia," the " brain tumour," and the " growth in abdomen "—represent a significant case excess (P<0.05).

Parents.—One reason for not obtaining the first name on a control selection list was death of the mother; therefore little meaning attaches to the excess of dead parents in the case series (20) as compared with the controls (5). Among nine dead parents in the leukaemia series two had died of leukaemia and one of lymphosarcoma. Among 11 dead parents in the other cancer series four had died of some form of malignant disease. One of the five deaths among parents of control children was attributed to leukaemia and two to other malignant diseases.

Grandparents.—A total of 4,508 deaths of grandparents were reported. Judged by the proportions of deaths for which the cause was unknown or attributed to " senility," mothers gave better information about their own than about their husbands' parents, but there was no difference between the standard of information supplied by case and control

mothers. Overall mortality was almost identical in the two series, the number of surviving grandparents being 2,936 in the case and 2,948 on the control side. Among 53 specified causes of death the largest proportional discrepancies between cases and controls were war injuries (60/95) and renal conditions (32/54), the excess in each case being on the control side. For malignant diseases as a whole there was a proportionally smaller but significant excess on the case side, the totals being 387 for cases and 316 for controls. This excess ran uniformly through the records (Table XXVI). The proportion of case children with an affected grandparent varied very little between the main diagnoses

TABLE XXVI.—*Reports of Malignant Disease in Grandparents of Cases and Controls*

	Cases	Controls	Ratio
Grandparents of leukaemia series	172	140	1·23
,, of other malignant series ..	215	176	1·22
All paternal grandparents	186	152	1·22
All maternal ,, ...	201	164	1·23

in the case series. There was no detectable variation in the sex ratio of cases as between groups with an affected grandparent on the paternal or maternal side, nor when these relatives were subdivided into father's father, father's mother, etc. One tabulation which did appear to show some variation was that of the reported site of the cancer in the grandparents. For example, lung cancers in grandparents showed *no* excess of cases over controls (52/55), but a relatively high case/control ratio was observed for breast and genital cancers (cases 58, controls 37).

Uncles and Aunts.—The total number of uncles and aunts was much the same for cases and controls (9,578 for cases, 9,425 for controls). Only deaths attributed to malignant disease were coded for these relatives, the numbers being: uncles and aunts of cases, 77; of controls, 55. The case/control ratio of 1.20 is almost identical with that for grandparents. In the case series there was no significant variation between the diagnostic subgroups, but the cancers in the uncles and aunts seemed to parallel two features noted in the grandparents. Thus cancers of the respiratory system showed no case excess (8/13), while breast and genital cancers showed a relatively large case excess (18/8).

Remoter Relatives.—Rather more case mothers volunteered statements about cancer in relatives beyond the second degree than control mothers, and this revealed one interesting cancer pedigree. This concerned a boy who died at the age of 6 from "cancer of the liver and bowel" and had five relatives on the father's side who had also died of bowel cancer—the father himself, the grandfather, the great-grandfather, and two aunts.

Consanguinity.—The number of cousin marriages in the case series was not large either in relation to the controls or in relation to the frequency in the general hospital population (Bell, 1940). Of the 10 consanguineous matings reported in the case series, only 5 (0.4%) were between first cousins. To these should be added one case (not fully followed up and not included in the 1,299 pairs) in which a father–daughter mating resulted in the birth of a girl who died at the age of 5 months from Letterer–Siwe's disease.

Discussion

All scientific investigations are faced with the problem of eliminating unknown bias from test and control observations. Where, as in the present case, the findings are based on human data and involve comparisons between facts which have been obtained by different observers from inexpert witnesses the main sources of such bias are: (1) unequal recording of events by different observers; (2) unequal reporting of events by the individuals chosen to represent case and control groups; and (3) faulty selection of cases and/or controls.

The first source of error has been controlled by insisting on the same doctor seeing each member of a given case/control pair. By so doing, errors due to lack of skill in interviewing and recording should be equally represented on the case and control sides. The same device might be expected to control the second source of error; but here we were on less certain ground, and have therefore applied other tests of reliability. Thus for several items specified in the questionaries the proportions of completed records in the two samples have been calculated and found to be alike. To illustrate this point we have summarized the findings for six items in which a low response rate might, with some justification, have been expected (Table XXVII). By these criteria it would seem that the doctors were equally pertinacious when questioning the cases and controls, and the mothers were equally helpful when replying to the doctors' questions.

TABLE XXVII.—*Response of Case and Control Mothers to Certain Questions*

Items	Response Rate (i.e., Percentage of Complete Answers)	
	Cases	Controls
Family income	87	88
Contraception	88	89
Dated illnesses (children)	89	87
,, (antibiotics and sulpha drugs) (children) ..	76	71
,, post-natal *x*-ray exposure	85	78
Specific cause of death (grandparents)	82	81

Other safeguards include repeated demonstration that significant differences between cases and controls are not compatible with any *general* tendency on the part of control mothers to be unduly forgetful or uncooperative. For instance, the case excess for two maternal diseases which might have affected foetal development—virus infections, and threatened abortions in the relevant pregnancy—were not accompanied by a case excess for other maternal illnesses, either during this period or previously. Again, the case excess for pulmonary diseases in childhood must be viewed against a background of almost equal numbers of all childhood diseases in the two groups, and the case excess for *x*-ray exposure in infancy against almost equal numbers for post-natal *x*-ray exposure as a whole.

The third source of bias—namely, faulty selection of cases and controls—has been dealt with in the following ways. The cases were drawn from the total number of cases for a three-year period, and those actually included in the survey represent such a high proportion of this total that, however atypical the *lost cases* may be, the consequences would remain numerically unimportant. The controls were drawn from the general population of surviving children, thus avoiding some of the confusion which might have resulted from using more accessible children—for example, other hospital patients, children attending welfare clinics, or siblings of cases. The controls actually included in the survey have been shown to differ in two important respects (first-born children and twins) from the national population. But because the numbers in these classes can be calculated from official vital statistics, it has been possible to show that the deficiencies in the control group had a negligible effect on the main finding—namely, a large excess for direct foetal irradiation in the case series. We have also shown that this finding was not affected either by systematic checking of the mother's statements against hospital records, or by the publication of a preliminary report half-way through the survey. Finally, we already know that at least one independent observer has tested this main finding and used different methods to obtain virtually the same result.

We are therefore confident that the finding in respect of direct foetal irradiation is not an artifact. Nor have we any reason to doubt that there are genuine case/control differences in other respects—namely, maternal age at the time of conception, post-natal pulmonary infections, and *x*-ray exposure in infancy. All these factors appear to have a " causal " association with childhood malignancies, but we do not suggest that any one of them is either a necessary or a sufficient cause of the malignant changes. The special risks associated with these events may perhaps be compared with the well-known risk of being a male child. Thus boys are known to run a higher risk than girls of developing cancers, but no one ever cites the Y-chromosome as a

"cause" of cancer. Nor do we claim that any of the associations are quantitatively important. The number of childhood malignancies which they might account for is small even in relation to the recent increase of these deaths reported during the last two decades—about 60% for children under 5 years (see McKenzie, Case, and Pearson, 1957).

Summary and Conclusions

The pre-natal and post-natal experiences of a large group of children who recently died of malignant diseases have been compared, point by point, with the experiences of a similar group of live children.

The frequency of three pre-natal events—namely, direct foetal irradiation, virus infections and threatened abortion—was significantly higher among the dead children than among the live children.

One other pre-natal influence—namely, excessive maternal age—appears to increase the risk of leukaemia in childhood and to be related to the fact that this disease and mongolism tend to occur together.

The frequency of three post-natal events—namely, *x*-ray exposures in infancy, acute pulmonary infections and severe injuries—was significantly higher for children who subsequently died of leukaemia than for other children. In the "pre-antibiotic era" some of these children might have died before showing signs of the leukaemia.

The health of the mothers and the home background of the children were not significantly different in the two groups, but there were minor points of difference in the family histories of cancer and leukaemia.

Our final conclusions are that foetal irradiation does not account for the recent increase in childhood malignancies, but the finding of a case excess for this event does underline the need to use minimum doses for essential medical *x*-ray examinations and treatments.

A survey on the scale achieved could never have been contemplated without the active co-operation of doctors and health visitors too numerous to mention by name. Principal medical officers of health of local authority areas assumed responsibility for the field work of the investigation and completed the whole of their arduous and self-imposed task in the short space of 18 months. The interviews were done either by principal or by assistant medical officers of health, and health visitors did invaluable work in tracing cases and controls. We record with gratitude the high standard of the work in all regions.

We are also indebted to the Lady Tata Memorial Trust, which defrayed all costs other than those borne by the Health Departments and Oxford University; to the General Register Office, which provided essential data; to the Medical Research Council Working Party on Leukaemia, who gave us constant encouragement and advice; and to the mothers of the dead children, who had the courage to reopen a painful topic and so often expressed the hope that by doing so they might be helping other children.

Finally, we thank two members of our own staff: Miss Dawn Giles, who, while holding the Mary Goodger Research Scholarship, helped with the organization of the survey and the coding of records, and W. E. C. Brooksbank, who was responsible for the machine sorting of the data.

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THE DANGERS FROM FALLOUT OF RADIOACTIVE STRONTIUM AFTER ATOMIC BOMB EXPLOSIONS

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Abstract

Recently there has been presented, particularly from American sources, a considerable amount of information for evaluation of the dangers from fallout of radioactive strontium after atomic bomb explosions. Some of the conclusions which have been drawn rest on a questionable statistical foundation. Also, there is reason to believe that more radioactive strontium will be taken up in the human organism than was believed previously. If one takes these things into account, one arrives at the conclusion that continued atomic bomb tests probably will endanger the health of considerable population groups.

It is natural that many people are concerned about the possible dangers to human health resulting from atomic bomb explosions, both those tests which have already occurred and those which will take place in the

future. Therefore it is of the greatest importance to obtain observations which can be used to evaluate the danger. The material presented by the American scientist and member of the Atomic Energy Commission, Willard F Libby, at the meeting of the American Physical Society of April 26 of this year is here of particular importance.[4] The results he presented and the conclusions he drew from them have been discussed in the Norwegian press and a copy of the lecture has been distributed in Norway.

There is no reason to question the reliability of the observational material presented by Libby and his co-workers. The conclusions which are drawn from the observations are often difficult to evaluate. However, it is the duty and the right of each scientist to make up his own mind on the basis of the available data. For continued discussions it will be an advantage also if the conclusions are based on statistical methods which reduce the range for subjective estimates.

At the present time it seems that the most dangerous aspect of the atomic bomb explosions consists of the formation of a radioactive strontium isotope, strontium 90. This has a half-life of 28 years, and it has a tendency to be accumulated in the human bone structure. As a suitable unit for the radioactive strontium concentration in the bone structure one may use a micro-microcurie per gram calcium or, as it is called, one strontium

unit*. American health authorities figure that the part

*Sometimes, and somewhat unfortunately, also called a sunshine unit.

of the population which in its work is associated with radioactivity ought not to have more than 1000 strontium units in the body. The rest of the population ought not to have more than 100 strontium units. There is reason to be worried if large groups of the population approach this latter value. As the given tolerance limits are considered to be fairly uncertain, it is best to stay well below them.

When one wants to estimate the danger presented by strontium fallout after atomic bomb explosions, several problems must be considered. The most important are:

1. How much radioactive material will be formed by the atomic bomb explosions? How much will, as time goes by, accumulate in the soil, and what is the geographical distribution of the fallout?

2. How is strontium absorbed into the human organism, and what is the connection between radioactive fallout and the radioactive strontium in the human body?

3. How does the radioactive strontium concentration vary from one individual to another? It is quite possible that even if the population as a whole has a

concentration which represents no danger, a fairly large number of individuals may nevertheless have concentrations in the dangerous range.

Among these questions we have the best basis to answer the last. The most important data have been gathered by Libby, who has measured the radioactive strontium concentration in 53 stillborn children in Chicago in 1953 (see Figure 1). For comparison we have a study of the variation in normal strontium in human bones determined by Turekian and Kulp[8] on the basis of 277 pieces of bone from a large part of the world. Then we have a study of the radium concentration in 50 bodies in northwest America, gathered by Palmer and Queen[5]. Radium resembles strontium chemically, it is also radioactive, and it is enriched in the bone structure. Finally, we have some general information regarding the distribution of the elements in nature.

Libby assumes that the distribution function is normal, i.e. that it follows a Gaussian distribution function. The mean of radioactive strontium in the stillborn children in Chicago is .14 strontium units with a mean deviation of 0.05⅘. The relative deviation (mean deviation divided by the mean) is then 39%. The mean radium content, according to Palmer and Queen, was $1.6 \cdot 10^{-14}$ with a mean deviation of $0.69 \cdot 10^{-14}$, and a relative mean deviation of 44%. The mean of normal

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strontium, according to Turekian and Kulp, is $0.59 \cdot 10^{-3}$ with a mean deviation of $0.263 \cdot 10^{-3}$, and a relative mean deviation of 45%. The material for determination of normal strontium was gathered from a very large area. If one takes the relative deviation within somewhat smaller areas, it was down to 30%. Libby then assumes that a relative deviation of 1/3 is a normal occurrence in material of this type.

If the radioactive strontium concentration in the soil increases, the amount contained in the bones will increase also, and probably in proportion to the amount in the soil. The relative deviation will be unchanged, and Libby then calculates that about one out of 700 will have the double radioactive strontium concentration compared to the mean and only one out of several million three times the normal concentration.

Here Libby has assumed that the distribution is Gaussian. However, there are strong reasons to believe that the Gaussian distribution function cannot be used in the present case, and that the choice of distribution function will be of crucial importance for the conclusions which are drawn from the data.

From the Gaussian distribution function one would deduce that there should also be negative concentrations of the considered substances. This is, of course, impossible. If a quantity cannot assume negative values and if the relative deviation is large, the quantity in

question cannot follow a Gaussian distribution function.

The most comprehensive data apply to the normal concentration of strontium in the bones. We see (Figure 1) that there are several high values which do not agree with the normal distribution function.

Ahrens[1] has studied the following problem. If we take a number of samples of a mineral or a rock and determine the content of a trace element, what kind of a distribution function will we find? He found on the basis of a large amount of data that if one plots the observations on a linear scale the distribution is askew. However, one gets a normal distribution function if the observations are plotted on a logarithmic scale. This distribution function, which is known as the log normal distribution, therefore is the one which is most suited for geochemical problems. As we are here confronted with geochemical problems, there is reason to use the distribution function proposed by Ahrens. It is free from the objections which have been raised above against the use of the normal distribution function. But one must, of course, check the presented material to see if the log normal distribution function agrees better with the data than the normal function.

In the table the observed distribution of normal strontium is compared with the expected value in the normal and the log normal distribution function. The

agreement may now be tested with standard statistical methods (the chi-square method). It turns out that there is much less than 1 per mille probability that the distribution is normal. There is, on the other hand, good agreement between the observations and the log normal distribution function.

There is available commercially a graph paper which gives a log normal distribution as a straight line. Along the abscissa the values are given in logarithmic scale. The ordinate gives the per cent of the observations which will fall below the value which is indicated by the abscissa. In Figure 2 the data on radioactive strontium in new-born children in Chicago (Libby), radium in human bones (Palmer and Queen), normal strontium in human bodies (Turekian and Kulp) and the content of radioactive strontium in bones from adults (Kulp, Eckelmann and Schulert) are plotted in this way. One will see that the observations fall on straight lines, and actually the logarithmic scale was used by Kulp, Eckelmann and Schulert.

The value which is found where the curve crosses the 50% line is the one which will occur most frequently in a certain collection of observational material. This quantity is called the type number (t). The slope of the curve gives the mean deviation in the observational material. The logarithmic mean deviation is given by

the letter λ . If λ is small the type number will not be very different from the arithmetic mean, but the difference increases with λ .

We now see that the curves for radioactive strontium in new-born children, the radium content in the bodies, and the content of normal strontium in bones have very nearly the same slope. On the other hand, the curve for radioactive strontium in the bones of adults show a large mean deviation. Here the material is gathered from a very large area, and the pieces of bone which were available undoubtedly in part contained bone substance which was formed prior to the atomic bomb tests and in part newly formed bone substance.

If we assume that the radioactive strontium concentration in the bones increases in proportion to the amount of fallout in the district, the logarithmic deviation will not depend on the actual concentration in the soil. With a logarithmic mean deviation of 0.181 we find that approximately 5% of the population will have the double concentration compared to the average, 0.4% will have three times the average, and 0.05% will have four times this concentration. One will see that the figures are very different from those given by Libby.

The next step is to investigate how the radioactive fallout is distributed geographically. The fallout takes two different paths. By big bombs the radioactive

products are lifted into the stratosphere. Now there is little exchange between the stratosphere and the troposphere (the lower part of the atmosphere). Libby estimates that atomic waste on an average stays ten years in the stratosphere before it comes down into the troposphere, from which it will quickly be washed down into the soil by rain. In the course of these ten years the radioactive products will be spread over the whole earth. By smaller bombs the radioactive products remain in the troposphere and will come down fairly quickly, and roughly at the same latitude at which the explosion occurred.

Libby gives the content of radioactive strontium in the soil for a total of 17 stations in the U.S.A. The amount is measured in millicurie per square mile, and the mean is 20 (about 8 mc/km^2). Practically the whole amount is found in the upper soil. Strontium is bound to the soil particles and is washed out only slowly. There are, however, marked local differences in concentration. The higher values are found in South Dakota, Iowa, Utah and Michigan; here local fallout from the explosions in Nevada undoubtedly has played a role. High values are found also in the state of Washington and in New York while one finds low values in the Gulf States, Florida, Louisiana and Georgia as well as in New Mexico and California. Among the 17 values 5 lie between 6 and 15 millicurie per square mile (about

25 - 6 mc/km^2), 5 between 16 and 25 (about 6 - 10), and 7 between 26 and 36 (about 10 - 14). It is uncertain which factors determine the inequalities in the amount of the stratospheric fallout. However, knowledge on this point is of the greatest importance for estimates of which areas of the earth are most exposed.

The most difficult task which remains is to find out how the radioactive strontium content in the soil affects the concentration in the bone. Strontium is absorbed along with calcium. But fortunately less strontium than calcium is taken up. Therefore the strontium-calcium ratio in the bones is smaller than it is in the soil. Libby figures that plants which serve as food for cattle discriminate against strontium with a factor of 1.4. But this does not apply to the strontium which falls directly on the leaves, and which is taken up through the surface of the leaves. Our most important source of calcium is milk, and Libby estimates that 80% of the calcium requirements of the American population are obtained from this source. Libby estimates that when the cows produce milk strontium is kept out by a factor of 7. Finally, the human body keeps strontium out of the bones by a factor which is assumed not to be less than 2 and possibly is as high as 8. Therefore when humans drink milk only 1/20 to 1/80 of the strontium of the earth will enter the bone structure.

However, it must be stressed that this discrimination against strontium is not so effective where there is a deficiency of calcium in the soil. According to Libby the radioactive strontium content in animals who live on soil poor in calcium may reach 5 times the normal value, a consideration which is of importance in many regions in Norway. If one mixes milk from different farms and districts the effect of a local calcium deficiency may be evened out, and the strontium pickup may also be kept down by adding lime to the soil. The quoted estimate applies only to the part of the calcium requirement which is covered by milk consumption. As milk is a fairly expensive source of nourishment, little milk is consumed in many parts of the world.

Now there is another basis for evaluating the average discrimination factor against strontium. According to Turekian and Kulp the average strontium-calcium ratio in the bones is $0.6 \cdot 10^{-3}$. One knows roughly the strontium-calcium ratio in the lithosphere. For the amount of strontium Goldschmidt[2] gives 150 grams per ton, Rankama and Sahama[7] give 300 grams per ton and Rankama[6] 220 grams per ton. As this last source is the newest and the one which is most up to date we set the amount to 220 grams per ton. Calcium is estimated to be present in approximately equal amounts by the mentioned authors, Rankama gives 36,300 grams per ton. The strontium-calcium ratio in the

lithosphere is then $6.1 \cdot 10^{-3}$ which is about 10 times what one finds in the bones. Therefore one should probably not assume a better discrimination ratio than 10. The numbers for the strontium-calcium ratio in bones from America do not differ much from the numbers in other parts of the world. Unless the strontium concentration in American soil should be exceptionally high we must count with a factor of this magnitude also for American conditions. The radioactive strontium which falls on the leaves is resorbed directly without discrimination. This should result in a somewhat more unfavorable discrimination ratio for radioactive strontium than for normal strontium.

According to Libby an amount of fallout of 25 millicurie per square mile (about 10 mc/km^2), which is considered to be the average amount in America today, corresponds to a radio-strontium concentration in the upper soil of 50 strontium units. With a discrimination factor of 10 the bones in equilibrium with the surroundings should then have an average concentration of 5 strontium units. The values will vary greatly because of individual variations and inequalities in fallout and calcium content in the soil at the different locations.

If the atomic bomb tests were stopped, the radioactive strontium content in the soil in the United States would remain constant for some years. This is so because the stratospheric fallout roughly will balance the

radioactive strontium content which disappears through decay. This applies in the U.S.A. which has received a reasonably large dose of local fallout. If the atomic bomb tests are continued at the same rate as in the past the amount of fallout will increase to 8 times the present value and the average amount in the bones should reach about 40 strontium units. If the fallout is evenly distributed, and if one does not take into consideration differences in the calcium factor, 1.4% of the population should then reach 100 strontium units.

However, we mentioned already that the amount of radioactive fallout is not evenly distributed. In the quoted investigation about 30% of the stations had between 6 and 15 millicurie per square mile (about 2.5 - 6 mc/m^2), 30% had between 15 and 25 (about 6 - 10), and 40% had between 26 and 35 (about 10 - 14). If one considers the stations to be distributed roughly according to population density, and assumes that the ratios between the amounts of fallout will be the same in the future as they are today, one may estimate that more than 4.4% of the American population will get more than 100 strontium units in the bones if tests are continued at the present rate.

It will be apparent that there are many uncertain points in the estimates of the danger of radioactive strontium from atomic bomb tests. With a conservative estimate based on the health norms which are used today it is probable that continued explosions will present a

health danger for large sections of the population. It is too early to state anything definite about the probable damages of the atomic bomb tests which have occurred so far.

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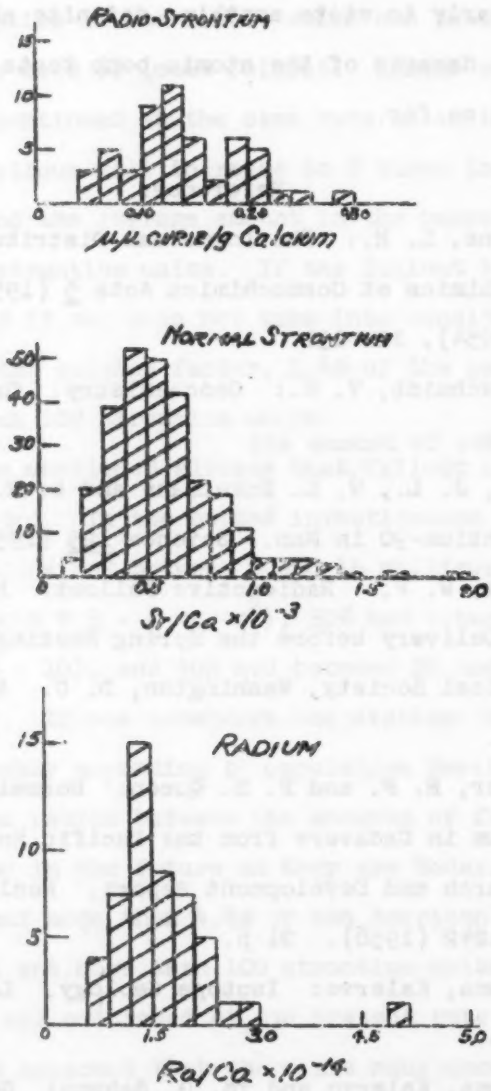

Fig. 1

The distribution of radio-strontium in 53 stillborn children in Chicago in 1953 (from Libby[4]), the strontium/calcium ratio in bones from the whole world (from Turekian & Kulp[8]) and the radium content in 50 bodies from northwest America (from Palmer & Queen[5]).

Table 1

The Strontium/Calcium Ratio (Multiplied by 10^3) in Human Bones Based on Data of Turekian and Kulp, Compared with the Expected Numbers According to a Normal and a Logarithmic Normal Distribution

Interval	0.0-0.1	0.1-0.2	0.2-0.3	0.3-0.4	0.4-0.5	0.5-0.6	0.6-0.7	0.7-0.8	0.8-0.9	0.9-1.0	1.0-1.1	>1.1
Observed	0	4	20	39	52	50	42	22	19	11	4	14
Expected according to normal distribution	8.3	10.8	18.0	27.4	36.6	41.6	40.2	35.2	25.8	16.6	9.4	7.2
Expected according to log normal distribution	0	3.3	22.2	43.8	52.6	45.7	35.2	24.9	17.5	11.1	7.2	13.6

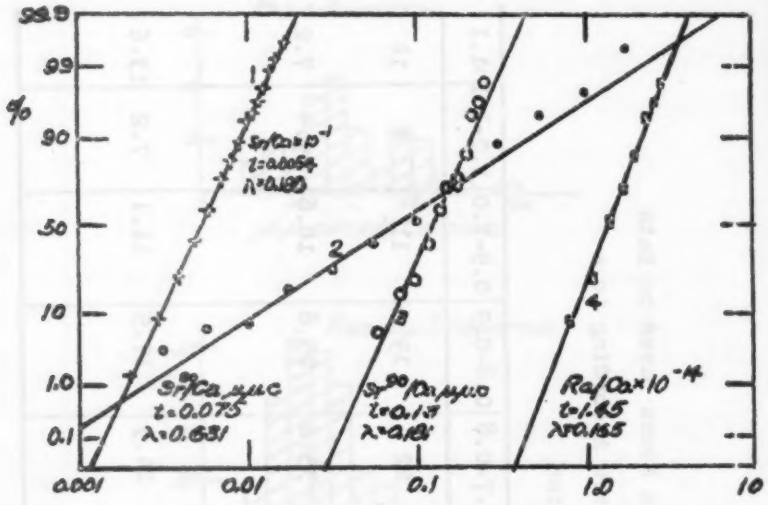

Fig. 2

Graphical representation in a scale which transforms log normal distributions to straight lines.

1. The strontium/calcium ratio in human bones.
2. The radio-strontium content in human bones from North America (from Kulp, Eckelmann & Schulert[3]).
3. The radio-strontium content in stillborn children in Chicago.
4. The radium content in 50 bodies from northwest America.

Further explanation in the text.

(Following is material submitted by Mr. Lauriston Taylor, Chief, Atomic Radiation and Physics Division, National Bureau of Standards. In addition "National Bureau of Standards Handbook No. 61" was submitted and is on file with the Joint Committee.)

April 23, 1959

Summary of NBS Handbook 69

"Maximum Permissible Amounts of Radioisotopes in the Human Body and Maximum Permissible Concentrations in Air and Water"

A Statement by the National Committee on Radiation Protection and Measurements

The National Committee on Radiation Protection and Measurements (NCRP) has completed the revision of its 1953 recommendations concerning the maximum internal permissible exposure of the human body to radioisotopes. Now in press as Handbook 69, "Maximum Permissible Amounts of Radioisotopes in the Human Body and Maximum Permissible Concentrations in Air and Water," 1/ these recommendations are being summarized in advance of publication because of widespread public interest in this subject.

The 1953 recommendations were for occupational exposure, 24 hrs per day, continuously for 70 years. The study was prepared by the Subcommittee on Permissible Internal Dose. It was directed by Dr. Karl Z. Morgan of Oak Ridge National Laboratory, took some 5 years to complete and included nearly 75 radioisotopes.

1/ National Bureau of Standards Handbook 69, "Maximum Permissible Amounts of Radioisotopes in the Human Body and Maximum Permissible Concentrations in Air and Water," expected to be available from the Superintendent of Documents, U.S.Government Printing Office, Washington 25, D. C., in about two months.

The study has continued under Dr. Morgan's leadership at an accelerated pace since 1953 with the added cooperation of the International Commission on Radiological Protection (ICRP). The present report includes maximum permissible concentrations (MPC) and body burdens for some 240 radioisotopes and includes many refinements based on knowledge not available in 1953. The study has included the evaluation of nearly 2000 separate researches reported in the literature, having bearing on the permissible dose problem. The new values reflect, where applicable, the lowering of the basic maximum permissible dose (MPD) recommended by the NCRP in 1957 and 1958. As a result of all of these considerations, some values have been increased, some decreased, and others remain essentially the same. In addition, the calculations have been changed from 70 years continuous occupational exposure to 50 years continuous occupational exposure - this latter being a more realistic figure for radiation workers.

The following table gives a few selected samples of the body burden and maximum permissible concentrations comparing the 1953 and 1959 reports:

Element & Organ	Body Burden microcuries		MPC (water) microcuries per cc		MPC (air) microcuries per cc	
	1953	1959	1953	1959	1953	1959
Ra^{226} bone	0.1	0.1	4×10^{-8}	10^{-7}	8×10^{-12}	10^{-11}
P^{32} bone	10	6	2×10^{-4}	2×10^{-4}	1×10^{-7}	2×10^{-8}
I^{131} thyroid	0.3	0.7	3×10^{-5}	2×10^{-5}	3×10^{-9}	3×10^{-9}
Sr^{90} bone	1	2	8×10^{-7}	10^{-6}	2×10^{-10}	10^{-10}
Cs^{137} muscle	90	50	1.5×10^{-3}	2×10^{-4}	2×10^{-7}	4×10^{-8}
whole body	---	30	-----	2×10^{-4}	-----	2×10^{-8}
H^{3} whole body	10,000	1,000	0.2	0.03	2×10^{-5}	5×10^{-6}

The table is for continuous occupational exposure. Where a worker is exposed for only 40 hrs/wk the MPC's are, in general, about 3 times higher.

Background Information

In 1934 the International Commission on Radiological Protection recommended for the first time what is now called a maximum permissible dose for occupational exposure to X-rays and gamma-rays originating in sources external to the body. In 1936 the NCRP proposed levels slightly lower than those of the ICRP. The basic NCRP value of 0.1 roentgens per day was employed in this country until 1948. This was derived in part from information based on an observable effect of X-rays on the skin. It was also based on radiation levels measured in areas where persons had worked for extensive periods and had suffered no observable ill effects.

Important information on the effects of radiation from radium deposited in the body was developed from the unfortunate experience in the radium dial painting industry. Here it was possible to make direct clinical study of a large number of victims, some of whom have died and many others of whom are still living. The early studies, supported by more recent ones, have shown that while all such radium deposits in the body may have caused some degree of effect, there appeared to be a quantity below which the individual was unaware of its existence. It could be detected only by the most careful clinical procedures, and was therefore described in the early days as "tolerable." In 1941 it was agreed that a total body burden of radium (mainly deposited in the skeleton) of 1 microcurie was tolerable, but to be on the safe side, a value of 0.1 microcurie was recommended for radiation workers. A vast amount of subsequent experience in the study of individuals exposed to radium has shown that the value of 1 microcurie may have been marginal but that the lower value of 0.1 microcurie in the body produces detectable damage only in the rarest of instances. This knowledge now forms one of our most important bases for establishing the permissible body burden for other radioisotopes that deposit principally in the bone.

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Other radioisotopes may deposit and concentrate in certain body organs, or may spread throughout the whole body. In each case the radioisotope moves about or is deposited in the body in essentially the same manner as its non-radioactive isotopes, e.g., radio-sodium behaves in this respect like ordinary sodium as in salt. Also, many radioisotopes move about in a manner often somewhat the same as chemically related but different isotopes, e.g., radio-strontium and ordinary calcium. Metabolism and other biochemical considerations often cause important deviations from such behavior.

Our knowledge of these metabolic processes is far from complete. However, for many chemicals, the extent to which they concentrate in the various body organs has been determined by direct experiment. From such data the distribution of other chemically-similar compounds...radioactive or not...can be determined approximately. Such experiments also give us considerable information as to how the chemicals pass through the body and are eliminated by normal processes, as well as the rates at which this occurs. It is therefore possible to determine directly in a few instances, and calculate in others, how much radiation dose is delivered to the whole body or individual body organs by these radioactive materials, either deposited or merely passing through.

For very short-lived radioisotopes, the body burden builds up to the maximum permissible level quickly and will only remain at this level if the exposure is practically continuous. For long-lived radioisotopes and particularly those that tend to concentrate and remain in certain parts of the body, intake would have to be continued at the maximum permissible level over a long period of time before reaching the maximum permissible body burden. However, a single intake may produce the maximum permissible level and then this level will be maintained for a long time. In any case, the body burden is the determining factor in setting the maximum permissible dose for radiation workers.

The problem of protection against radiation injury caused by radiation from external sources is relatively simple. You either shield yourself or stay away from the source. As soon as you remove yourself or the source, the exposure ceases.

Once radioactive material is inside the body, in general the exposure is reduced only by normal processes of body elimination, and normal decay of the radioactive material.

The problem, then, is to prevent the material from entering the body in dangerous amounts in the first place. At present this can best be accomplished by limiting the concentration of radioactive material in the air we breathe, the water we drink, or the food we eat, to such levels that the body burden will not build up to or beyond what is considered to be permissible. The determination of these concentrations involves a large number of very complicated factors, about some of which much is known, and about others of which much remains to be learned. It involves critical judgement on many factors where there is a range of choice. Wherever a choice must be made, it is invariably made in the conservative direction - that is, in the direction of lower concentrations. Sometimes there is uncertainty concerning the distribution of the radioactive material in the body. This is especially so with respect to the distribution within a body organ, but in most situations we have our best knowledge on the radiation dose delivered once a given body distribution is assumed or actually determined. Our biggest uncertainties are in establishing the MPC's that will prevent the body burden from exceeding the permissible amount. It is in this stage that the greatest conservatism is exercised.

PROTECTION OF PERSONS OUTSIDE OF CONTROLLED AREAS

Where radioactive material may escape from the controlled area and expose the neighboring public, the NCRP recommended in 1955 that the levels be 1/10 those for occupational exposure. This recommendation continues. It must be

emphasized, however, that the factor of 1/10 is for the primary purposes of controlling the radioactive materials discharged from an operation involving the use of such materials. The committee believes that exposure at the recommended occupational levels will cause observable radiation effects in an exceedingly small percentage, if any, of the radiation workers. Radiation effects to outsiders will be an even smaller percentage. The lower values recommended for persons exposed in the neighborhood of plant operations are given to assist in protection design for radiation installations.

For lack of better information, the lower values have also been employed as guides for radiation situations not connected with controlled operations. They are, however, guides that may be specifically applied in such situations. Properly used, they apply to only a small percentage of the population (2 or 3 percent at most).

WHOLE POPULATION EXPOSURE

Exposure of the whole population to natural and man-made sources of radiation presents a different problem and involves many factors beyond technical control. The NCRP reports, thus far, contain recommendations only on matters that are subject to control. The recommended values for MPD's and MPC's apply specifically only to occupational conditions and conditions in the neighborhood of controlled areas which are attributable to operations within the controlled area. Until more specific information is available, it is believed that with the exercise of some judgement similar principles may be applied as guides in application to problems involving exposure of the whole population to radiation.

The ICRP has made some tentative recommendations relative to whole population exposure. These are designed for long-range planning purposes and in this connection the NCRP considers them to be sound. They are not, however, subject

to direct control in the immediate future. In the meantime the NCRP considers that undue risks to the population will not be incurred by following current policies for a while longer, during which time it is hoped that methods may be established for a meaningful analysis and control of population exposure. When we can measure more accurately the body burdens of radioisotopes in the various body organs, we will have advanced further towards a practical solution of the problem. Recent developments in the analyses of body wastes and in the use of devices for determining the specific location of radioactive material in the body have been encouraging.

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on Radiation Protection (and later, the National Committee on Radiation Protection and Measurements - NCRP). The revised Committee included representation from the various professional societies with an interest in the problem, government agencies with related interest and responsibilities, as well as individual experts. The continued sponsorship of the Committee by the National Bureau of Standards was in accordance with its statutory responsibility to cooperate with other governmental agencies and with private organizations in the development of standard practices, incorporated in codes and specifications. In addition, the recommendations of the National Committee on Radiation Protection and Measurements have been published as handbooks by the National Bureau of Standards, again in accordance with its statutory authorizations.

The conclusions in the present handbooks are to be considered only as recommendations of a group of experts in the radiological protection field. They carry no legal implications demanding or requiring adoption. Inasmuch as the recommendations of the National Committee on Radiation Protection and Measurements impinge upon the areas of statutory responsibility of both the U. S. Public Health Service and the U. S. Atomic Energy Commission, it was considered important to determine that these agencies would not object to the publication of these recommendations by the National Bureau of Standards. Such assurances were obtained although these involve no commitment on the part of these agencies to adopt the recommendations of the National Committee on Radiation Protection and Measurements. Nor should the publication be construed as a recommendation by the National Bureau of Standards for adoption inasmuch as the important medical and biological factors involved in developing the recommendations are clearly outside of the Bureau's area of technical competence.

April 23, 1959

Preface to NBS Handbook 69 [1/]

The present handbook and its predecessors stem from the Second International Congress on Radiology, held in Stockholm in 1928. At that time, under the auspices of the Congress, the International Commission on Radiological Protection (ICRP) was organized to deal initially with problems of X-ray protection and later with radioactivity protection. At that time "permissible" doses of X-rays were estimated primarily in terms of exposures which produced erythema, the amount of exposure which would produce a defined reddening of the skin. Obviously a critical problem in establishing criteria for radiation protection was one of developing useful standards and techniques of physical measurement. For this reason two of the organizations in this country with a major concern for X-ray protection, the American Roentgen Ray Society and the Radiology Society of North America, suggested that the National Bureau of Standards assume responsibility for organizing representative experts to deal with the problem. Accordingly, early in 1929, an Advisory Committee on X-ray and Radium Protection was organized to develop recommendations on the protection problem within the United States and to formulate U. S. points of view for presentation to the International Commission on Radiological Protection. The organization of the U. S. Advisory Committee included experts from both the medical and physical science fields.

As a result of the extensive developments immediately preceding and during World War II that added substantially to the importance of radiation protection problems, the Advisory Committee was reorganized in 1946 as the National Committee

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1/ National Bureau of Standards Handbook 69, "Maximum Permissible Amounts of Radioisotopes in the Human Body and Maximum Permissible Concentrations in Air and Water." In press.

since it would be made available in the complete ICRP report published by Pergamon Press. A Table of Contents of the parts of the ICRP report not contained herein is given on page . In addition the ICRP committee has prepared a detailed bibliography containing hundreds of references relative to the problem. Since the demand for this bibliography will be somewhat limited it is expected that it will be made available in limited quantity as a mimeographed report from the Oak Ridge Laboratory.

The National Committee on Radiation Protection and Measurements is governed by representatives of 17 participating organizations, including the National Bureau of Standards. Eighteen subcommittees have been established, each charged with the responsibility of preparing recommendations in its particular field. The reports of the subcommittees are approved by the Main Committee before publication.

The following parent organizations and individuals comprise the Main Committee:

H. L. Andrews, U. S. Public Health Service and Subcommittee Chairman
E. C. Barnes, Am. Indust. Hygiene Assn.
C. M. Barnes, Amer. Veterinary Medical Assn.
A. C. Blackman, Int. Assn. of Govt. Labor Officials
C. B. Braestrup, Radiol. Soc. of North Amer. and Subcommittee Chairman
J. C. Bugher, Representative at large
R. H. Chamberlain, Amer. College of Radiology
W. D. Claus, U. S. Atomic Energy Commission
C. L. Dunham, U. S. Atomic Energy Commission
T. P. Eberhard, Amer. Radium Society and Subcommittee Chairman
T. C. Evans, Amer. Roentgen Ray Society
G. Failla, Representative at large
J. W. Healy, Health Physics Soc. and Subcommittee Chairman
P. C. Hodges, Amer. Medical Assn.
M. Kleinfeld, Int. Assoc. Govt. Labor Officials
H. W. Koch, Subcommittee Chairman
W. Langham, Subcommittee Chairman
R. M. Lechausse, Col, U. S. Air Force
G. V. Leroy, Subcommittee Chairman
W. B. Mann, Subcommittee Chairman
W. A. McAdams, Atomic Indust. Forum and Subcommittee Chairman
G. M. McDonnel, Lt. Col., U. S. Army
G. W. Morgan, Subcommittee Chairman

Since the publication in 1953 of the report of Subcommittee 2 of the National Committee on Radiation Protection on Permissible Internal Dose entitled "Maximum Permissible Amounts of Radioisotopes in the Human Body and Maximum Permissible Concentrations in Air and Water" (NBS Handbook 52) the study of the problem of internal irradiation has been continuous. The basic work for the preparation of this report has been handled to a major extent by Dr. K. Z. Morgan and by members of his staff at Oak Ridge. The same task has been carried out simultaneously for the corresponding committee of the International Commission on Radiological Protection of which Dr. Morgan is also Chairman.

Since 1953 new information relative to the problem of internal irradiation has been steadily increasing. Simultaneously there have been increasing demands for more information on radionuclides than was covered in the original edition of Handbook 52. In spite of the enormous amount of work which has been done by this subcommittee the problem of developing maximum permissible concentrations of radionuclides is still rendered difficult because of the relatively limited direct experience with the action of the radiation from radionuclides on human tissue. The contents of this Handbook are based on what is believed to be the best information available and it is to be expected that as our knowledge increases the numerical quantities presented in this report will be in a state of continuous modification.

The recommendations of this Handbook take into consideration the NCRP statement entitled "Maximum Permissible Radiation Exposures to Man," published as an addendum to Handbook 59 on April 15, 1958.

As noted above this study was carried out jointly by the ICRP and the NCRP, and the complete report is more extensive than the material contained in this Handbook. It was felt that for the sake of producing a Handbook suitable for daily use, some of the more extensive treatment of the problem could be omitted,

K. Z. Morgan, Health Physics Society and Subcommittee Chairman
R. J. Nelsen, American Dental Assn.
R. R. Newell, Amer. Roentgen Ray Society
C. Powell, U. S. Public Health Service
E. H. Quimby, Amer. Radium Society and Subcommittee Chairman
J. A. Reynolds, Natl. Electrical Mfgr. Assn.
H. H. Rossi, Subcommittee Chairman
M. D. Schulz, Amer. College of Radiology
L. S. Skaggs, Subcommittee Chairman
J. H. Sterner, Amer. Indust. Hygiene Assn.
R. S. Stone, Radiol. Soc. of North America
L. S. Taylor, National Bureau of Standards
E. D. Trout, Natl. Electrical Mfgr. Assn.
Bernard F. Trum, Amer. Veterinary Medical Assn.
Shields Warren, Representative at large
E. G. Williams, Representative at large
S. F. Williams, Capt., U. S. Navy
H. O. Wyckoff, Subcommittee Chairman

The following are the Subcommittees and their Chairmen:

Subcommittee 1. Permissible Dose from External Sources. (Responsibility of the Executive Committee)

Subcommittee 2. Permissible Internal Dose, K. Z. Morgan

Subcommittee 3. X-rays up to Two Million Volts, T. P. Eberhard

Subcommittee 4. Heavy Particles (Neutrons, Protons, and Heavier), H. H. Rossi

Subcommittee 5. Electrons, Gamma Rays and X-rays Above Two Million Volts, H. W. Koch

Subcommittee 6. Handling of Radioactive Isotopes and Fission Products, J. W. Healy

Subcommittee 7. Monitoring Methods and Instruments, H. L. Andrews

Subcommittee 8. Waste Disposal and Decontamination (This subcommittee has been inactivated.)

Subcommittee 9. Protection Against Radiations from RA, Co^{60}, and Cs^{137} Encapsulated Sources, C. B. Braestrup

Subcommittee 10. Regulation of Radiation Exposure Dose, W. A. McAdams

Subcommittee 11. Incineration of Radioactive Waste, G. W. Morgan

Subcommittee 12. Electron Protection, L. S. Skaggs

Subcommittee 13. Safe Handling of Bodies Containing Radioactive Isotopes, E. H. Quimby

Subcommittee 14. Permissible Exposure Doses Under Emergency Conditions, G. V. Leroy

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Subcommittee M-2. Standards and Measurement of Radiological Exposure Dose, H. O. Wyckoff

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In addition, many scientists of many countries have contributed, not only through their original research which is the basis of the report, but also by their generous aid in interpreting their results for use in this report. Finally, the technical work of collecting the data and interpreting it for conditions of occupational exposure as well as the writing of the text is largely the work of the Internal Dosimetry Section of the Oak Ridge National Laboratory headed by Dr. K. Z. Morgan. In particular, Mary Jane Cook has been responsible for the collection and presentation of the biological data, Mary Rose Ford has been responsible for the physical data used and for computation, James Muir and Janet Kohn have computed the tables for the gastro-intestinal tract values and for the effective energies respectively, and Dr. Walter S.

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USCOMM-NBS-DC

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RADIATION PROTECTION STANDARDS*

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When we speak about standards we may well ask ourselves what we mean. To most people standards mean something firmly fixed, or at least reproduceable — something quite permanent or something that is quite definitely definable. As a matter of fact, there are very few truly absolute standards.

We may think of physical standards - like the metre-bar with two scratches on it that can be stored away in a vault and which we can confidently expect to remain the same year in and year out. Or we may have what we call standard preparations. These are used, in the main, for checking measurement methods or techniques. For example, we have standard samples of steel which after careful analysis in the laboratory are sent out to the mill where they are run through an analysis procedure as a check on its accuracy. Or we may have standards of radioactivity - again where a radionuclide has been accurately measured in the laboratory and is then used to check the measurement technique in a hospital or some other laboratory.

*Presented at Meeting of the Washington Philosophical Society, May 8, 1959

In the field of human applications we have what may be called biological standards. These are frequently based on some specified biological result produced in an animal when exposed to the particular material in question. The proving of a drug against a biological standard involves the exposure of a number of animals and then a statistical analysis of the final result. By its very nature, this is a procedure that cannot use man as the standardizing animal. But at least a procedure can be described which for most cases will determine the potency of a biological agent within some range of certainty.

There is much talk today about radiation protection standards. Here, the so-called standards are based mainly on long experience with man working in a known environment of radiation and followed by the exercise of technical judgement as to whether or not an effect may have been produced on him. Radiation protection standards do not permit of direct testing. In my own humble opinion most of them should not be called standards at all. They might much better be called guides.

In fact most of our radiation protection standards of today are based on the absence of the demonstration of any deleterious effect. Because of our limited knowledge of the effect of low levels of radiation on animals, and the almost non-existent knowledge of the effects of small doses of radiation on man, the permissible exposure levels for radiation workers have been set low enough so that there is a negligible probability of radiation damage occuring to the individual exposed.

This concept may be summed up by a quotation from the most recent report of the International Commission on Radiological Protection. This says:

"The permissible dose for an individual is that dose accumulated over a long period of time or resulting from a single exposure, which in the light of present knowledge, carries a negligible probability of severe somatic or genetic injuries; furthermore it is such a dose that any effects that ensue, more frequently are limited to those of a minor nature that would not be considered unacceptable by the exposed individual and by competent medical authorities.

"Any severe somatic injuries (such as leukemia) that might result from exposure of individuals to the permissible dose would be limited to an exceedingly small fraction of the exposed group; effects such as shortening of life span which might be expected to occur more frequently, would be very slight and would likely be hidden by a normal biological variation. The permissible doses can therefore be expected to produce effects that could be detectable only by statistical methods applied to large groups."

It is obvious from this criterion that radiation protection standards based on an absence of observable effect, could be too low. About all that we know is that there is rarely, if ever, an identifiable radiation effect in relationship to a specific known radiation exposure at the permissible level.

On the other hand, it is also possible that our exposure standards may be too high. The mere fact that we have not been able to observe some damage does not necessarily mean there has not been damage. It may well be, that we simply do not yet know enough about how to observe an effect which may in fact be deleterious to the individual concerned. Also, when we speak of an "observed effect" we must use great care. It could well be that radiation might cause some effect, without this effect being, in itself, deleterious. Great widom will be required to properly define what is really deleterious.

The problem is further complicated, by the fact that at least at low radiation exposure levels, practically any effect which might be caused by radiation can also occur spontaneously for reasons unknown and unrelated to the radiation exposure.

Radiation effects may for convenience be placed in three categories. The first of these would be somatic effects or damage to the body cells exclusive of the germ cells. Included in this category might be such damage as bone sarcoma from the deposition of radioactive material in the bone.

The second would be genetic damage causing alterations in the germ cells which play a part in our hereditary process. Somatic effects, of course, are limited to the individual exposed, whereas genetic effects may be transmitted down through the family strain for generations.

A third effect is the shortening of the average life span, or life expectancy of individuals. The existence of shortening of the life span has been fairly well demonstrated in animals, but because of the complication of the problem, has yet to be demonstrated clearly in man. An often-quoted report on the life expectancy of early radiologists, as compared with other physicians, initially indicated an average shortening of the life span of about five years. However, when the same data was subjected to proper analysis the results turned out to be quite inconclusive.

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Life shortening, in itself, is something exceedingly difficult of definition. It is only rarely that this will show up as some identifiable radiation produced disease. For the most part the animal or man simply appears to grow old a little bit sooner, or may succumb a little bit more easily to the ordinary diseases to which he is prone. But these same things can happen anyway, from a variety of other causes. From all we know today, life shortening effects, if they exist in man at all, can only be detected by the most critical of statistical studies on large samples of exposed population.

One of our most important lacks of knowledge relates to the determination of the effects of very low level radiation exposures occuring over long periods of time. Thus far, no experiments with animals and no information on man have demonstrated that there exists a threshold of radiation effect. By threshold we mean some radiation level below which radiation damage definitely will not occur, and above which radiation damage probably will occur. Similarly we have virtually no knowledge on the linearity of radiation effects. Is there an established relationship between total dose or dose-rate and a measureable biological effect? For the most part, the answer to this today is that we do not know. On the other hand some information is beginning to develop as a result of a careful analysis of individuals developing leukemia. This is a clearly definable disease and it is known that it can be produced by radiation as well as by other causes.

To develop some perspective on the problem of establishing radiation protection standards for occupational exposure, it may be interesting to go back about 30 years to the time when the first of these was tentatively put forward. Early permissible exposures were expressed in terms of the fraction of a skin erythema dose produced by radiation under conditions where it was felt that there was some reasonable idea as to the amount of radiation involved. Of course the amount of radiation necessary to cause an erythema was not considered as acceptable. The first permissible dose as proposed by Mutscheller was specified as 1/100 of an erythema dose in 30 days. Others expressed this as 1/1000 of an erythema dose in 3 days.

It must be borne in mind that up to 1928, we had no acceptable units of radiation dose. By the early 30's the roentgen had become established as an acceptable unit. Measurements then showed that the permissible dose level could be expressed in terms of roentgens. The value turned out to be approximately 2/10 roentgen per day. This was recommended as the permissible dose level by the International Commission on Radiological Protection in 1934. In 1936 the National Committee on Radiation Protection in this country recommended a level of 1/10 roentgen a day. This was mainly to correct between dose measurements made in free air instead of on the skin.

Because of extensive radiobiological information gained during the Manhattan District operations and because of the introduction of kinds and quantities of radiations not considered earlier, the protection problem again came under intensive review in this country beginning in 1946. As a result of these studies it was found that the old level of 1/10 roentgen per day while probably satisfactory might be marginal. Accordingly, it was recommended in about 1948 that the level be reduced from 1/10 roentgen per day to 3/10 roentgens per week. This value was subsequently recommended by the International Commission in 1950.

At the time that this study began in the late 40's, there was an acute awareness of the genetic problem. It did not appear feasible then to establish any quantitative relationship between dose and genetic effect. However, at a meeting of the International Commission in Stockholm in 1952, at which a number of geneticists were present, an attempt was made to assess the situation. It was agreed then, that for genetic reasons the radiation exposure of the population should be kept as low as possible, and that preferably the average per capita dose should not exceed a value somewhere between 3 and 20 rems through the childbearing period. Unofficially, it seemed that a value of 10 rems might be satisfactory.

Beginning in about 1955 the genetic aspects of our radiation protection problem came under intensive study by various groups in this country and abroad. As a result of this the ICRP, in April 1956, recommended that the average per capita dose to the gonads not be greater than a value of the order of background radiation, in addition to background radiation. In June the National Academy of Sciences recommended the specific level of 10 rems average per capita gonadal dose for the whole population up to age 30. This age was selected because 50% of the childred were born to parents of that age or under.

In 1957 the NCRP stated this somewhat more specifically as follows:

"The maximum permissible dose to the gonads, for the population of the United States as a whole, from all sources of radiation including medical and other man-made sources and background, shall not exceed 14,000,000 man-rems per 1,000,000 of population over the period from conception up to age 30 and one-third of that amount each decade thereafter."

At the present time this can be broken down approximately as follows:

- TABLE 1 -

Background radiation	4,000,000 man-rems
Medical irradiation	5,000,000
Occupational exposure	150,000*
Radiation in plant environs	150,000*
Fallout	200,000*
	9,500,000 man-rems
Balance	4,500,000* man-rems

*Portions subject to control. This is the part referred to later that the ICRP includes in its definition of genetic dose.

At the time that the ICRP made its recommendation with regard to genetic dose for the whole population, it also reconsidered the problem of permissible dose standards for occupational exposure. For the principal purpose of holding down the genetic dose among radiation workers, it was found possible to further reduce the old occupational levels and still retain a satisfactory use of radiation sources. It was recommended that the dose for radiation workers not be accumulated faster than approximately 50 rems up to age 30, and 50 rems per decade thereafter. This could be reduced to an average of 5 rems/yr or 1/10 rem per week.

Table 2 gives a somewhat simplified picture of the trends in the basic permissible dose levels for occupational exposure since 1934:

- TABLE 2 -

	Level (rems/year)
ICRP (April 1956)	5
ICRP (1950-1956) at 0.3 rem/week	15
NCRP (1935-1948) at 0.1 rem/day	30
ICRP (1934-1950) at 0.2 rem/day	60
Prior to 1934	100

Considerable public attention has been directed recently to the repeated lowering of the maximum permissible dose for radiation workers. Some comments have implied that these lowerings were clear indication either of dereliction in establishing the earlier standards or that the bodies responsible for the recommendation didn't know what they were doing in the first place.

It should be emphasized that neither implication is valid. Nobody will admit more quickly than the many people who have worked on this problem that we still lack a great deal of information necessary to establish protection standards on a more quantitative basis. On the other hand the problem has been studied by the Nation's leading experts in the various aspects of the radiation protection problem. In addition, millions of dollars worth of research, mainly supported by the AEC, have been put into studies leading directly or indirectly to a better understanding of the overall protection problem.

A variety of factors have entered into the considerations leading to the successive lowering of the permissible exposure for radiation workers. For one thing the number of radiation workers is increasing rapidly so that damage, if it occurs, may involve a larger absolute number of people. Also, beginning in the 30's, new radiation sources operating at very much higher potentials were developed, so that individuals were exposed to more penetrating radiation, which could reach cells at body depths not as seriously affected under the conditions of low energy radiations used earlier. Also, many new sources of radiation began to be developed as the result of nuclear fission. We now have possible, exposure to very high energy X-rays, neutrons, as well as the normal radiations from radionuclides. Furthermore, because of the nature of nuclear-energy operations it is much more easily possible for people to get radioactive material into the body where it may lodge indefinitely or at least long enough to do some harm.

An important consideration in reducing permissible dose levels had also to do with the fact that improved techniques, and procedures, and improved knowledge of shielding, has made it possible to work with larger quantities of radiation, while at the same time exposing the workers to less stray radiation. The AEC for example, found a few years ago, that most of its operations were being conducted without exposing its workers, to more than a small fraction of permissible dose then in use.

With this brief review of the development of protection standards as we now have them, let us return to look at some of the difficulties involved. One of the problems has to do with the relationship between the rate at which the radiation is delivered, and the end biological effect. We know, for example, that the administration of heavy doses of radiation for therapeutic purposes induce a form of radiation sickness in the patient, whereas the same dose delivered more slowly in daily fractions may not produce any evidence of sickness. We know also that a dose of say a 1,000 roentgens given all at once to the whole body of an individual will cause early death. We know just as well that the same amount of dose has been accepted by large numbers of our early radiologists without any demonstrable damage having resulted.

These facts indicate that if the damage is not too great, some degree of tissue recovery must take place. Precisely how much or what the mechanism may be is not known, but there is no doubt that it exists.

As mentioned above, the first permissible dose was expressed in terms of radiation exposure per day. It made no difference, whether the day's exposure was delivered all at once, or uniformly, over the working period. Lack of evidence of any effect of larger daily exposure might be integrated. Other proposals would have allowed integration over a month or even a quarter of a year. Now there must be some point at which this extension of the integration time must cease. But so far there is no evidence to indicate that it makes any difference as to whether a permissible dose is received in a matter of seconds or in a month or two. This is provided, of course, that the dose is somewhere within the relatively low levels we are talking about. For practical reasons, some periods of integration have to be selected.

Our recommendations of today say, in effect, that an individual's exposure should not exceed an average of 5 rem per year, but added to this is a provision designed to prevent abuse of the averaging period. This provision states that within any 13 consecutive weeks the dose shall not exceed 3 rems.

You will notice, that up to here the discussion has dealt rather loosely with "averages"; yearly averages, weekly averages, daily averages and so on. But it is in precisely this area that difficulties begin to arise. These difficulties are not introduced just because of the hazards of radiation itself; difficulties are also introduced because of the hazards of our legal procedures. Radiation has become so important to so many people that its control is insisted upon, and properly so, of course.

However, in order to accomplish this in due legal style it seems we must put aside our sense of proportions and our sense of humor and we must define our permissible exposure levels in terms that are all completely black or all completely white. The inability of our legal procedures to deal with averages has led to the development of a sort of formula for expressing the maximum permissible dose (MPD) for occupational workers. This states that at any age the exposure of any individual shall not exceed 5 times the number of years of his age beyond 18, and may be expressed as MPD = 5(N-18) where N is the person's age.

To minimize the possibility of unduly large single doses, it is further specified that his exposure during any 13 consecutive weeks shall not exceed 3 rem. It should be pointed out that this formula carries no mathematical magic; it is merely a convenient way for expressing the principal that we desire to achieve. It satisfies the legal requirements; It is something that can be enforced; it is something that can be managed within any particular radiation installation.

As mentioned earlier, it is possible for radioactive material to enter the body where it may either lodge indefinitely, or be eliminated gradually. Elimination, of course, may take place either through the normal body processes or by means of natural radioactive decay. Standards for permissible amounts of radioactive material in the body are derived by two different approaches.

For radionuclides that are primarily bone-seekers and alpha or beta emitters, we fall back on the unfortunate experience of the radium dial workers in the 1920's. Here, through a great amount of effort, it has been determined that radium lodged in the body in amounts totaling less than 1 microcurie is not known to have produced damage to the individual. If there are any exceptions to this, they are exceedingly rare. Nevertheless, to be on the safe side, a permissible body burden for radium has been set at the level of 1/10 microcurie. In the meantime it has been determined that 1/10 microcurie of radium lodged in bone will produce a local dose of the order of 6/10 rem a week. Analysis of damage caused by radium, or other radionuclides behaving in a similar manner, is very complicated, and includes an understanding of the effects of distribution within the bone as well as the total amount present. For other radionuclides the acceptable amount in bone has been based on that amount which will not produce a local dose greater than the same 6/10 rem produced by radium. It is on this basis that, for example, the permissible body-burden of 2 microcuries is set for strontium-90.

We have no comparable knowledge on the effect of ingested radioactive material on other organs of the body. It was therefore decided that the dose to a single organ produced by any other radionuclide should not exceed the level previously set for the weekly whole-body dose for external radiation; namely, 3/10 rems per week. This appears to be very conservative since it is presumed that the whole body can withstand this same dose from external sources of radiation. On the other hand,it is difficult to assign a factor to the essentiality of a particular organ in relation to the function of the body as a whole.

It was mentioned above that in 1957 the basic permissible level for exposure of the whole body was reduced from an average of about 15 rem per year to about 5 rem per year. However, when we are considering exposure of individual body organs to radiation from ingested radionuclides, it is not believed necessary to make a reduction in the exposure of most individual body organs by this factor of 3. On the other hand if the gonads or the whole body is exposed, the reduction by a factor of 3 is considered to be necessary. Without going into further detail, you can see from this discussion, that our permissible dose standards go back to different fundamental considerations under different circumstances. However, cross checking as far as we are able, we find that no grave inconsistencies have been introduced by doing this.

Table 3 gives a simplified picture of the current basic permissible dose standards for occupational exposure.

- TABLE 3 -

Basic "permissible dose" standards
(Occupational exposure)

External irradiation:

Whole body, gonads, bloodforming organs, eyes - - 5 rems/yr (average)

Internal irradiation:

Whole body, gonads, bloodforming organs - - 5 rems/yr (0.1 rem/wk)

Thyroid, bone (for certain radionuclides) - - 28 rem/yr (.56 rem/wk)

Other single organs - - - - - - - - - - - - - 15 rem/yr (0.3 rem/wk)

A remark was made above about our lack of knowledge as to whether radiation effects were linear or non-linear or whether there are thresholds. It should be pointed out that the various protection bodies, in setting permissible levels such as discussed above, have always operated on the basis that there were no thresholds and that the effects were essentially linear in nature. This, of course, may or may not be true, but if true it would imply that there is some degree of radiation damage to the human system no matter what the exposure level may be. This is in spite of our failure to observe specific effects at very low doses. Also if true, this implies that any radiation exposure whatsoever involves some possible risk to the individual. We are thus faced with the dilemma that if we are to reduce radiation risk to zero we must at the same time eliminate all use of all radiation. Of course we cannot eliminate background radiation. Again, if this is true, we must be willing to face this small and undefinable risk. It is something that we are accustomed to in many other areas of life and I will not dwell on it further at this time.

We should emphasize again, our almost total lack of knowledge about radiation damage at very low exposure levels. Simply because we assume a linear dose-effect relationship and the absence of a threshold this does not necessarily make either true. The use of such assumptions however makes our current permissible dose levels very conservative. On the other hand, we must be very careful indeed as to how this is interpreted to the public.

As already stated, all of our permissible radiation exposure levels are based on the absence of positive knowledge of damage. This in itself should provide some degree of reassurance. It means, that the percentage of people exposed and who have resultant damage is so small as to be essentially undetectable. It may never be absent but the percentage is extremely small. On the other hand if one considers the entire population of a country of this size, while the percentage is small the absolute numbers may be appreciable. Here we find ourselves involved in a very serious philosophical argument. Do we deal in percentages or do we deal in numbers? In either case the individuals are not defined.

Let us turn to another set of standards which we are compelled to use for reasons that will become obvious. Radiation is produced in a variety of places that we commonly refer to as installations. The permissible levels discussed thus far refer to the individuals who work within these installations, and for whom the working conditions are extremely carefully controlled. The individuals are monitored; they are subject to special health precautions. On the other hand if radiation excapes from the installation, as for example, through the wall or ceiling to the next door apartment, or by way of contaminated effluents from an atomic energy plant, radiation or radioactive material may subject people outside of the controlled area to an exposure over which they themselves have no control. For ethical as well as biomedical reasons, we cannot expose these people to the same levels that may be allowed to individuals within the plants. This is in spite of the fact that there is no evidence of damage to workers within the plants.

For people exposed outside of controlled installations the permissible exposure levels have been set at 1/10 of those allowed within the installation. This is not arbitrary in spite of the fact that no damage is presumed to have occurred to the people exposed to the permissible levels within the plants. On the other hand people outside a particular plant might be exposed not only to the radiation from one plant but possibly from several; there is no way of controlling this. Furthermore, because of the possible escape of radioactive material larger numbers of people may be exposed outside of the plant than inside of the plant. Even more critical is the fact that the outside population will contain children and pregnant women; the embryo is especially sensitive to radiation. We have here, a situation where an individual is subjected to conditions over which he himself has no control. People outside of installations can be protected only by properly regulating operations within the installations.

Thus far all of the levels that have been discussed are subject to direct control, and in a sense we have set standards (or preferably guides) by which this control can be effected.

We have a very different situation involving the general population when this population is subjected to other sources of man-made radiation not subject to control from within an installation such as has been described. For example we have the present-day problem of radioactive fallout.

As already mentioned, the NAS, NCRP, and other bodies have recommended a permissible exposure for the whole population at an average per-capita dose not exceeding 10 rem above background up to age 30, from all sources of man-made radiation. Here the primary consideration is the genetic burden to the whole population.

As far as an individual is concerned, exposure at 1/10 the maximum permissible occupational level appears to be very conservative. However, if the entire population were to receive this amount of exposure the geneticists feel certain that the total would be unacceptably high. For example, if we take the average of 5 rems per year multiplied by 1/10 and again multiplied by 30 we would find that this would give a dose of 15 rem to the gonads up to age 30. In addition to this would be the background of 3 or 4 rems and the average medical exposure of 5 rems making a total of somewhere between 20 and 25 rems up to age 30. This is unacceptably high.

Table 1 shows roughly how we may stand today with regard to the distribution of gonadal dose from the various sources of radiation to which our population is exposed.

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The International Commission on Radiological Protection has recognized the need for some guidance in future planning of the amount of radiation that may be delivered to the public from all sources. Without some advance planning there is of course the danger that unbridled exploitation of radiation by industry, medical applications or weapons testing, might result in each of them increasing to a degree that in total would be unacceptable. For planning purposes the Commission has given the illustrative apportionment of the genetic dose shown in Table 4.

TABLE 4

ICRP GENETIC DOSE GUIDES

(Excludes Natural Background and Medical Exposure)

Available: 5 rems, Average Per Capita Dose Up to Age 30

Occupational	1.0 rem	5 rem/yr to 1/7% of pop. for 12 yrs
Special groups	0.5 rem	0.5 rem/yr to 3% of pop. for 30 yrs
Population-at-large	1.5 rem	0.05 rem/yr to 100% of pop. for 30 yrs (internal emitters
	0.5 rem	0.05 rem/yr to 33% of pop. for 30 yrs (external radiation)
Reserve	1.5 rem	
	5.0 rem	

In developing the illustrative apportionment above, the ICRP has excluded both background and medical irradiation. The first cannot be controlled and is widely variable. The latter is considered separately because of its variability and also because other methods are being studied for limiting it. In addition, medical irradiation serves a useful purpose. The remaining sources of irradiation correspond to those indicated in Table 1 and amount to 5×10^6 man-rems per 10^6 population or an average per-capita dose of 5 rems.

It should be emphasized, that these suggestions of the ICRP are purely illustrative guides; at the present time we can see no way of enforcing them.

We are, however, concerned in this country about the manner in which these recommended guides will be accepted. This concern stems from the fact that we in the U. S., are attempting to legislate our way through much of the problem of radiation protection. There is some fear that the mere mention of such guides as shown here may be picked up by some well-meaning legislator and made into law. It is clear indeed that one cannot legislate on such matters. One can, nevertheless, use such guides for constructive national planning; one can watch closely over the output from various sources of radiation and if it is found that any one of these is proceeding in a dangerous trend, steps can be taken to curtail it. It is not an unreasonable plan. But the National Committee on Radiation Protection in this

country believes that it is wise, for the time being, to refrain from making specific recommendations along these lines, with the same degree of confidence that it has made recommendations for permissible dose under situations that are completely within our control.

In due course a decision will have to be made at a high governmental level as to how we are going to assign the radiation exposure of the population to the different sources which produce it in the first place. We believe however, that the problem is not so pressing but that we can wait for a few years longer before attempting to solve it in any regulatory way. To attempt to do this at the present time, with the meager knowledge and experience we have, would undoubtedly hamper the uses of radiation to a degree that could well be unacceptable to our society as a whole. Radiation is far too valuable an agent in the service of man for us to put fetters upon it in such a way that it is never allowed to properly grow. But as a matter of general principle we must attempt to our best ability to hold down radiation exposure from all sources whatsoever.

BASIC "PERMISSIBLE DOSE" STANDARDS

EXTERNAL IRRADIATION:	
Whole-body, gonads, bloodforming organs, eyes	5 rem/yr (Average)
INTERNAL IRRADIATION:	
Whole-body, gonads, bloodforming organs	5 rem/yr (0.1 rem/wk)
Thyroid, skin of whole-body	30 rem/yr (0.6 rem/wk)
Bone (for certain radionuclides)	(0.56 rem/wk)
Other single organs	15 rem/yr (0.3 rem/wk)

ICRP genetic dose guides

(Excludes natural background and medical exposure)

Available: 5 rems, average per capita dose up to age 30

Occupational	1.0 rem	5 rem/yr to 1.7% of pop. for 12 yrs
Special groups	.5 "	.5 rem/yr to 3% " " " 30 yrs
Pop. at large	2.0 "	.05 " " to 100% " " 30 yrs
Reserve	1.5 "	
	5.0 "	

Fourth Suggested Revision of Appendix B, Handbook 61

Section 1. Scope

It is the purpose of these regulations to state the requirements that shall be applied in the use of sources of radiation, to reduce, to an acceptable minimum, the risk to all persons from the use of all such sources of radiation.

Section 2. Application

These regulations shall apply to the manufacture, use, storage, handling, transportation and disposal of radiation machines and radioactive materials except as exempted by the provisions of Section 4.

Section 3. Definitions

For the purposes of these regulations the following definitions shall apply. (71-73)

Absorbed dose of any ionizing radiation is the energy imparted to matter by ionizing particles per unit mass of irradiated material at the place of interest. The unit of absorbed dose is the rad.

Adult is a person of age 18 or more. 5/

Agency is that governmental agency that is given the responsibility for administering these regulations.

Body burden is the amount of radioactive material in the body at the time of interest. 6/

Critical organ is that part of the body that is most susceptible to radiation damage under the conditions considered.

Dose evaluation is the determination, without personnel monitoring devices, of the radiation dose received by a person during a specified period.

5/ Section 6, National Bureau of Standards Handbook 59

6/ Section D, National Bureau of Standards Handbook 52

Excessive radiation dose is a dose of radiation in excess of the maximum permissible dose.

Harmful effect is any body injury, disease, or impairment, except where such condition is transitory, infrequent, or of short duration and does not endanger persons so affected.

Installation is an area in which one or more sources of radiation is used. The use of a source of radiation is the storage or operation, or anything done with a source of radiation.

Maximum permissible dose is a dose of radiation that, in the light of present knowledge, is not expected to cause appreciable bodily injury to a person at any time during his lifetime. 7/

Mobile source is a radiation machine or a sealed source, the use of which is not confined to an installation.

Personnel monitoring is the determination of the radiation dose received by the person during a specified period. The determination is made by film-badges, small ionization chambers, or other dosimeters worn by the person.

Population group is a civil population (usually more than 100,000 persons) living in grographic proximity and generally dependent upon the same sources of food and water.

Qualified expert is a person fitted by training and experience to perform dependable radiation surveys, to oversee radiation monitoring, and to estimate the degree of radiation hazard. If the ability of a qualified expert is questioned, the Agency shall be the judge of his qualifications, in regard to which it may consider the testimony of other persons whom it deems expert.

(R-5) Rad is the unit of absorbed dose and is equal to 100 ergs per gram. 9/

7/ Section 4.3, National Bureau of Standards Handbook 59, as modified.
8/ National Bureau of Standards Handbook 51.
9/ International Commission on Radiological Units (1953). See Am. J. Roentgenol. Radium Therapy Nuclear Med. 71, 139 (1954); Radiology 62, 106 (1954)

(R-6) Radiation is gamma rays and x-rays, alpha and beta particles, high-speed electrons, neutrons, protons, and other nuclear particles; but not sound or radio waves, or visible, infrared, or ultraviolet light.

Radiation hazard is any condition that might result in the exposure of persons to radiation in excess of the maximum permissible dose.

Radiation machine is any device that produces radiation when in use.

Radioactive material is any material, solid, liquid, or gaseous gas, that emits radiation spontaneously.

(R-7) RBE (relative biological effectiveness) is the biological effectiveness of one type and energy of radiation, relative to that of lightly filtered x-rays generated at potentials of 200 to 300 kilovolts, for the particular biological system and biological effect, and for the conditions under which the radiation is received. 10/ (A list of RBE values for various kinds of radiation is given in Table 4 of Section 15). (Note to Subcommittee - please give an opinion re Reynolds' comment)

(R-8) Rem is the unit of RBE dose and is the quantity of any radiation such that the energy imparted to a biological system (cell, tissue, organ, or organism) per gram of living matter by the ionizing particles present in the region of interest, has the same biological effectiveness as an absorbed dose of 1 rad from lightly filtered X-rays generated at potentials of 200 to 300 kilovolts. The RBE dose in rems is equal to the dose in rads multiplied by the appropriate RBE. 11/

Source of radiation is a quantity of radioactive material or a radiation machine. A sealed source is a quantity of radioactive material enclosed to prevent the escape of any radioactive material. 12/

10/ Section 3.6, National Bureau of Standards Handbook 59.
11/ Section 4.8, National Bureau of Standards Handbook 59.
12/ National Bureau of Standards Handbook 54.

(R-9) Survey is the evaluation of radiation in and near an installation. 13/

(R-10) User is a person, organization or institution having administrative control over one or more installation or mobile source.

Section 4. Exemptions

(a) The following materials, machines and conditions are exempt from these regulations:

(1) Natural radioactive materials of an equivalent radioactivity not exceeding that of natural potassium. (The specific radioactivity of natural potassium is 6.9×10^{-6} curies per gram)

(2) A radioactive material in a single sealed source in an amount not exceeding 10 times the amount given in Section 14, Table 1, or in any number of sealed sources in a total amount not exceeding 100 times the amount given in Section 14, Table 1, providing that the dose rate at the point of nearest approach to the source(s) does not exceed 1/10 the maximum permissible limits specified in Section 6b. The manufacture of the sources themselves shall not be exempt.

(3) A radioactive material, not in sealed source form, in an amount not exceeding the amount given in Section 14, Table 1, or in a concentration, when used in combination with non-radioactive materials, not exceeding the concentrations given in Section 14, Table 2, providing that the dose rate at the point of nearest approach to the material does not exceed 1/10 the maximum permissible limits specified in Section 6b.

13/ National Bureau of Standards Handbook 51.

(Notes to Subcommittee)

Note 1. Table 1 will be similar to that in present Handbook 61. Table 2 will be similar to that proposed recently by the AEC for addition to Part 30 of CFR Title 10. NCRP/SC 10 and AEC will try to agree on same tables.

Note 2. This rewrite of paragraphs 2 and 3 is intended to cover also the previous paragraph 4.)

(4) Electrical equipment that produces radiation incidental to its operation for other purposes, but that, by nature of design, does not produce radiation at the point of nearest approach at a dose rate in excess of 1/10 the maximum permissible limits specified in Section 6b. The production testing or production servicing of such equipment shall not be exempt.

(5) Radiation machines not being used in such manner as to produce radiation.

(6) Radioactive material being transported across the state in conformance with regulations of any Federal agency having jurisdiction over safety in interstate transport.

(7) Other radiation machines or radioactive materials which the agency finds to be without radiation hazard.

Section 5. Registration

(a) The user shall register each installation or mobile source with the Agency within 30 days after receipt of the first source, and once every two years thereafter as long as he continues to possess any source of radiation. The registration shall be submitted on a form obtained from the

Agency, and shall describe the confines of the installation, and the use, type, activity level and age (or date of purchase) of each source. The registration shall also give the name and address of the user and the name, address and qualifications of the qualified expert.

(b) The user shall further notify the agency in writing within 30 days of any changes in the installation or mobile source which may substantially increase the potential for personnel exposure.

(c) Acknowledgment of registration shall not imply the Agency's approval of the conditions described in the registration.

Section 6. Maximum Permissible Exposure Limits

(a) The user shall control all sources of radiation within an installation, and all mobile sources, in such a way as to provide reasonable assurance that no person will receive a dose of radiation in excess of any maximum permissible exposure limit specified in paragraphs (b) through (f) below.

(b) Accumulated Dose - External Exposure - Occupational

For persons within an installation or who use a mobile source,

1. The maximum permissible dose (MPD) to the whole body, head and trunk, active blood-forming organs, gonads, or lens of the eyes, accumulated at any age shall not exceed 5 rems multiplied by the number of years beyond 18 (i.e. MPD= (N-18) where N is the age in years and is greater than 18); and the dose in any period of 13 consecutive weeks shall not exceed 3 rems.

2. The maximum permissible dose to the skin of the whole body, accumulated at any age, shall not exceed 10 rems multiplied by the number of years beyond 18, (i.e., MPD= N-18);

and the dose in any period of 13 consecutive weeks shall not exceed 6 rems.

3. The maximum permissible dose to the hands and forearms, and the feet and ankles shall not exceed 75 rems per year; and the dose in any 13 consecutive weeks shall not exceed 25 rems.

(c) Accumulated Dose - Internal Exposure - Occupational

For persons within an installation or who use a mobile source, radiation dose to tissues of the body from radioactive materials within the body shall be controlled by limiting the average rates at which such materials are taken into the body. Where this intake results from the occurrence of radioactive materials in the air or water, the average concentration of the radioisotopes in the air or water shall not exceed the maximum permissible concentrations listed in Section 14, table 5.

(d) Accumulated Dose - External Exposure - Non-Occupational

For persons outside an installation who do not use a mobile source, the maximum permissible dose to the whole body, head and trunk, active blood-forming organs, gonads, or lens of the eye, due to mobile sources, sources within an installation or radioactive materials escaping an installation, shall not exceed 0.5 rem in any one year.

(e) Accumulated Dose - Internal Exposure - Non-Occupational

For persons outside an installation who do not use a mobile source, the radiation dose to tissues of the body from radioactive materials within the body shall be controlled by limiting the average rates at which such materials are taken into the body. Where this intake results from the occurrence of

radioactive materials in the air or water, the average concentrations of the radioisotopes in the air or water shall not exceed one-tenth the maximum permissible concentrations listed in Section 14, Table 5.

(f) Accidental and Emergency Exposure (For consideration by the Subcommittee)
Notwithstanding the provisions of Section 6 (a) (b) and (c), an individual need not be restricted from radiation work if he receives an occasional accidental or emergency dose to the whole body, head and trunk, active blood-forming organs, gonads, or lens of the eyes, not exceeding 25 rads. However, if in any 13 consecutive weeksor in any year, several persons receive a dose in excess of any of the maximum permissible limits of Section 6 (b) through (e), the Agency may at its discretion compell the user to alter the installation or mobile source, or the operating procedures.

Section 7. Survey and Monitoring

(a) The user shall provide for surveys and monitoring sufficient to determine the levels of radiation to which persons in and around the installation or mobile source are, or may be, exposed. Such surveys and monitoring shall be made by, or under the advice of, a qualified expert.

(b) The user shall provide for the routine personnel monitoring or, on the advice of a qualified expert, the dose evaluation of all persons within an installation or who use a mobile source. However, on the advice of a qualified expert, he may discontinue such personnel monitoring and dose evaluation, if the results

for six consecutive months show that no person has received a dose in excess of 25% of the maximum permissible limits specified in Section 6b. He shall reinstate the personnel monitoring or dose evaluation after any change in equipment or operating procedure that would increase the dose to any person.

(c) The user shall not be required to provide routine personnel monitoring or dose evaluation of persons occupationally exposed to radiation from radiation machines when all of the following conditions are met:

(1) A qualified expert has specified the operating conditions under which there is no reasonable chance that any person will be exposed to more than twenty-five percent (25%) of the maximum permissible limits specified in Section 6b.

(2) The operating conditions in (1) above are made known to all persons who may be occupationally exposed to the radiations.

(3) The installation continues to perate only under the specified conditions.

Section 8. Radiation Exposure Records and Reports

(a) The user shall record the personnel monitoring and dose evaluation results for each individual as obtained under Section 7a. When personnel monitoring and dose evaluation is discontinued under the provisions of Sections 7b and 7c, the user shall estimate and record the dose received by each individual.

(b) The user shall preserve the exposure records on each individual as long as the individual remains in his employ. When an employee terminates his employment, the user shall turn over the records,

or a certified copy, to the Agency for retention. As an aid in keeping track of an individual's exposure, these records shall include his Social Secutiry number.

(c) Upon termination of employment of a person, the user shall supply to the Agency, upon request, a summary statement of that person's total radiation dose. (The estimated maximum dose shall be stated if no personnel monitoring or dose evaluation has been carried out.) This record shall include statements of any circumstances wherein the dose to the employee, from any source of radiation, exceeded those specified in these regulations. 19/

(d) When it is known or believed that dose to a person in the installation may have exceeded 5 times the amount permitted by applicable portions of Section 6 of these regulations, the user shall report to the Agency within 7 days of the discovery, all of the facts relative to the occurrence thereof and shall place a copy of the report in that person's personnel file. The user shall immediately seek out and correct the cause of the overexposure. (Note to Subcommittee - Alternate paragraph in line with AEC requirements to be prepared later.)

(e) The user shall keep records of the amount and kind of radioactive materials received by and discharged from or transported out of an installation, including radioactive waste products released to the environs.

(f) All the records required in this section shall be made available to the Agency on request.

(g) Any release of radioactive material to the environs in concentrations

or total quantities exceeding ____ times amounts listed in (to be completed later after discussions with the AEC) shall be reported to the Agency within ____ hours. (Note that this paragraph has been moved to this Section from Section 12)

(h) The loss or theft of any registered source of radiation shall be reported to the Agency within ____hours.

Section 9. Control of Exposure

(Note that the previous Section 9. Medical Exposure has been deleted. I believe that is what we agreed to do)

(a) The user shall provide for the services of a qualified expert who will supervise for the radiation safety program of the installation.

(b) The user shall provide, or have readily available, instruments suitable for the detection and measurement of radiation and radioactive contamination in accordance with other provisions of these regulations, and he shall further see that these instruments are maintained in proper calibration.

(c) Every employee and authorized visitor shall use such safety devices as are furnished for his protection and for carrying out all radiation safety rules that concern or affect his conduct.

Section 10. Storage of Radioactive Materials

(a) The user shall see that radioactive materials are controlled or kept in such a manner as will provide assurance that no person will inadvertently receive a dose in excess of the maximum permissible limits set forth in Section 6 either during routine access to the installation, or in the event of a fire, earthquake, flood, windstorm, explosion or other emergency.

(b) Vaults or rooms used for storing materials that may emit radioactive gases shall be ventilated in such a manner that the gases do not constitute a radiation hazard.

(c) When there is any possibility that chemical, radiation, or other action might lead to leakage, the container shall be provided with a secondary tray or catchment adequate to retain the entire amount of radioactive material.

Section 11. Control of Radioactive Contamination

(a) All work with radioactive materials shall be carried out under such conditions as to minimize the possibility of any contamination that would result in any person's being subjected to radiation levels exceeding those specified in Section 6 of these regulations.

(b) Where the nature of the work is such that a person or his clothing may have become contaminated to such degree as to present a hazard, both shall be monitored according to procedures established by the qualified expert. Contamination external to the body which could lead to a dose in excess of any value specified in Section 6 shall be removed according to procedures established by the qualified expert. Clothing or other material contaminated to a degree which could result in the exposure of any person to a dose in excess of any value specified in Section 6 should be retained inside the installation until it can be decontaminated or disposed of.

Section 12. Radiation Information Labeling

(a) The user shall indicate the presence of radiation by posting conspicuous signs or labels either on each source or on its

container. He shall similarly post signs in and near storage areas, work areas, or any other areas, where radiation hazards exist on a frequent or infrequent basis. The signs and labels shall bear appropriate wording (such as "caution-x-ray", "danger-radiation", "caution-radioactive material") to further explain the nature of the hazard.

(b) The standard symbol for designating any radiation hazard shall be:

(To be pictured with dimension ratios)

The standard color specification shall be a background of yellow with the lettering and distinctive symbol in purple (magenta).20/ The use of this symbol for any other purpose is expressly prohibited. The symbol and lettering shall be as large as practical, consistent with size of the equipment or material.

(c) All containers, source holders, manufactured products or other items containing a quantity of radioactive material greater than one-tenth (1/10) the amounts listed in Section 15 Table 1 shall be labeled with the standard radiation symbol specified in paragraph (b) above. A label shall not be required for laboratory containers, such as beakers, flasks, and test tubes, used transciently in laboratory procedures, when the user or his representative is present.

(d) All radiation work areas, source storage areas, or other normally occupied areas where a radiation hazard may exist shall be posted with signs or labels bearing the standard radiation symbol (except where such labels may be a source of disturbance to patients undergoing radiation treatment.)

(Note to Subcommittee - I believe we agreed to keep in the part in ( ). Correct me, if I am wrong.)

(e) All areas that are readily accessible but not normally occupied, and where a radiation hazard may exist on a frequent or infrequent basis, shall be suitably fenced off and posted with signs having the standard radiation symbol.

(f) All signs and labels bearing the radiation symbol shall be removed when the hazard is no longer present.

Section 13. Disposal of Radioactive Wastes

(Notes to Subcommittee - (1) The note in ( ) has been relegated to the preface as background information, as agreed. (2) I thought we agreed to use Newell's proposed rewrite of paragraph (a), but I don't quite know what to do with paragraph (d) if we do this. Therefore, I have modified (d) (1) somewhat, in an attempt to clarify the wording and meaning.)

(a) Users shall dispose of radioactive material in such a manner that the material discharged, in combination with that discharged by other owners, will not cause contamination of the environs in excess of any maximum permissible limit in Sections 6d and 6e. If several users are discharging radioactive waste to the same environs, they shall, upon being notified of the fact, cooperate in limiting the release and shall file with the Agency a statement of their agreed pro-rata releases. If this is not done within a reasonable time, the Agency arbitrarily may assign quantity limits to them severally.

(b) Users who release radioactive materials may take reasonable advantage of the environmental factors (dilution, dispersion, etc.)

to minimize the cost of disposal, provided they meet the performance requirements indicated in paragraph (a) above.

(c) An owner may dispose of radioactive waste by dumping or burial only in areas and by procedures approved by the Agency for that purpose. Such areas shall be designed and operated so that they comply with the other provisions of these regulations. (Note to Subcommittee - AEC representatives are to review this provision to see if it poses any problem with respect to their procedures.

(d) For purposes of controlling the non-occupational exposure of persons outside the installation, the limits of radioactivity resulting from disposal of radioactive material shall be determined on the following basis:

(1) The average concentration of the isotope (or isotopes) in air at points where it is commonly used by humans or in water at points of supply (exclusive of treatment, if any) prior to use by humans shall not exceed the maximum permissible limits specified in Sections 6d and 6e (i.e. 10% of the concentrations listed in Section 14, Table 5) of these regulations. When it is not possible to determine the concentrations at the point of use or supply, the average concentrations at the point of release from control of the user shall not exceed these limits. Concentrations lasting only over a period of a few days may be allowed to exceed these specified limits provided the average concentration over any interval of one year does not exceed them.

(2) The total quantity of the isotope (or isotopes) released in a period of any 24 consecutive hours, shall not exceed the quantities listed in Section 14, Table ___ (Note to Subcommittee - I believed we agreed to put this in, but I don't know what Table

to use or, for that matter, what values to suggest.)

(3) Average rates of radiation dose to persons from radioisotopes outside their bodies shall not exceed the maximum permissible limits specified in Section 6d of these regulations.

(4) Average concentrations in portions of public waterways not used as sources for human consumption shall be consistent with health, economic, and recreational uses of the water and the future plans for its use that are under consideration by responsible authorities.

(5) For any radioisotope where the effective half-life in the body is less than 60 days, the term "average" as used above shall mean the arithmetic mean of a series of determinations representative of plant operations and environmental conditions over any period of 13 consecutive weeks; for other radioisotopes this arithmetic mean shall be taken over a period of any 12 consecutive months.

(6) If the permissible average concentration of a mixture of radioisotopes in air or water depends almost entirely on the concentration of one of the radioisotopes involved, for routine practical estimates the contributions of the other radioisotopes in the mixture may be neglected. Where more than one isotope is important, the permissible concentration shall be arrived at by adding the exposures to be received from each significant component.[21]

(e) Prior to and during design and construction of facilities for disposal of radioactive wastes, users of radioactive materials

shall review their disposal plans with the Agency. However, the user shall remain responsible for meeting the performance standards related to radioactivity established by the Agency and shall allow representatives of the Agency to inspect and evaluate his methods of treatment and release.

(f) Nothing in these regulations shall be construed as permitting release of materials that would be unlawful for other reasons.

Section 14. Tables Referred to in Previous Sections.
Tables to be supplied later)

Fifth Suggested Revision of Appendix B, Handbook 61

Section 1. Scope

It is the purpose of these regulations to state the requirements that shall be applied in the use of sources of radiation in order to reduce, to an acceptable minimum, the risk to all persons from the use of all such sources of radiation.

Section 2. Application

These regulations shall apply to the manufacture, use, storage, handling, transportation and disposal of radiation machines and radioactive materials except as exempted by the provisions of Section 4.

Section 3. Definitions

For the purposes of these regulations the following definitions shall apply:

Absorbed dose of any ionizing radiation is the energy imparted to matter by ionizing particles per unit mass of irradiated material at the place of interest. The unit of absorbed dose is the rad.

Adult is a person of age 18 or more.

Agency is that governmental agency that is given the responsibility for administering these regulations.

Controlled area is a defined area in which the occupational exposure of personnel to radiation or to radioactive material is under the supervision of an individual in charge of radiation protection. (This implies that a controlled area is one that requires control of access, occupancy, and working conditions for radiation protection purposes.)

Dose, as used in these regulations, is a quantity of ionizing radiation in which the energy is imparted to body tissue.

Dose rate is the dose delivered per unit time.

Installation is an area in which one or more sources of radiation is used. The use of a source of radiation is the storage or operation, or anything done with a source of radiation.

Maximum permissible dose is a dose of radiation that, in the light of present knowledge, is not expected to cause appreciable bodily injury to a person at any time during his lifetime. Appreciable bodily injury means any bodily injury or effect that competent medical authorities would regard as being deleterious to the health and well being of the individual.

Mobile source is a radiation machine or a sealed source, the use of which is confined to a controlled area but not necessarily to an installation.

Monitoring is the periodic or continuous determination of the dose rate in an occupied area or of the dose received by a person.

Personnel monitoring is the determination of the radiation dose received by the person during a specified period by film badges, small ionization chambers, or other dosimeters worn by the person.

Qualified expert is a person fitted by training and experience to perform dependable radiation surveys, to oversee radiation monitoring, and to estimate the degree of radiation hazard. If the ability of a qualified expert is questioned, the Agency shall be the judge of his qualifications, in regard to which it may consider the testimony of other persons whom it deems expert.

Rad is the unit of absorbed dose and is equal to 100 ergs per gram.

Radiation is gamma rays and x-rays, alpha and beta particles, high-speed electrons, neutrons, protons, and other nuclear particles; but not sound or radio waves, or visible infrared, or ultraviolet light.

Radiation machine is any device that produces radiation only when in operation.

Radioactive material is any material, solid, liquid, or gaseous, that emits radiation spontaneously.

RBE (relative biological effectiveness) is a numerical factor which stands for the appropriate value of the biological effectiveness of the radiation in question relative to that of x-radiation with an average specific ionization of 100 ion pairs per micron of water, for the particular biological system and biological effect under consideration and for the condition under which the radiation is received. (A list of RBE values for various kinds of radiation is given in Table 4 of Section 15).

Rem is the unit of RBE dose and is the quantity of any radiation such that the energy imparted to a biological system (cell, tissue, organ, or organism) per gram of living matter by the ionizing particles present in the region of interest, has the same biological effectiveness as an absorbed dose of 1 rad from lightly filtered x-rays generated at potentials of 200 to 300 kilovolts. The RBE dose in rems is equal to the dose in rads multiplied by the appropriate RBE.

Source of radiation is a quantity of radioactive material or a radiation machine. A sealed source is a quantity of radioactive material enclosed

to prevent the escape of any radioactive material.

Survey is the evaluation of radiation in and near an installation.

User is a person, organization or institution having administrative control over one or more installation or mobile source.

Section 4. Exemptions

(a) The following materials, machines and conditions are exempt from these regulations:

(1) Natural radioactive materials of an equivalent specific radioactivity not exceeding that of natural potassium. (The specific radioactivity of natural potassium is 6.9×10^{-6} curies per gram)

(2) A radioactive material in a single sealed source in an amount not exceeding 10 times the amount given in Section 14, Table 1, or in any number of sealed sources in a total amount given in Section 14, Table 1, providing that the dose rate at the point of nearest approach to the source (s) does not exceed 1/10 the maximum permissible limits specified in Section 6b. The manufacture of the sources themselves shall not be exempt.

(3) A radioactive material, not in sealed source form, in an amount not exceeding the amount given in Section 14, Table II.

(4) Radioactive material in a concentration, when used in combination with non-radioactive materials, not exceeding the concentrations given in Section 14, Table 2, providing that the dose rate at the point of nearest approach to the material does not

exceed 1/10 the maximum permissible limits specified in Section 6b.

(Note to Subcommittee

Table I will be similar to that in present Handbook 61. Table Z will be similar to the table which the AEC recently added to Part 30 of CFR Title 10.)

(5) Electrical equipment that produces radiation incidental to its operation for other purposes, but that, by nature of design, does not produce radiation at the point of nearest approach at a dose rate in excess of 1/10 the maximum permissible limits specified in Section 6b. The production testing or production servicing of such equipment shall not be exempt.

(6) Radiation machines not being used in such manner as to produce radiation.

(7) Radioactive material being transported across the state in conformance with regulations of any Federal agency having jurisdiction over safety in interstate transport.

(8) Other radiation machines or radioactive materials which the agency finds should be exempted.

Section 5. Registration

(a) The user shall register each installation or mobile source with the Agency within 30 days after receipt of the first source, and once every two years thereafter

as long as he continues to possess any source of radiation. The registration shall be submitted on a form obtained from the Agency, and shall describe the confines of the installation and any adjacent areas where radiation may be present as a result of sources within the installation. The registration shall also state the use, type, activity level and age (or date of purchase) of each source and shall also give the name and address of the user and the name, address and qualifications of the qualified expert.

(b) The user shall further notify the agency in writing within 30 days of any changes in the installation or mobile source which may substantially increase the potential for personnel exposure.

(c) Acknowledgment of registration shall not imply the Agency's approval of the conditions described in the registration.

(At 5:15 p.m., Thursday, May 7, 1959, a recess was taken until Friday, May 8, 1959, at 10 a.m.)

FALLOUT FROM NUCLEAR WEAPONS TESTS

FRIDAY, MAY 8, 1959

CONGRESS OF THE UNITED STATES,
SPECIAL SUBCOMMITTEE ON RADIATION,
JOINT COMMITTEE ON ATOMIC ENERGY,
Washington, D.C.

The subcommittee met, pursuant to recess, at 10 a.m., in room P–63, the Capitol, Hon. Chet Holifield (chairman of the subcommittee) presiding.

Present: Representatives Holifield, Durham, Price, Van Zandt, Bates, and Westland; Senators Anderson (chairman of the Joint Committee) and Gore.

Also present: James T. Ramey, executive director; George E. Brown, Jr., and George F. Murphy, professional staff members; Richard T. Lunger, technical adviser; and Dr. Paul Tompkins, committee consultant, Joint Committee on Atomic Energy.

Representative HOLIFIELD. The committee will be in order.

Before we start this morning's session I would like to express on behalf of all members of the Joint Committee our profound sense of loss over the news of the passing away last night of Donald A. Quarles, Deputy Secretary of Defense. Mr. Quarles was a fine and honorable man, and this country owes him a great deal of gratitude for his many contributions to the Nation's security. I have known Dr. Quarles for several years, and had him before the Subcommittee on Military Operations, another subcommittee of which I am chairman, and he has been before the Joint Committee on Atomic Energy many, many times. We feel a deep sense of personal loss, as well as public loss, at the untimely passing of Dr. Quarles.

I would like to make a brief statement with regard to the subcommittee's approach to these hearings which, in view of some recent editorial comments, I don't believe has been completely understood.

As I mentioned in my opening statement, the committee's major aim is to get the facts on fallout on the table where everyone can see them. Where there are no facts available we are seeking expert opinion from the men who are doing research in the field. As I have stated before, we have attempted to get a variety of views on a given subject area, so that overall presentation would be well balanced.

In this connection, the subcommittee wishes to express its thanks to an informal advisory group which was invited to assist the committee in preparing for the hearings and to make suggestions as to subject matters to be covered and witnesses to be invited. We had such highly competent people on this advisory group as Dr. Walter V. Selove, of the University of Pennsylvania, who is the new chairman of the Federation of American Scientists. We had people like Dr.

Neuman, of the University of Rochester, Dr. Bentley Glass, of Johns Hopkins University, giving us the benefit of their great experience, together with representatives of the Public Health Service, the Atomic Energy Commission, the Department of Defense, the National Academy of Sciences, the National Bureau of Standards, and the U.S. Weather Bureau. I have great confidence in these men and in their suggestions regarding the expert witnesses of varying points of view who could make an important contribution to the hearing.

I would like at this point to reaffirm the subcommittee's intention to get all the available data we can on the major issues involved on the fallout question for the use of the Congress, the scientific community, and the public. I think it is important that no pertinent points be swept under the rug. This is in the best tradition of scientific inquiry, and I would say that in addition to the witnesses who have appeared personally before this committee to testify, that we have received many papers from scientists throughout the United States, all of which will be printed in the record, from men of such diverse views as Dr. Linus Pauling, Morris B. Visscher and others. Their statements will be accepted and placed into the record on the same parity as any other data which is presented before this committee. (See vol. 3, apps. B, F, and G.)

The committee does not intend to be swayed by emotional arguments on the relative dangers or lack of dangers from fallout. We are trying to keep this inquiry as objective as we possibly can, and we will let the chips fall where they may. Due to limitations on the committee's time, we have not been able to hear all of the scientists in the United States who are competent in the fallout field. As the press and public have noted, we have in addition to the oral presentations which have been made sought an equal number of statements for the record from experts in the field so that as complete a printed record as possible will result. We have had valuable contributions from scientists in individual States such as North Dakota and Minnesota, whose papers will also be made a part of the record.

The subcommittee is grateful to all of these scientists of varying opinions who have contributed their time and effort to giving the committee, the Congress, and the public a better understanding of the fallout question. There are still many unresolved questions and differences of opinion. There are scientists who have changed their minds about certain complex aspects of the problem since our last hearings in 1957. This is as it should be, and I think no apologies should be made by these dedicated men.

We ran a little behind schedule yesterday, so we will begin this morning's session with a discussion on the status and implications of testing. To prepare for this discussion, the committee has requested that a panel of specialists in this area hold a seminar prior to discussion of the matter in these hearings, and that they draw up a brief summary statement reflecting the consensus of the group.

At this point I will place in the record an outline of our request.

(The document referred to follows:)

OUTLINE OF INFORMATION WANTED UNDER ITEM VIII OF HEARINGS "STATUS AND IMPLICATIONS OF TESTING"

SPECIAL SEMINAR

I. Purpose:

A. To obtain as quantitative an evaluation as possible of the increase in the radiation environment of the world from the radioactive isotopes introduced by the testing of nuclear devices.

B. Forecast what the future status would be as a function of time if all such testing is discontinued.

C. Forecast what the future status would be if the testing experience of the past 5 years is essentially duplicated in the next 5 years.

II. Desired form of the forecasts:

A. Estimates are desired for at least four time periods:

1. The period 1947–53 (pre-Castle).
2. The period 1953–58, inclusive (current).
3. The period 1958–63 (future).
4. The equilibrium condition for the assumed testing rate.

B. Inventory:

1. Total quantities of fission products (or MT equivalent) created during each period. (MT detonated.)

2. Quantities of fission products (or MT equivalent) unavailable for worldwide fallout through "close in" deposition and the total quantity available for offsite distribution.

3. The total quantity deposited during the period, their distribution and resultant concentrations in terms of activity per unit area. The resultant increase in gamma background as well as isotope concentrations of interest should be estimated.

C. Status at the end of each period showing—

1. Amount not yet deposited and its distribution.
2. Distribution of material deposited on the ground.
3. Resultant concentrations in selected foods, such as milk and grain. (Selection to be made by the group.) This may also be shown by graphs of concentrations in soil, food, and bones as a function of time at selected localities (preferably from a high concentration area).

III. Future testing implications:

A. The concentration of Sr^{90}, Cs^{137}, etc., in food and bone, as well as the gamma radiation levels, should be forecast at least one period beyond that over which testing is assumed.

B. The future implications of testing should be forecast by assuming that such programs will not differ substantially from the pattern established over the period 1953–58.

IV. The group may wish to relate the results of these environmental estimates to alternative standards of permissive levels of exposure of a population, or criteria for limiting the radiation exposure of a population. The Joint Committee, during the roundtable under item VIII, will undoubtedly desire to obtain information on the relationship between the environmental estimates and alternative standards or criteria of permissive radiation exposure of populations.

V. A short statement should be included showing how the fallout consequences can be substantially altered. This should include, but not necessarily be limited to, underground testing, stratospheric testing, and limitations on amount and location of weapon debris injected into the atmosphere. This need not be quantitated but general ranges coupling the yield to detonation conditions should be attempted. Evaluation can be made by estimates of probable quantities of fission products available for deposition resulting from such alterations.

ROUNDTABLE DISCUSSION BY FALLOUT PREDICTION PANEL

Representative HOLIFIELD. I understand that Dr. William Neuman has been selected by the group to present this summary. Following Dr. Neuman's presentation, we will address questions to the panel and discuss the various aspects of their report. Following these discussions we will have presentations on the research programs being un-

dertaken by various Government agencies in the field of fallout, and the present status of organizational arrangements.

I suggest that Dr. Neuman come forward, and other members of the panel also, at this time, to avoid later confusion. Those members are Dr. Wright Langham, Dr. Willard F. Libby, Mr. Joshua Holland, Dr. Frank Shelton, Dr. Lester Machta, Mr. Hal Hollister, Mr. Merril Eisenbud, Dr. Walter Selove, and Dr. James Terrill.

We have also a statement sent up to the committee by Dr. Libby in response to our request. It is very short. Does this have matters in it which need to be discussed by the panel, Dr. Libby? I have not had a chance to read it.

Dr. LIBBY. The panel was not entirely clear, Mr. Holifield, but we rather decided that it ought to be a statement that was made more by the Department of Defense and the AEC than by the panel. The nature of the questions to the panel decided that. They asked those who were connected with AEC and DOD to prepare something in answer to your question. That is what we are doing, preparing something of a technical nature on this question of fallout from tests. That is what it is. I think it is in the context of the panel, and it was part of the question you addressed to the panel, but the panel decided it did not want to deal with it. So we took it on separately.

Representative HOLIFIELD. Then we will have your short statement read after Dr. Neuman's statement, and then it will be open for discussion of the panel.

You may proceed, Dr. Neuman.

Dr. NEUMAN. Before I read the formal report of the seminar, I would like to make a few very brief and personal comments, if I may.

Representative HOLIFIELD. Proceed, please.

Dr. NEUMAN. It was exciting and, I might add, an exhausting experience, this seminar. I feel we might have spent more time at a little more leisurely deliberation with some profit. Nonetheless, in my opinion, a great deal was accomplished because the scientific atmosphere was excellent, impersonal and factual, yet friendly and cooperative.

The questions asked by your committee embrace so many fields of specialty that no one seminar participant could possibly claim authoritative competence in all of the various fields. To say that this report represents the consensus opinion then is to say that each of the participants took part in developing his own area of professional specialization. In other words, if the meteorological predictions prove to be horribly wrong, it is not my fault, and so on and so on, for the rest of my colleagues. This report then profits from the advantages and suffers from the disadvantages to which all group "togetherness" efforts fall heir.

I fear the seminar removed some spontaneity from the testimony thus far, and I feel also that the report will be somewhat redundant, but it has had a good influence in bringing us all together ahead of time.

Not stated in the report, but I believe by tacit implication, this seminar adopted the assumption that strontium 90 and cesium 137 represent the principal sources of hazards from past tests. In the

present setting, that is, with the cessation of tests, this assumption is exceedingly difficult to challenge.

Representative HOLIFIELD. Before you start your formal statement, the Chair would like to state that the purpose of this seminar was to obtain as quantitative an evaluation as possible of the increase in the radiation environment of the world from the radioactive isotopes introduced by the testing of nuclear devices; (*b*) forecast what the future status would be as a function of time if all such testing is discontiuued; (*c*) forecast what the future status would be if the testing experience of the past 5 years is essentially duplicated in the next 5 years, and so on.

It was upon this call that the seminar was asked to come together and those are the subjects to be discussed.

STATEMENT OF 1959 FALLOUT PREDICTION PANEL READ BY DR. WILLIAM F. NEUMAN

Dr. NEUMAN (reading):

Once again at the request of the Joint Committee on Atomic Energy a panel of specialists met to review predictions of radioactive contamination in the environment and in man. Many of the scientists who were present at the 1957 hearings again participated. A list of the participants follows:

Lyle T. Alexander, USDA.
Allen V. Butterworth, AEC.
Charles L. Dunham, M.D., AEC.
Gordon M. Dunning, AEC.
Merril Eisenbud, AEC.
Edward P. Hardy, AEC.
John Harley, AEC.
J. Z. Holland, AEC.
Hal Hollister, AEC.
Harold A. Knapp, AEC.
Wright Langham, AEC.
Willard F. Libby, AEC.
Lester Machta, U.S. Weather Bureau.
Charles L. Marshall, AEC.
E. A. Martell, Air Force Cambridge Reservation Center.
William Neuman, Universtiy of Rochester.
J. Calvin Potts, AEC.
A. Schulert, Lamont Geological Observatory.
Walter Selove, University of Pennsylvania.
Robert F. Reitemeier, AEC.
Frank Shelton, AFSWP, DOD.
James Terrill, USPHS.
P. Tompkins, Naval Radiological Defense Laboratory.

The present group was charged with the responsibility of evaluating the "status and implications of testing." (An outline of subject matter to be considered was furnished the group by the Joint Committee, p. 1787.) On the assumption that the final assessment of hazard must be made in biological terms and in view of the uncertainties in the transposition of low-level radiation into expected biological effects it was possible to ignore small differences, alternate hypotheses, and alternate models. For example, for different assumed models of stratospheric distribution it was the similarities in predictions rather than the differences which seemed important.

In this frame of reference, an attempt was made to summarize the total inventory of fission products produced by test explosives to date. Despite the uncertainties having to do with many of the individual tests being summated, the overall totals were of a relatively high order of certainty. These totals are given in table I.

(Table I follows:)

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TABLE I.—*Total yield of test explosions to date*

[Megatons]

	Fusion	Fission	Total
All countries	80	[1] 90	170
United States, plus United Kingdom		65	

[1] Of which less than 1 megaton was introduced before 1952.

Dr. NEUMAN. These figures have been given earlier: For fusion all countries, approximately 80 megatons; for fission all countries, 90 megatons; giving a grand total of approximately 170 megatons exploded to date. For the United States and United Kingdom, the fission megatonnage is approximately 65 megatons. It is important to note that, of all these totals, less than 1 megaton of fission was introduced before 1952. [Reading:]

Next, an attempt was made to estimate a rough material balance of the total fission products produced between: (*a*) That deposited at or near the test sites, (*b*) that already fallen to earth as worldwide fallout, and (*c*) that still in the atmosphere. There were several methods of approaching this problem. Direct measurements of the stratosphere are now available, fission product ratios permit estimates of fractionation, and finally, direct measurements of fallout on the test sites on various occasions have been used. Despite the paucity of data and the uncertainties of each approach, the agreement was remarkably good and as a summary the overall, approximate distribution is given in table II.

(Table II follows:)

TABLE II.—*Estimated overall fission product distribution (November 1958)*

	Megacuries Sr^{90}	Equivalent Megatons
In atmosphere	3	30
On ground, Worldwide	3	30
At or near testing sites [1]	3	30

[1] Tropospheric plus stratospheric.

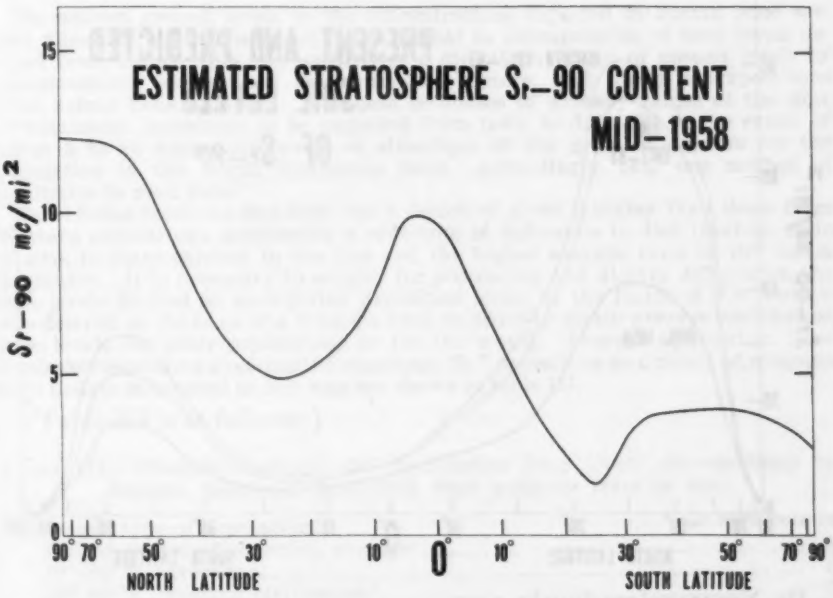
Dr. NEUMAN. We find that in terms of megatons fission products produced, approximately 30 megatons are still in the atmosphere or were injected into the atmosphere and have not fallen; 30 megatons are on the ground in worldwide distribution, and 30 megatons settled near the test sites. In terms of strontium 90, that gives three megacuries in the atmosphere, three megacuries on the ground and three megacuries at or near the test sites. These values are not corrected for decay. [Reading:]

To translate these inventories of Sr-90 into terms which can be used to predict future bone levels of Sr-90, it is necessary to summate two quantities: (a) the distribution of fallout already on the ground and (b) the distribution of the fission products still in the stratosphere and their probable future pattern on the ground.

The first quantity is derived from actual soil analyses for Sr-90. The second quantity, the distribution of fission products now in the stratosphere, is based on measurements too few and too uncertain (the Russian tests are of so recent origin that only limited data are available) to eliminate some serious uncertainties in interpretation. The mid-1958 (the latest period for which completed measurements are available) stratospheric inventory according to latitude is given in figure 1.

(The material referred to follows:)

FIGURE 1



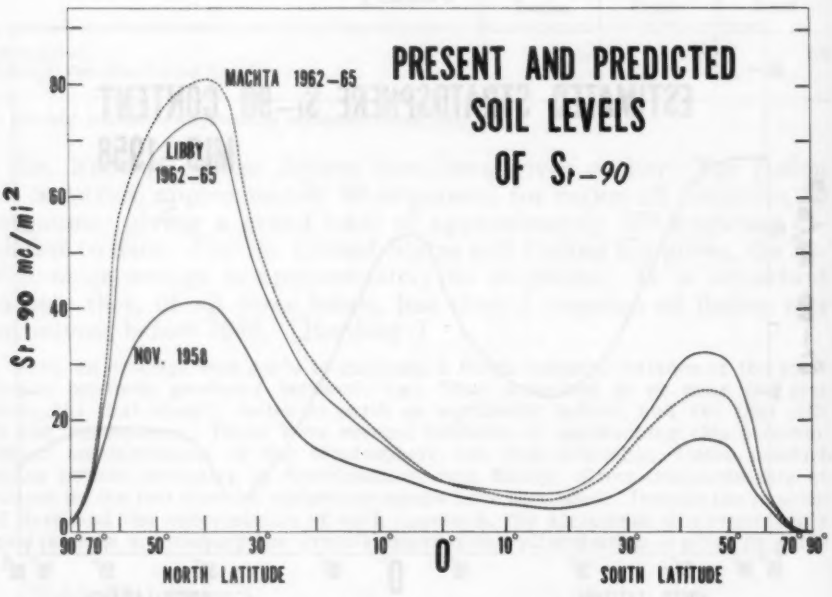
Dr. NEUMAN. You will note the strong polar concentration. This is mid-1958, before the Russian shots last fall. Despite the polar concentrations, fallout, for meteorological reasons which have been discussed earlier, has been concentrating in the northern latitudes. [Reading:]

It was generally agreed that the bulk of the airborne inventory will fall in the Northern Hemisphere with banding in the North Temperate Zone at least as nonuniformly as has occurred for the total fallout to date. As a first approximation, it seemed reasonable to conclude that the present stratospheric inventory, when all deposited, will cause ground levels, worldwide, to double those observed in November 1958.

The estimated present (November 1958) and predicted ground levels are given in figure 2.

(The material referred to follows:)

FIGURE 2



Dr. NEUMAN [reading]:

The forecast made by Libby partitions fallout from pre-October 1958 stratospheric debris equally between hemispheres; Machta's prediction deposits twice as much in the Northern as in the Southern Hemisphere. Both estimates keep all of the October 1958 Russian debris in the Northern Hemisphere. It should be noted that fallout in the United States may be 10–15 millicuries per square mile higher than the predicted peak because of tropospheric fallout. The maximum cumulative levels of Sr-90 are expected to occur in about 1962–65; Sr-90 decay to this date has been included in the estimate.

The important thing, I think, to note here, is that the predicted levels, made by the most divergent hypothetical models we have, give really very close estimates.

Representative HOLIFIELD. The top line is Dr. Libby's line and the second line is Dr. Machta's line, is that right?

Dr. NEUMAN. No, sir. You have them just reversed. The higher prediction is that of Machta's model which keeps more of the airborne strontium in the Northern Hemisphere.

Representative HOLIFIELD. What is the difference in degree there approximately, or percentages?

Dr. NEUMAN. The difference is 72 in one instance and 81 in another, about 10 or 12 percent.

Mr. RAMEY. How much would you add for your tropospheric?

Dr. NEUMAN. These have been included, if I am not mistaken. We add another 10 or 15 millicuries per square mile to predict the U.S. ground levels.

Mr. RAMEY. So that would be what?

Dr. NEUMAN. Between 82 and 96 millicuries per square mile.

Mr. RAMEY. Eight or ten percent for the Nevada tests?

Dr. NEUMAN. Yes. Even so, I think you will appreciate that, when we have figures between 80 and 100, and try to translate this difference in biological terms, this is rather academic. [Reading:]

To convert ground levels to the concentrations expected in human bone several alternate approaches were possible, that is, extrapolation of bone levels derived from known milk concentrations, a direct conversion of ground levels by experimentally determined discrimination factors, and direct extrapolations from actual bone analyses. All these estimates of average values at the time of maximum deposition to be expected from tests to date fell in the range of about 5 to 10 micromicrocuries of strontium 90 per gram of calcium for the population in the North Temperate Zone. Accordingly, only one method of prediction is used here.[1]

Fetal bones from the Far East run a factor of about 2 higher than those from Western populations, supposedly a reflection of difference in diet (that is, ratio of milk to plant calcium in the diet and the higher average ratio of Sr^{90} to Ca in plants). It is necessary to weight, for population and dietary differences, the bone levels derived by multiplying deposition levels by the factor of 0.08 (which was derived on the basis of a Western diet) in order to obtain average equilibrium bone levels for other populations or for the world. Average equilibrium bone levels corresponding to estimated maximum Sr^{90} deposition as a result of weapons tests to date calculated in this way are shown in table III.

(Table III is as follows:)

TABLE III.—*Predicted average Sr^{90} equilibrium bone levels corresponding to average maximum deposition from weapons tests to date*

Region and type of population:	*μμc Sr^{90}/gram Ca*
United States of America, average	6
20°–60°N, average [1]	8
20°–60°N, Western civilization [2]	5
20°–60°N, Eastern civilization [3]	10
World average, weighted [1]	7

[1] Weighted by both population and diet.
[2] Milk constitutes major source of calcium in diet.
[3] Plants constitute major source of calcium in diet.

Dr. NEUMAN. In this estimate, it is predicated at the time of maximum deposition, bone at equilibrium would have the following levels: In the United States, an average of 6 micromicrocuries of strontium 90 per gram of calcium. In latitudes 20 to 60 degrees north, an average of 8. For 20 to 60 degrees north in Western civilization, an average of 5. From 20 to 60 degrees north in Eastern civilization an average of 10.

For the world average, weighted for population and dietary differences, an average of 7 micromicrocuries Sr^{90}/gram calcium. [Reading:]

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[1] The bone data of Kulp, et al., indicate an estimated equilibrium level in bone of the Western population between 20° N.–60° N. as of January 1958. of 2 micromicrocuries of strontium 90 per gram of calcium. This estimate is consistent with other estimates derived from Sr^{90}/Ca ratios in diet (including milk) assuming a discrimination ratio of 0.25 in going from the diet to the bone. That is to say, that the average Sr^{90}/Ca ratio in bone at equilibrium will be one-fourth of that ratio in the diet. As of January 1958 the average soil deposition level in the north temperate population belt (between 20° N.–60° N. where 80 percent of the world population lives) was 25 millicuries per square mile. Dividing 2 micromicrocuries of strontium 90 per gram bone calcium by 25 millicuries per square mile gives a factor of 0.08 micromicrocuries per gram bone calcium per millicurie per square mile. Multiplication of any estimated soil deposition level (present or future) by this factor in the north temperate population belt will give the average equilibrium Sr^{90} bone level for all populations whose diet corresponds to that of the Western populations.

This method uses the concept of bone-soil equilibrium as a basis of computation. The equilibrium bone level is that level which should ultimately be attained if environmental contamination remained constant. With changing environmental levels, the actual bone levels lag behind the equilibrium value in time, that is, when environmental levels are rising, bone levels will be less than the equilibrium value. The panel recognizes that environmental contamination levels will not, if there is no further testing, remain constant. Nevertheless, for the 1962–65 period this computation should give a reasonable estimate of expected levels.

These values are averages only and make no allowances for individual variations as a result of local variations in fallout, difference in physiological status, et cetera. The statistical frequency distribution of the human bone values has, of course, been tabulated. However, inherent in these data are unusual problems of distribution, sampling, and analysis, all of which tend to increase the variability of the observed values. For this reason, it was believed unprofitable to try to express the variability in precise statistical terms for this statement.

In addition to considering future levels of Sr^{90} contamination, the group also considered the current status of gonadal irradiation of the world population. The fallout gamma dose rate in the North Temperate Zone may be expected to double in the next few years due to the deposition of cesium 137 now stored in the stratosphere. Estimates based on current data taking into account both short-lived as well as long-lived debris are in agreement with the forecasts of the U.N. Scientific Committee on the Effects of Radiation. The worldwide external genetically significant dose, in agreement with the U.N. Scientific Committee, from short- and long-lived gamma emitters is calculated to be 10 millirad in 30 years. The North Temperate Zone gonadal dose may be taken as 2½ times the world average, or about 25 millirem in 30 years, and to this another 25 millirem in 30 years should be added for internal Cs^{137}.

Mr. RAMEY. Is this from tests to date? That is not on the assumption of any continued testing?

Dr. NEUMAN. That is correct. [Reading.]

The estimated doses to the populations in the immediate vicinity of the Nevada test site have been revised in testimony submitted to these hearings. These corrections do not significantly change the genetically significant dose to the population of the United States as a whole.

The detonations prior to 1954 were such that the stratospheric inventory in February 1954 may be assumed to have been small in relation to the debris already deposited. The doses observed by February 1954 therefore define the total to be expected from detonations up to that time. The upper limit of bone marrow dose from Sr^{90} was estimated to have been approximately 0.4 millirep per year. This is derived from the known Sr^{90} contamination of milk in northeastern United States as of February 1954.

The panel, as a group, felt that no single or simple relation between radiation dosage and biological effect has as yet been established, particularly at the low dosages to be expected from off-site fallout. Therefore, no attempt was made to convert the predicted concentrations of Cs^{137} and Sr^{90} into the number of tumors, cases of leukemia, genetic effects and the like. The biological effects, if any, to be expected from past tests would be manifest in a very small proportion to the world's population, but because the population is so large, the absolute numbers calculated under certain assumed relations appear startlingly large. It is useful, on an individual basis, to relate these predicted concentrations to radiation received from natural sources and to possible MPC's derived by suitably authorized committees or agencies. A comparison of fallout and natural background dose estimates appears as table IV.*

*For doubling the past 5-year testing pattern in the next 5 years, multiply the predicted values by 1.8; for indefinitely long repetition, multiply by 8.

(NOTE.—The final paragraph of the panel statement appears on p. 1803.)

(Table IV is as follows:)

TABLE IV.—*Comparison of worldwide fallout and natural background radiation dose estimates*

[Rem]

	Predicted by this panel: dose from testing to date	Natural radiation background [1]
70-year mean bone marrow dose	0.2–0.4	7
30-year genetically significant dose	0.05	3

[1] Report of United Nations Scientific Committee on the Effects of Atomic Radiation, ch. III, table V.

Dr. NEUMAN. Here the mean dose to marrow of testing to date predicted by this panel varies between two-tenths and four-tenths of a rem as compared with natural background radiation of 7 rem.

The 30-year genetically significant dose, 0.05 rem as compared to 3 for natural radiation background.

Mr. RAMEY. How would that compare with the population standard based on your strontium units of 66 which is the ICRP level?

Dr. NEUMAN. It is about one-tenth.

Mr. RAMEY. It would be around 7 strontium units. I believe Dr. Langam had some figure?

Dr. NEUMAN. Yes; about 7 as compared to 66 as now tentatively recommended figures. That is about a tenth.

Mr. RAMEY. That is based on testing to date. I believe you have a footnote on page 7 of your statement?

Dr. NEUMAN. I will read that footnote.

Mr. RAMEY. I think it ought to be discussed a good deal, too.

Mr. NEUMAN. I think I should say, in addition to this footnote, we should have had a little more qualification. In assuming the past 5-year testing pattern, it is necessary to assume not only the same megatonnage yield but also the same distribution of shots. A different distribution of explosions would give different residence times and different deposition patterns. It is so fanciful to try to predict the future test rate that we have to assume that we are duplicating past patterns.

Representative HOLIFIELD. Does that mean the test rate of the last 2 years or averaged since 1946?

Dr. NEUMAN. Averaged since 1953. The last 5 years. Under those circumstances, and with that kind of qualification for repeating the past 5-year testing pattern in the next 5 years, multiply the predicted values by 1.8. For an indefinitely long repetition of such test patterns—in other words, continued testing at the average 5-year rate—multiply by approximately 8.

Mr. RAMEY. Could you multiply those out for the record? We did this 2 years ago.

Dr. NEUMAN. Let us take the world average weighted figure. You want dosages or would you prefer to have it expressed in micromicrocuries?

Mr. RAMEY. I think strontium units would be most understandable.

Dr. NEUMAN. Mr. Hollister will put the figures on the board. The panel is not trying to dodge its responsibility. We appreciate the

fact that we were asked to make these predictions "in the event of continued testing." But it seemed somewhat unrealistic to presume that we were going to repeat the testing pattern (which is more or less happenstance) over and over and over again indefinitely. This seems to be rather unrealistic.

TABLE III OF PREDICTION PANEL STATEMENT AS MODIFIED FOR DISCUSSION

Predicted average Sr^{90} equilibrium bone levels corresponding to average maximum deposition from weapons tests to date

Region and type of population	μμc Sr^{90}/gram Ca		
	No more testing	One repetition of past 5-year testing pattern	Indefinite repetition of past 5-year testing pattern
United States of America, average	6	10.8	48
20°–60° N, average [1]	8	14.4	64
20°–60° N, Western civilization [2]	5	9.0	40
20°–60° N, Eastern civilization [3]	10	18.0	80
World average, weighted [1]	7	12.6	56

[1] Weighted by both population and diet.
[2] Milk constitutes major source of calcium in diet.
[3] Plants constitute major source of calcium in diet.

Mr. RAMEY. We just asked for a 4- or 5-year period and then any other estimates that might go beyond that 4- or 5-year period. The past pattern is the only pattern we have had; so obviously you could not be expected to go much beyond that.

Dr. NEUMAN. I am only speaking for myself now, but I think most of the committee members will agree that if you were to repeat and repeat and repeat, which is rather unlikely, you can see that we are approaching average levels in bone of 48, 64, 40, 80 and an overall world average of 56.

Representative HOLIFIELD. That is expressed in strontium units?

Dr. NEUMAN. Yes. Micromicrocuries per gram. Since the tentative maximum permissible level has been given as 66, this would be much too close in my opinion. I am not speaking for the panel.

Representative HOLIFIELD. Let us read this off for the record since you don't have a prepared chart that we can photograph.

Dr. NEUMAN. The predicted levels in strontium units in micromicrocuries of strontium 90 per gram of calcium: For the United States would be 10.8; for 20 to 60 degrees north, average 14.4; for 20 degrees to 60 degrees north, Western civilization, 9.0; for 20 degrees to 60 degrees north, eastern civilization, 18; for a weighted world average 12.6.

These numbers are based on the assumption of a repetition of the past testing pattern of 5 years. On the assumption of a continued testing indefinitely, these become the following: For the United States, 48; for 20 to 60 degrees north, 64; for 20 degrees to 60 degrees north, Western civilization, 40; for 20 degrees to 60 degrees north, Eastern civilization, 80; for a weighted world average, 56 micromicrocuries of strontium 90 per gram of calcium.

Representative HOLIFIELD. That is against the ICRP maximum permissible concentration of 66?

Dr. NEUMAN. So far, this is a suggestion. Is 66 a formally accepted figure for the population MPC?

Representative HOLIFIELD. For planning purposes.

Dr. NEUMAN. For planning purposes, because the NCRP subcommittee has not reported as I understand from testimony given by Dr. Crow yesterday.

Mr. RAMEY. This is the ICRP we are talking about. They said for future planning purposes.

Representative DURHAM. I think it should be made perfectly plain to the public, Mr. Chairman, that these numbers depend on doubling the amount and going on for 40, 50 or 100 years, isn't that correct?

Dr. NEUMAN. This would be at least 40 years of continued tests of the same pattern as the last 5 years.

Representative DURHAM. The public should understand that these levels do not exist today.

Dr. NEUMAN. No, sir.

Representative DURHAM. It doesn't exist today.

Mr. RAMEY. The present amount in the United States is 6 strontium units.

Dr. NEUMAN. Not the present amount. Two S.U. is the highest average presently observed. Most people think I am on the pessimistic side but, actually, if tests stay stopped, as it were, I don't believe we will really see these predicted values in more than a fraction of the total population.

Representative VAN ZANDT. Could you say it this way: These figures obtained by multiplying table III by 1.8 or by 8 have no value unless tests are resumed?

Dr. NEUMAN. Only the numbers that appear in our statement, table III, have any validity as predictions if tests are not resumed.

Mr. RAMEY. Unless atmospheric tests are resumed. As later testimony will show, you can have underground tests without any contamination?

Dr. NEUMAN. Yes, sir. We had to make the assumption that the test pattern was the same as in the last 5 years. That is the qualification I underlined.

Dr. TOMPKINS. Wouldn't you have to extend the statement of atmospheric to megaton yield of atmospheric testing to make these figures apply?

Dr. LIBBY. Everything has to be done exactly as it was before?

Dr. NEUMAN. That is the requirement in our assumption.

Representative HOLIFIELD. This is following the same type of testing, the same pattern of weapons that we have followed in the past in order to make this prediction. It is the only standard we have to go by.

Dr. NEUMAN. It is the only one we have. Otherwise we would have an infinite number of predictions.

Mr. RAMEY. But does it include the last Russian tests?

Dr. SHELTON. Not only do they include the last Russian tests, the first column includes the deposition on the ground of all the material in the stratosphere at this time. These are the average results expected at the time of maximum deposition as a result of testing to date.

Representative HOLIFIELD. Then this would actually be predicting the United States of America level of six ahead until possibly 1962?

Dr. SHELTON. 1962, 1963, or 1965. This is as high as I expect to reach as a result of testing today. As Dr. Neuman said I don't actually expect to see these levels come about.

Representative HOLIFIELD. We can assume that if all testing ceases that your maximum amount would be the first column there?

Dr. SHELTON. That is correct.

Representative HOLIFIELD. So the heading there, testing to date, is correct but it is misleading unless you pin to that, that the projection is into the future for the testing up to date?

Dr. NEUMAN. May I say just a word. The title of that table in the report reads "Predicted Average Strontium 90 Equilibrium Bone Levels" corresponding to the average at the time of deposition maximum. That word "equilibrium" has a very important qualification. Obviously, adults will not be in "equilibrium" and only a certain fraction of the young people will achieve equilibrium.

Mr. HOLLISTER. So is the word "average" important. The panel, if I can speak for them, is not supposing that no individual will be above six.

Senator GORE. I was going to inquire, Mr. Chairman, if these figures refer to strontium only, or do they include cesium?

Dr. NEUMAN. This particular table refers to strontium only, because cesium does not concentrate in bone. The gonadal dose is principally related to cesium because cesium is a soft tissue component and strontium 90 does not cause a major problem in terms of gonadal radiation.

Senator GORE. My real point, Mr. Chairman, was to inquire if the panel was to cover the possible hazards from elements other than strontium.

Mr. RAMEY. Dr. Neuman, in your table, don't you show the effects of cesium and so on in the 30-year genetically significant dose?

Dr. NEUMAN. Yes, that is, the 30-year genetically significant dose in table IV that is contributed principally by cesium.

Dr. SHELTON. I would recommend, Mr. Chairman, that we go ahead and put the same kind of numbers for cesium following the last 5-year pattern of testing.

Representative HOLIFIELD. That is fine. We have to keep reminding ourselves of the difference between the strontium 90 and the genetically significant dose from other types of radiation. At least I do.

Dr. NEUMAN. I will speak for myself and I would like to see some nods from other committee members. In multiplying simply by these convenient numbers, the assumption is being made that the cumulative dose from cesium will be strictly cumulative. There is plenty of evidence to indicate that it is not proportional to deposition level but rather is dependent on the rate of fallout.

If that is the case, these numbers (at least those from internal irradiation) should not be multiplied by much of anything.

Mr. EISENBUD. If I understood our discussion of the other evening, in order to extrapolate the gonadal dose we would be safe in assuming that component of the gonadal dose which is due to cesium already inside the body has already reached equilibrium and will not increase

from continued testing, or for that matter from continued fallout. So that the 25 milirem in 30 years due to the internal irradiation of cesium in the body should not change, whereas the 25 milirem in 30 years from cesium on the ground will build up approximately as a linear function of the testing rates and approximately by the same factors that we use for calculating strontium 90.

Mr. RAMEY. How do you get a relationship between this cesium dose, or the dose of your various other radioisotopes, in relation to a maximum permissible dose? I believe in your table you refer to the natural background showing what that is. Would the natural background constitute your standard or your measure?

Dr. LIBBY. In table IV you have three units for the natural background for the 30-year genetically significant dose and from past tests we have 0.05 units. If we double we have .09 units, which would be about 3 percent.

If we continued forever, then in about 50 or 100 years it would reach 0.4 which is about 12 percent of the natural background dose. (See p. 1795).

Representative HOLIFIELD. 12 percent of the background dose?

Dr. LIBBY. That is right.

Representative HOLIFIELD. Not of the permissible concentration?

Dr. LIBBY. No. This is the background dose.

Mr. RAMEY. What is the measure for this? We had the 66 strontium units as your measure on strontium 90. What is the other measure to compare that to?

Dr. SHELTON. I believe the previous testimony on genetics indicated that we would regard the natural background as a datum point and using that as a measure, we do not want too much more than perhaps double the natural background or maybe in units of tenths of the background value.

In other words, whether it is a tenth of a natural background or equal to the natural background. That is the basis.

Mr. RAMEY. In this situation, then, on cesium, continuing your testing indefinitely following the past pattern, would approximate 10 to 15 percent of the natural background. In that situation, it doesn't look as difficult as your strontium in the bone situation.

Dr. NEUMAN. Again, I don't know if the rest of the committee would agree, but I think these figures are maximizing the effects of cesium 137 as we have indicated and for the reasons indicated.

However, if tests resume and continue and debris has a short residence time in the stratosphere, we will have to take into account shorter lived elements which we are not taking into account in this calculation. Only in the framework that tests have ceased, is cesium the only gamma emiter with which we need to concern ourselves.

Mr. RAMEY. In that connection we want to ask about radioiodine. We had reference to two papers by Dr. Lewis and Dr. Wolff on the concentration of this short-lived isotope in the thyroid that might be approaching near your background levels in one location in the body. Is that of any significance?

Dr. NEUMAN. Yes, sir. The short-lived isotopes are all dependent on test rate. With zero test rate we can eliminate them from our consideration and consider only the long-lived isotopes such as cesium

and strontium. Again this is one of the difficulties in predicting when we can't predict the test rate.

Dr. TOMPKINS. Dr. Neuman, can you also ignore as a function of test rate on your eight times repetition. In this case the new material is coming in at a certain rate. Will these relationships hold in your opinion?

Dr. NEUMAN. I thought I made abundantly clear, the 1.8 are based on the assumption that the last 5-year tests are repeated in the same pattern and total yield.

Dr. TOMPKINS. This is not my point. I want to be sure that considerations are brought out. The 1.8 is in direct ratio of the fission yield; is that right?

Dr. NEUMAN. No. You are putting an equal amount into the stratosphere from the last 5 years and then decay about 10 percent (2½ percent a year for 5 years).

Dr. TOMPKINS. Your numbers are based on the predominantly long-lived material?

Dr. NEUMAN. These are entirely based on long-lived and I made that as a preliminary statement.

Dr. TOMPKINS. The reason why you can afford to eliminate considerations of barium 140, strontium 89, iodine 131 is the fact that the decay is rapid. Therefore, the total 30-year dose or the 70-year dose does not build up. Whatever you are going to get, you get rapidly and then it disappears. When you repeat this eight times you are putting your rapidly disappearing material in eight times. So a comparison of the short-range dose, essentially on a per year basis which is the proper relationship, is this approximately the same as the assumption you have made here, is my question.

In other words, can you still include that and consider the difference negligible enough to ignore.

Dr. SELOVE. May I say there is no essential difference in the relative effect of short-lived and long-lived isotopes for different testing rates. The reason is this. If you conduct one series of tests or have one explosion, then there will be some short-lived isotopes which deliver their radiation within a certain short period of time and some long-lived isotopes which deliver their radiation over a longer period of time. Whether you hold the next test series the next month or 5 years later, the ratio of radiation dose integrated over a long period that comes from the short-lived and the long-lived isotopes is the same independent of what the space in between test series was.

Dr. SHELTON. I believe the answer to your question is this: We have taken that into account. Because the numbers based on the 5-year past history included whatever happens to be in the short-lived isotope situation.

Representative HOLIFIELD. I want to again say if any member of the panel wishes to clarify or modify or object to any statement and propound their position they have the opportunity by raising their hands. This gentleman has raised his hand.

Mr. TERRILL. I am Terrill with the Public Health Service. I don't recall in any of our deliberations that we took iodine into consideration in the manner reported by Dr. Shelton. I don't mean to emphasize any disagreement with what is stated here, but we approached this on the basis of a certain model. If we inject quickly the short

isotope situation into this model, we are liable to say things that we will have to correct after the sessions are over.

The other point I would like to make is that these are all averages. They are not maximums. They are the averages at the time of maximum deposition. The key to this and the thing that is going to make it difficult for all of us are the variations.

In carrying out the arithmetic of comparing our readings in milk, water, food, or bone, with the maximum permissible levels, these variations are going to cause difficulties. Because, as you can see, the variability between these numbers and assumptions is such that even if we are absolutely conscientious we are going to end up with a span of numbers that are going to be on both sides of MPC's as they are now listed in the International Committee's recommendations. We have to keep this in mind.

Dr. SHELTON. If we continue testing indefinitely at the rate of the past 5 years only.

Mr. TERRILL. Sir, I think that we are going to find situations when we make allowances for all the radiation affecting bone, both strontium 89, strontium 90, and external radiation and when we follow the arithmetic, as outlined in the handbooks where we will have now exceeded MPC's momentarily. Unless we have the resources to average these things and actually average the low levels as well as the high values, we are going to be in difficulty.

It is going to be a very difficult thing to tell people how to adjust these averages in individual situations.

Dr. TOMPKINS. Mr. Terrill, I believe you are referring to the problem of actually applying this kind of result to a practical situation.

Mr. TERRILL. That is precisely the case.

Dr. TOMPKINS. The connotation of these particular tables was to give a base to assess the characteristics and magnitude of a problem from the standpoint of what it is likely to look like. You are bringing up the very real point that you have added to this the proposition of applying this on a continuous basis in the future.

This runs you into problems which are not really highlighted by this method of forecasting. Isn't that what you are saying?

Mr. TERRILL. Right to the point of our statement, I think if we quickly inject any new isotopes into this model that we have agreed to select for the purposes of this statement, we are almost bound to change the numbers in a way that would be unfortunate. I think we would have to make corrections—all of us would have to make corrections in the statement—after these hearings are over. We would quickly say things that we would not want to have in the record. It is too complicated for this type of juggling of the numbers on a few minutes' notice.

Dr. TOMPKINS. What you are saying is that you feel that more attention needs to be given to these widely varying shorter lived isotopes?

Mr. TERRILL. Yes, sir. I don't think we ought to quickly inject iodine into this picture because we have to consider the physiological relationships between relatively high iodine doses and relatively high bone doses. That is probably more important than the concentrations themselves.

Dr. TOMPKINS. I want to be sure I understand what you are saying, Mr. Terrill, in raising the question of iodine that you are questioning the basic assumption which Dr. Neuman stated, that the treatment of strontium 90 and cesium 137 would cover the predominant risk, and all others would be less.

Are you questioning that?

Mr. TERRILL. No, sir. I am not questioning our model as we drew it up and worked over it together. I am questioning first my capability to inject a total new set of circumstances into this model and quickly report to a congressional committee on the effects of all these additions.

Representative HOLIFIELD. Dr. Selove has had his hand up.

Dr. SELOVE. May I make one comment on the relative effect of short-lived and long-lived isotopes. The integrated dose received by the entire population of the United States or of the world from the long-lived isotopes is greater, considerably greater, than the integrated dose received from short-lived isotopes such as iodine. On the other hand, the integrated dose from strontium 90 is spread over 40 years and the amount received in any 1 year is correspondingly smaller. The integrated dose from iodine is all received in a very short time, a matter of a few days.

So the particular individuals who get that iodine have all of the iodine dose from a given test explosion. It is not averaged over all of the people in the same way that the long-lived effects are.

Representative DURHAM. Following up the doctor's statement, has there been any thought given by the panel to setting up a model for short-lived radiation material or radioisotopes?

Dr. SELOVE. I am afraid not. There simply was not time.

Representative DURHAM. You seem to want to separate the two?

Dr. NEUMAN. I think this underlines one of the problems that has been with us from the year 1, so to speak: The divergence of opinion among scientists. We carefully defined a frame of reference. It was done in the setting of a cessation of tests. In this setting and within this frame of reference, all of the scientists are essentially in agreement. We are in accord.

If you don't describe and don't define the limits from which you are doing your predicting then everybody comes out with a different prediction it is said "there is a big controversy raging." It seems to me the whole situation can be given in an analogy. We are playing with cosmic forces and we are doing it in a microcosmic political situation, so to speak. Some of us think that we are lifting the lid on Pandora's box.

Representative DURHAM. I agree with that.

Dr. NEUMAN. Some of us look at this and say, "The lid is not up, so why worry?" But others are alarmed at the process. Here, today, we are in the position of trying to define whether the lid is "up" or "down." Obviously we can't because one can't describe a dynamic process in static terms.

Representative DURHAM. The question of whether the lid is up or down depends to a large degree on the circumstances and the continuation of what we have been doing for the past few years. Wouldn't you say that, Doctor?

You can put the lid down now. We know pretty well what is in the atmosphere?

Dr. NEUMAN. Nothing much has come out from the lifting of the lid thus far. I think we are all in agreement that from tests to date, the hazards are extremely small relative to other hazards that we take every day. I think everyone is in agreement.

Representative DURHAM. I think the table definitely shows that.

Dr. NEUMAN. Yes. I think it also shows if we leave testing uncontrolled and ignore these probabilities, we can very easily get in serious trouble.

Representative HOLIFIELD. Mr. Eisenbud?

Mr. EISENBUD. I thought I might emphasize at this point that we did what other groups, including the National Academy and the United Nations Committee have done. We have considered the two isotopes and the two situations which seemed to be of greatest concern; namely, the gonadal dose from the gamma emitters and the bone dose from strontium 90. Iodine is an additional problem but is not a problem which has its primary effect on either the gonadal tissue or the skeleton tissue. It affects thyroid and has been thoroughly discussed.

Representative HOLIFIELD. It is mainly concentrated in the thyroid gland?

Mr. EISENBUD. Yes, sir.

Representative HOLIFIELD. And therefore it has a very limited local application?

Mr. EISENBUD. Yes.

Representative HOLIFIELD. In consideration to the whole body dose?

Mr. EISENBUD. The same type of study could be made with respect to the dose from iodine and the dose to the thyroids of the people.

Representative HOLIFIELD. We didn't ask you to do that.

Dr. Neuman, I think you have one more paragraph to read.

Dr. NEUMAN. Yes, I have one more paragraph to read. We could not resist patting ourselves on the back. This is not a proper way to end. [Reading:]

We find the predictions made by the 1957 panel with respect to maximum concentrations, if past tests or equivalent were repeated once, are consistent with predictions made today. It is noted that prior to 1957 approximately 50 MT fission yield equivalent had been detonated, and since that time approximately 40 MT more.

Submitted by:

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Lester Machta, Weather Bureau
William Neuman, University of Rochester
Walter Selove, University of Pennsylvania
Frank Shelton, AFSWP, DOD
James Terrill, USPHS

Representative HOLIFIELD. For the record we will put in the material that you referred to which is on page 16 of our analysis of 2 years ago at this point for comparison.

Dr. NEUMAN. Thank you.

(The material referred to follows:)

At the invitation of AEC's Division of Biology and Medicine, a meeting was held on July 29, 1957. Principal participants were Mr. Eisenbud, Dr. Kulp, Dr. Langham, Dr. Libby, Dr. Machta, Dr. Neuman, and Dr. Selove. Also present were Dr. Dunham, Dr. Alexander, Mr. Hollister, Dr. Calvin J. Potts, Dr. Reitemeier, and Dr. Western. A written summary of the meeting appears in the print of the hearings. Three forecasts were made at the meeting, all predicting what the future human skeleton levels of strontium 90 in young persons might be in the Northeastern United States under various conditions.

The basis of these forecasts is an assumption that an average value of 0.8 strontium unit, based on Dr. Kulp's measurements of strontium 90 levels in the skeletons of children in the Northeastern United States, as of the fall of 1956, represents a reasonable point from which to extrapolate. Then the following three forecasts were developed:

1. *Forecast.*—The future skeletal concentrations in young persons in the Northeastern United States *resulting from the strontium 90 already on the ground before 1957* are predicted to fall within a range of 1.5 to 2 strontium units.

2. *Forecast.*—The future skeletal concentrations in young persons in the Northeastern United States *resulting from the strontium 90 produced before 1957* are predicted to fall within a range as follows:

(*a*) 1.5–3.5 strontium units if stratospheric fallout is uniform;

(*b*) 2–5 strontium units if existing fallout pattern is maintained;

(*c*) 4–10 strontium units if predicted increase in "banding" of stratospheric fallout in latitudes of Northeastern United States occurs.

3. *Forecast.*—The future skeletal concentrations in young persons in the Northeastern United States *resulting from all strontium 90 produced before 1957 plus an equal amount produced in the next several years from a repetition of past tests* (or equivalent) are predicted to fall within a range as follows:

(*a*) 3.5–9 strontium units if stratospheric fallout is uniform;

(*b*) 5–12 strontium units if existing fallout pattern is maintained;

(*c*) 10–25 strontium units if predicted increase in "banding" of stratospheric fallout in latitudes of Northeastern United States occurs.

4. *Forecast.*—The future skeletal concentrations resulting from indefinitely continued testing (often approximated by taking 100 years) at the average annual rate of the past 5 years (10 megatons equivalent fission yield per year) were *not* forecasted, nor were the effects of an increasing rate of testing considered.

The forecasts that were made (for the Northern United States) show that if the amount of fission products put into the atmosphere doubles in the next 10 years or so the bone levels should somewhat more than double. Local fallout, of course, is not included.

Note that the units used are "strontium units," defined on page 9. The maximum permissible concentration of strontium 90, it will be recalled, is 100 strontium units for a population—subject, as stated above, to serious questions and limitations. Before any of the forecasts above, or those contained in the hearings, are compared with 100 strontium units, or some other standard, it should be kept in mind that some of the forecasts assume uniformity of stratospheric fallout distribution. If, as the hearings strongly indicate, this assumption is incorrect, then the forecasts need adjustment before a comparison is made.

It is to be noted that this group did not go as far into the future, in making forecasts, as some of the individuals did.

Representative HOLIFIELD. Before you go any further, Mr. Hollister, you placed on the board an outline. Would you like to comment on that and explain it to the committee?

Mr. HOLLISTER. This is a grossly oversimplified but pictorial way of presenting this short- versus long-lived emitter problem that the panel has been referring to. What I have on the board is a graph of

dose *rate* versus time for two hypothetical isotopes—a long-lived emitter and a short-lived emitter—and for the purposes of illustration we will assume that the biochemical nature of the long-lived and short-lived emitters is similar.

We will assume therefore that the short-lived emitter and long-lived emitter go to the same part of the body and behave chemically and metabolically in the same fashion. Then the shaded area at the left would represent—the *area*, not the height—the dose to an organ from the short-lived emitter from the past testing to date. If the testing pattern of the past 5 years is repeated once, then the second shaded area on the right would represent a repetition of that dose from the same short-lived emitter.

Then the crosshatched area under the lower, almost horizontal curve, starting way over at the left, would represent the dose from the long-lived emitter from the past testing to date. Notice that the dose from the long-lived emitter results from a dose *rate* that extends far beyond the 5-year period of testing. Then the second cross-hatched area starting in the middle represents the dose from the long-lived emitter from the first repetition of the testing pattern.

The point is that if we go on and repeat the testing pattern time after time, you repeat the little shaded area every time but the little shaded areas don't add vertically upon each other. Thus, the dose *rate* is not cumulative though the dose is. But the second cross-hatched area, you notice, is superimposed upon the first cross-hatched area. If I were to repeat for a third time I would get another cross-hatched area 5 years further along superimposed on the cross-hatched area that I have there and this would keep building up. Thus, the dose *rate* is cumulative. But it does not keep building up an indefinite number of times because of decay. The footnote in the panel's statement says for an indefinite number of repetitions you still multiply by 8—you don't multiply by an indefinite number. This is because of the fact that as you go along building up and building up activity the decay becomes more important. Finally you reach a point of testing equilibrium or balance of the sort talked about 2 years ago, when the decay in the environment is exactly compensating for the injection.

This is grossly oversimplified. In actual fact the dose from the short-lived emitters may or may not go to the same part of the body as from the long-lived emitters. There may be several short-lived emitters to be compared with one or two long-lived emitters. This is to be understood to be oversimplified. It gives the idea.

Representative HOLIFIELD. Even though it is oversimplified it has cleared up in my mind considerably the point of the short-lived isotope and its effect on the human body.

I would ask you to prepare for the record a proper chart of this kind so it can be in the printed record and also the table of figures which you had before because we have referred to them.

Mr. HOLLISTER. I would be happy to.

(The material referred to follows:)

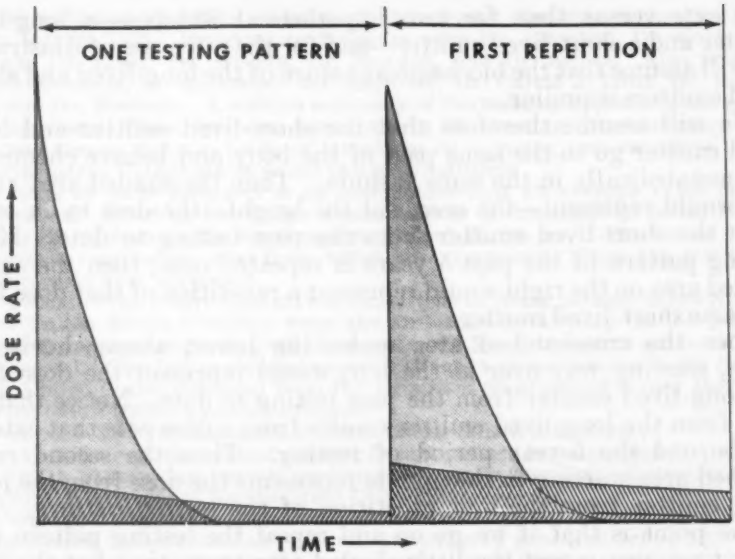

ILLUSTRATION OF DOSE RATE AND TOTAL DOSE FROM SHORT-LIVED AND LONG-LIVED ISOTOPES

Assumption: the two isotopes are chemically and biologically identical in behavior, differing only in radioactive decay lifetime.

DISCUSSION OF DOSAGE FROM SHORT-LIVED ISOTOPES IN WORLDWIDE FALLOUT
SUBMITTED BY AEC

In considering the so-called short-lived isotopes problem in worldwide fallout, two points are to be kept in mind:

(1) In order for "short-lived" isotopes even to exist in worldwide fallout, their radioactive lifetimes must be "long enough"—taking into account their initial production abundances in fission—so that the material would not have disappeared before falling out as worldwide fallout. And, of course, further disappearance takes place before an internal emitter of short lifetime could be deposited in the body.

(2) As the chart explains in a simplified way, doubling or repeating the past 5 to 6 year testing pattern will, other things being equal, of course double the total dose from both short- and long-lived isotopes. But, as the chart illustrates, the dose rates does not cumulate for the short-lived isotopes but does cumulate for long-lived isotopes.

If testing stops, the dose rate from short-lived isotopes drops soon afterward. For example, 1 year would represent about seven half lives of Sr^{89} reducing the amount present at the beginning of the year to one one hundred twenty-eighth, or less than 1 percent. On the other hand, for long-lived isotopes such as Sr^{90}, 1 year represents only about 2 to 3 percent reduction in the initial amount, so that if testing stops the dose rate continues high for an extended period afterward.

Now to consider three specific problems involving short-lived isotopes, consider Sr^{89}, $Zr^{95}Nb^{95}$, and I^{131}:

(1) For equal activities of Sr^{89} and Sr^{90} (that is, equal numbers of microcuries) initially deposited in bone, the final dose from Sr^{90} is about 100 times that from Sr^{89}. Conversely, for the dose to be equal, about 100 times the initial activity of Sr^{90} would have to be present as Sr^{89} activity. It is to be kept in mind, of course, that the total dose received from Sr^{89} would be received in a much shorter length of time.

Transient levels of Sr^{89} on the order of 100 times the activity levels of Sr^{90} have been observed in milk. For the year ending February 1959, average Sr^{89}/Sr^{90} levels in milk ranged from 5 to 9 for the five Public Health Service stations for which records exist. (An initial ratio of 10 in bone would give a Sr^{89} dose of about 10 percent of the Sr^{90} dose.) The PHS data also indicate that in February 1959 (a minimum of 3 months after the last weapons test) the Sr^{89}/Sr^{90} ratios in milk for 10 PHS stations ranged from 1 to 9.

(2) Data submitted by the Argonne National Laboratory as an attachment to Dr. Brues' testimony suggests dose rates from Zr^{95}–Nb^{95} deposited on the ground of from 4 to 16 times those from Cs^{137}. Because the half lives of Zr^{95} and Nb^{95}

are short compared to that of Cs^{137}, relative concentrations in the soil reflect rates of fallout and are subject to large variations. We have insufficient data on the migration of these materials into the soil to make reliable estimates of total external dose from these data. However, if one computes such doses on the assumption that the materials remain on the surface of the ground until radioactive decay is complete, the ratio of the total dose from the short-lived Zr^{95}–Nb^{95} to that from the long-lived Cs^{137} will be much smaller.

(3) PHS data gives the result that for the year ending February 1959 for five PHS stations, the average I^{131} content of the milk sampled ranged from 28 micromicrocuries per liter at New York and Salt Lake City to 83 at St. Louis.

If one consumed 1 quart a day of milk for a whole year, all at 83 micromicrocuries per liter, one would have a total year's intake of about 30,000 micromicrocuries of I^{131}. Of this, about one-fifth, or 6,000 micromicrocuries of I^{131} would be expected to go to the thyroid. The dose to an adult (20 gram) thyroid would then be about 37 millirads for the year's intake. In the case of children, because the thyroid is much smaller and the uptake of iodine higher, the radiation dose to the thyroid may be from 10 to 15 times as high.

Of course, some I^{131} might be taken up from other foods, and this additional amount would have to be added. Not much is known about the size of the I^{131} contribution from food, but it is not expected to be as great as from milk.

It is to be noted that the iodine levels fluctuate greatly, so that yearly averaging is useful only for rough estimates. For example, the 12 monthly milk samples from St. Louis that average out to 83 include 1 sample of 624 and 4 samples of zero.

Representative VAN ZANDT. May I ask a question about table III? I do this to clear up a point in my mind. Mr. Ramey, I think, made the exception of underground bursts. What about the bursts in the high altitude? Do we have instrumentation that would detect those bursts or without instrumentation will the effect of those bursts show up on the findings such as are being reported during these hearings?

Dr. LIBBY. We are going to have a short statement on this in a few minutes, Mr. Van Zandt. We draw a distinction between high altitude in the atmosphere and high altitude in outer space.

Representative VAN ZANDT. I am concerned about outer space. Very well.

Dr. SHELTON. I realize that as time goes on certain scientists are going to look at our two figures. I feel the text is quite adequate for a complete interpretation of the two figures. The two figures of the text are extremely useful if one knows what they mean.

Figure 1 illustrates the amount of strontium 90 in the stratosphere as it existed in mid-1958. Notice that it has a bump in the high north latitude and there is a bump around the Equator and a small bump in the Southern Hemisphere. The microcuries per square mile on the lefthand side means that if the strontium 90 in the stratosphere were brought down vertically to the ground and deposited in a vertical manner—if we could precipitate it in some way and just bring it down as it exists in its distribution in the stratosphere—those would be the microcuries per square mile on the ground.

I say again the figure is if it came down vertically from where it is located. That brings us to figure 2.

It doesn't come down vertically. So no man should take figure 1 and say if it all came down we would have such and such, like 12 microcuries per square mile at the Equator. The bump on figure 1 at the Equator does not come down vertically. There is the mechanism by which the material in the stratosphere comes out through the breaks in the tropopauses and locates principally in the north and south temperate latitudes. So the material in the stratosphere given by figure 1 is deposited on the ground really, as illustrated by figure 2.

The amount that comes at the Equator, for instance, is very small. One cannot take figure 1 and drop its values down to the ground.

He must go through the fallout mechanisms to get it on the ground. That is what we did in our text.

Representative DURHAM. Could we extrapolate what would happen if it all came down at one time?

Dr. SHELTON. We did bring it all down; yes, sir. But we distributed it on the ground by the regular mechanisms by which it does come down.

Representative DURHAM. Over the years. You brought it down on the basis over the years?

Dr. SHELTON. We brought it down over a period of time, that is correct. It just is not going to come down all at once.

Representative DURHAM. No, I hope not.

Mr. RAMEY. In inserting into the record the results of the seminar held after our hearings in 1957 there were alternatives put in on what would happen in 5 years. I wanted to ask Dr. Machta if it didn't appear that his alternative, assuming banding, is really the one that is coming out now. I gather from the report of the panel that this assumption or theory or model, as you call it, is the one that is the more appropriate model for our future predictions.

Dr. MACHTA. Yes. The panel did assume that all of the debris from polar injections is coming out in the north temperate latitude, banded as the material in the past was banded. We did not completely agree on what would happen with the debris already in the stratosphere, but it makes little difference. I should point out in the 1957 testimony I made the prediction that if we injected approximately 10 megatons equivalent into the stratosphere each year and looked for the highest concentration in the north temperate latitudes in the relatively small area of a few States, one would obtain a fallout amount on the ground which could be as low as 350 millicuries per square mile, and perhaps as high as 850 millicuries per square mile at equilibrium.

From information consistent with the data used by the committee and from what we find in the United States at the present time, I think I would now predict a value of about 800 millicuries per square mile. This is close to the upper bound which was predicted 2 years ago.

Mr. RAMEY. That appears at pages 1336 and 1337 of the 1957 hearings. If that were extrapolated into strontium units for that area, that would be a rather high rate, would it not?

Dr. MACHTA. No, not particularly high. We are actually using 650 millicuries per square mile for our prediction. So it is not very much higher.

(Extracts from 1957 hearings (pp. 1336–1337) referred to above follow:)

Senator ANDERSON. Dr. Langham, I wonder if you would like to run through that table now of the calculations showing strontium 90 in the bone in relation to maximum permissible concentration?

Dr. LANGHAM. This is a repeat of numbers in essence that were placed before you last week. What it amounts to is taking the various estimates of present levels in the bone, or present levels in the soil, and applying ecological discrimination factors, and estimating what the equilibrium level will be when our entire environment is in equilibrium with the present test rates, assuming 10 megatons of fission yield per year injected into the biosphere.

All this amounts to is taking the various estimates of present bone and soil levels and applying Dr. Libby's factor of 8, which is about what these levels will

be increased by when equilibrium is reached, assuming that we can continue to test at 10 megatons fission yield per year.

First, we can take Dr. Libby's estimate given in his recent speech, in which he estimated that the United States or rather the north temperature population belt, would reach equilibrium bone levels of somewhere between 5 and 20 sunshine units. These values were derived by assuming different ecological discrimination factors: 1 in the region that Dr. Neuman is talking about, and 1 in the region which Dr. Libby has talked about on occasion, up around 80.

If we take Dr. Kulp's estimate of two for the equilibrium bone level assuming no more tests, which, of course, is for the general population of the latitudinal belt, and multiply it by 8, we obtain an equilibrium bone level of 16 for continued testing at the present rate.

I have taken Kulp's bone data and applied certain corrections that I think refine it to some degree.

Mr. HOLLISTER. At the present rate of testing, and assuming the rate continues, or at the present rate of testing, assuming no more testing?

Dr. LANHAM. These are numbers assuming we continue to test at the equivalent of 10 megatons of fission products injected into the biosphere per year until we reach equilibrium. This is until the decay of strontium is equal to the amount we are putting up.

So we have 5 to 20 sunshine units by Dr. Libby. Dr. Kulp's numbers give 16.

Our numbers from Dr. Kulp's bone data is 25.

Mr. Eisenbud, in this meeting, estimated about 40 from New York milk data.

My estimate on the basis of other milk data given an equilibrium bone level of 29.

If we take the soil data, which is the 22 millicuries per square mile that Dr. Libby has estimated at present, multiply by 1.8 to give sunshine units in the soil, and apply a discrimination factor of 10 in going from soil to bone, we come out with 42.

Now, Dr. Machta's numbers that he just quoted are handled in this fashion. That is, we take the soil numbers he gave, multiply them by about 2 (1.8 to be exact) to get them to sunshine units in the soil, and then take one-tenth of that, assuming a discrimination factor of 10, and we come out with an estimate of from 70 to 170 sunshine units ($\mu\mu c$ Sr^{90}/g bone Ca) as the equilibrium bone level that will be reached if biospheric contamination continues at the present rate.

Mr. HOLLISTER. Which soil data are you taking now?

Dr. LANGHAM. The ones Dr. Machta just gave as the equilibrium level with continued testing (350–850mc/mi^2). Is that not right?

Dr. MACHTA. Yes.

Dr. LANGHAM. The ones he just gave would predict, then, for the United States on this basis, assuming a discrimination factor of 10, 70 to 170 sunshine units in bone at equilibrium.

Now, the maximum permissible level that we have been talking about is 100 of these units. So I think you can see that at the present rate of testing, if we assume all numbers other than Dr. Machta's are correct, the average is a factor of 3 below what we have accepted as a maximum permissible level, meaning that we can assume that some people will be 3 times the average value, and still not exceed what we have set as a maximum permissible level.

If we take Dr. Machta's number of 70 sunshine units which is his best or average number—is it not—for this?

Senator ANDERSON. The lowest number.

Dr. LANGHAM. The lowest number. Then our present test rates would not allow a factor of three safety. If we take his most pessimistic guess, then our present test rates could not be permitted.

Senator ANDERSON. Actually, Dr. Neuman's testimony of 2.2 megatons of fission products to be somewhat safe—I hope I do not misstate you, Doctor—would indicate also we would be a little bit above what he regarded as a safe level. I should not have said it. Maybe you should.

Dr. NEUMAN. I made two calculations, one using the MPC of 50, and one using the MPC of 100. Using the 100 value, I came out with a maximum equilibrium test rate of 4.4 megatons of fission products injected anually into the stratosphere. But in that calculation I assumed Campbell's model, and 20 percent overall decay due to the reservoir, and I used a factor only of 2 to take care of the variation in individuals, but I used a factor of 3 for nonuniformity.

So, in a sense, my calculation really anticipated Dr. Machta's, although I did not know I was anticipating it at the time I made it.

Senator ANDERSON. It is very interesting that the two worked out as they did.

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(Extract from 1957 hearings, pp. 1341–1342)

Dr. MACHTA. My numbers on the board obviously stick out like a sore thumb. The reason lies partly in the factor of 8 used to increase the other values. The aging factor that Dr. Libby just indicated as being included in the 8—when you put that in my figures, they would be reduced to the same order of magnitude as the others. So those numbers are not comparable. If you do not take into account Libby's aging factors, and if you make the assumption, for example, that half of what fell out in the United States is tropospheric, and half stratospheric, then instead of a factor of 8 you get something like 14 for the conversion of the 5-year fallout to equilibrium fallout.

Further, if you assume what came out of Castle is only 3 years old instead of 5 years old, the number 114, becomes even higher.

So this is the reason for the difference. Dr. Libby has incorporated in the 8 other factors. These and other differences should be included in the 70 and 170, and the changes will bring my answers down to the others. So as now written they are not comparable and I do not wish the record to show this comparison.

Dr. LIBBY. Should they not be still higher?

Dr. MACHTA. No, I do not think the answers will come out higher if you take into account the aging and other factors.

Mr. HOLLISTER. Do we not all agree on the discrimination factors? So there is only the question of uniformity and nonuniformity, and the upper limit would have to be higher; isn't that so?

Dr. MACHTA. The upper limit, yes.

Representative HOLIFIELD. You finished your statement, Dr. Neuman. I think at this time we will have a short statement by Dr. Libby, and then we will have the whole matter before us.

Dr. LIBBY. I have been asked, Mr. Holifield, to make a short technical statement on the ways of reducing worldwide or off-site fallout from testing at a given level. I will not attempt to be strictly quantitative, but will give general ranges coupling the expected fallout with detonation conditions and will estimate the quantities of fission products available from deposition from alternative methods of testing.

There are two methods of reducing the off-site worldwide fallout below the 100 percent value which holds for atmospheric tests. It is important to emphasize that the atmospheric tests, as far as we know, give all of their fallout offsite and worldwide. These two methods have been used and investigated by the AEC and the Department of Defense.

The most effective of these is underground testing at such depths and in such rock formations as will completely contain and seal in all of the radioactivity produced.

As you know, we have conducted a total of five nuclear shots to investigate and us this technique. We have done enough to know that such underground confinement is possible and is a way to conduct lower yield shots free of offsite fallout or free of any fallout.

It is necessary to emphasize that our experience is limited and that we have to learn more about such matters as the characteristics of the rock necessary to completely seal in the radioactive fallout so that it cannot contaminate ground waters or escape to the surface. The whole technique is very promising, and we believe most of the Nevada-type shots can be made in this way.

The second method we know about and have experience with is firing on the surface of the land or the ocean in remote areas of the Pacific so that much of the fallout will drop back into the ocean. We believe that surface land firing retains about 80 percent of the fallout on the site. So if we fire on coral atolls, only about 20 percent of the fallout is dispersed worldwide, the remainder falling out in the ocean. Surface ocean shots appear to release something between the 30-percent figure that the Department of Defense has used in its calculations and the 80-percent figure I have used. Perhaps the best figure may be something like 50 percent. But in any case, a considerable amount of the fallout will fall back into the ocean in the case of detonations on the ocean barges in the remote areas of the equatorial Pacific.

There are further developments of the surface firing technique which may be helpful. For example, from Operation Hardtack last year we put silica sand in the barges in the hope that the fallout would be made insoluble, that is, would not dissolve in water, and therefore be harmless as far as strontium 90 is concerned, and also reduce the cesium 137 exposure substantially. To date we have not been successful in obtaining data completely conclusive on the solubility of the very fine Hardtack debris which does not fall into the Pacific Ocean. So we cannot say at this time whether this idea worked or not, but it seems likely that it will work at least to a limited extent.

There is a fourth possible technique which may give very low fallout, and this is the technique of firing in outer space. I must point out very carefully that this must be far enough out so that there is no atmosphere. A distance perhaps somewhere near halfway to the Moon or even farther would be best. We have had no experience with this method so far, so most of it is theory. But the theory looks pretty sound, and it says that the only fallout which would come down would be the fraction of the total solid angle subtended by the Earth at the site of the detonation.

This fraction for a distance of 60,000 miles above the Earth would be 1 in 900, or a little over one-tenth of 1 percent. So, to summarize, there are several ways of conducting tests which reduce the amount of fallout or its effects appreciably.

Two of these are pretty well proven. The third is in process of being tested, and the fourth is conjectural and theoretical, but may be promising.

Representative HOLIFIELD. Dr. Libby, you have brought a very important point into the discussion. The whole world is concerned about the fallout from tests. It has been a weapon of propaganda by the Soviet Union, and it has alarmed the people throughout the world. The moral issue of whether tests should be held or not or whether war should be waged or not is not being considered in this particular hearing. The data which we are accumulating are for people to use to make up their minds in the field of moral or religious conviction. However, if this process of testing—and I am looking at it from the technical standpoint—can continue without contamination of the world's atmosphere, you certainly take away from the Soviets one of their greatest weapons of propaganda.

I would like to have a full discussion on this as far as security allows us, as to the relative progress which could be made in weapon

development by limiting ourselves to underground tests, and far out into space stratospheric tests.

I would like to also have a discussion as to the relative difficulties involved as far as the obtaining of test data from space is concerned at this time in our history.

Dr. LIBBY. I would say that our underground experience has been limited to pretty small explosions. Our biggest one was something like 20 kilotons TNT equivalent. Of course, that is a long way from some of the explosions which we have conducted in our experiments in the past. So in the statement I made that so far as we see at the moment, the Nevada-type test—the kinds of explosions we felt free to conduct in Nevada, and you know, Mr. Holifield, we have limited the magnitude of those for obvious reasons—can be run underground. I want to hasten to point out that you can't blow a tank over from a device buried underground, or you can't perform troop training exercises and things of this sort far underground. Dr. Shelton will point out immediately that the Department of Defense loses certain effects information from underground testing. As far as telling about how the device works in its innards, we think we can do pretty well.

Our experience, as I say, is limited, but it looks quite hopeful that we can tell how the thing works and most of what we need to know without radioactive fallout.

Representative HOLIFIELD. The point that you make, then, is that as far as the development of small weapons is concerned, you can utilize the underground method. But as far as the effects which might occur from those weapons on military equipment is concerned, of course, you are precluded from that type of knowledge.

Dr. LIBBY. Yes.

Representative HOLIFIELD. Dr. Shelton, do you wish to add anything to that area, and then we will have questions on it.

Dr. SHELTON. Maybe just to amplify the subject a little further. The Department of Defense, of course, is vitally concerned in the employment of the weapons as we require them if it is necessary to go to war. The environments with which we must be familiar are those environments in which we would employ the weapon in the actual circumstances. Therefore, underground testing and away out in space are not the most likely environments for the immediate applications of the weapons. Just to amplify again, under these two types of testing situations, we are reduced to very small amounts of information on the main purpose of the weapon, that is, how to use it and where to use it and what it will do.

Representative HOLIFIELD. But you have had a great deal of testing experience on the large weapons in the atmospheric environment. There has been a limited experience in the development of small weapons in the environment. So it would be reasonable to suppose that if the cessation conference on weapons testing is not successful, and that testing is resumed, that you could proceed in the field of small weapons by this means without contaminating the atmosphere. Is that correct?

Dr. SHELTON. I believe there is quite a bit of profit in the development of small weapons.

Representative HOLIFIELD. Will you both testify in a general way on the difficulties and the limited or extensive area which may be in-

volved in the stratospheric testing? I notice you suggested making the tests halfway to the moon. I would say that is a long way to bring data back.

Representative HOLIFIELD. Could we keep on the ground for a moment?

Doctor, this contaminated area that is developed as the result of underground shots, does the material remain contained until it loses its life?

Dr. LIBBY. As far as we can tell from Rainier, and that is our oldest one, it is still sealed in that kind of beer bottle glass that was made. You remember what happened. It blew itself a bubble about 110 feet in diameter and the skin of the bubble was about 3 or 4 inches thick of molten glass, I think is the best way to describe it. That sealed into the glass all of the fission products or essentially all of them—a very small fraction, less than 1 percent, of the gaseous materials like krypton managed to escape—but even the gases were trapped and pretty well sealed into this glass.

Representative VAN ZANDT. Then there is just a small possibility of contamination beyond the area of the bubble?

Dr. LIBBY. That is correct. There was no contamination from the Rainier shot even of the test site. We can still go back in there and pick up the black looking molten glass, and it has the material in it. In my statement I made clear we had further study to do about the nature of the rock. This was a good rock for this particular kind of experiment. We don't know for sure that some other kind of rock would seal it in as well. So we have some more work to do on that.

Senator GORE. Mr. Chairman, what you are saying is that the intense heat glassifies, if that is a good word—not classifies—the sand, the rock, the material and encases the radioactivity within the glass. Glass being insoluble, it remains.

Dr. LIBBY. That is correct.

Senator GORE. Doctor, what are the essential things you want to learn from the test of a newly designed small weapon? Is it not first its workability?

Dr. LIBBY. That is correct, to be sure that it works.

Senator GORE. You want to know if it will detonate and if the mechanism is workable.

Dr. LIBBY. That is right.

Senator GORE. Secondly, you want to know the yield of the weapon?

Dr. LIBBY. That is right.

Senator GORE. Thirdly, you want to know the amount of radioactivity created thereby.

Dr. LIBBY. That is right.

Senator GORE. Cannot all of these three essential results of a test be obtained from an underground detonation?

Dr. LIBBY. We certainly believe so, though until we have more experience, I would say we couldn't say that we are absolutely certain. Our laboratories and our technical people seem to believe that we can do pretty well in the small weapons field without making any radioactive fallout by underground testing.

Senator GORE. From the standpoint of defense capability and applicability, the principal loss is in the inability to test effects upon weaponry, mechanism, et cetera.

Dr. SHELTON. Mr. Gore, that is essentially correct. If one were to think about very new weapons with which we perhaps would have very little experience in their effects on man or equipment, or the effects on other types of defense, we would always be extrapolating. We would always be taking the output of the weapon underground and saying what will it do in the case of war. To the extent that the weapons—I can't visualize the weapons in 1965—are so radically different from what I am used to and the effects I have measured today that we may be getting so far afield that I would just be hesitant to say that those weapons would have their maximum benefit and use within the Department of Defense.

Senator GORE. The purpose and trend of weapon development in the nuclear field has been toward development of a smaller warhead or projectile which will give the required explosive capacity; is that correct?

Dr. SHELTON. Generally the military characteristics have been stated broadly in those terms. There have been, however, refinements to them which I don't think we can discuss.

Senator GORE. Without referring to particular numbers, is it not a fact that a weapon now weighing only a very small percentage of the weight of the first atom bomb dropped will give an even larger blast?

Dr. SHELTON. That is certainly true.

Senator GORE. This kind of information, the yield of the weapon, the testing of the design of the weapon, the workability of the weapon, the amount of radiation created thereby, is, as the doctor has testified, obtainable in underground tests. So, though the Defense Department would undoubtedly give up something if the atmospheric tests were stopped, neverthcless, insofar as perfection and development of underground weapons are concerned, with the exception of learning the specific applicability, such could possibly be accomplished in underground tests.

Dr. SHELTON. I think that is correct, sir, to the extent that our present meager information on testing underground pays off and we don't find very large difficulties in obtaining those things that you just discussed. Our experience in underground testing is very limited at this time. The fact that we can develop weapons underground may involve a pretty rough road ahead of us, that is before we really iron out the problems.

Senator GORE. Mr. Chairman, I want to ask a few questions on tests in interplanetary space, but I will yield to you to go into that.

Representative HOLIFIELD. It is perfectly all right. We are ready for that. Is there any particular statement on the part of Dr. Libby and Dr. Shelton in regard to the planetary space testing to set the table for the Senator's question? Congressman Durham, did you have a question?

Representative DURHAM. Mr. Chairman, carrying that a little further, of course, we may lose something, but I think we have to think in terms of what we gain. This galaxy here this morning of select brains—some of the finest in the world, in my opinion—must not forget this one thing: That we confine strontium 90 and cesium and we have no danger from underground testing, as I understand Dr. Libby's statement. We did not put more in the air. The Defense Depart-

ment might have lost something here and yonder in assessing the value of the weapon which would be applicable to some type of weapon which the enemy is going to use against us. But we have to think in terms of how far we protect the people of this world, and not only of this country.

In thinking in those terms we have something here that is substantial and we have made a great contribution, I think, in making these underground tests.

Senator GORE. I agree with you completely. The problem cannot be viewed alone from the standpoint of weaponry. Indeed, the moral and political position of the United States in the family of nations of the world is just as important and just as essential to our national security as is our military strength. This problem must be viewed in balance.

Representative HOLIFIELD. Did you wish to make a statement in regard to the techniques involved in stratosphere testing? I realize you cannot go into that to any great extent, but at least give us what you can.

Dr. LIBBY. I haven't much to add to the statement I made. The main point is that it is theoretical, but the theory looks pretty sound. We have seen that fairly sizable objects can be carried out beyond the Moon. We think the fallout calculations where you do it just by the shadow the Earth casts on the side of the explosion is a sound calculation. We have no experience with it, really, on which to say how practical it is, Mr. Holifield. The only things that even approach it were the Johnston Island shots and the Argus shots, but they are at much lower altitudes. So it is conjectural. Though it looks promising, I would hesitate to give you the impression that it is just around the corner or anything like that. I think it is a possibility which we must keep in mind. As far as the fallout hearings are concerned, I think we can say pretty certainly that this kind of testing gets rid of most of the fallout.

Representative HOLIFIELD. Dr. Shelton, you agree to that?

Mr. SHELTON. Yes, I have nothing to add to that statement.

Dr. SELOVE. Mr. Holifield, recently the United States proposed a cessation of tests below 30 miles of altitude. I understand, as Dr. Libby has indicated, that for a test carried out at 100,000 miles from the Earth, very few of the radioactive particles emitted will be going in the direction of the Earth, and therefore we will get a reduced fallout. But by the same token an explosion carried out fairly close to the Earth, even a few hundred miles above the surface of the Earth directs the radioactive particles half toward the Earth and half away. Even if the explosion is in the region where there is no atmosphere, the half directed toward the Earth would travel down toward the atmosphere until the particles finally became imbedded in it. So one would think that explosions at altitudes of even a few hundred miles would deposit in the very outer regions of the Earth's atmosphere half of the total amount of fallout.

Representative HOLIFIELD. You would confine that to the long-lived particles, rather than the short lived.

Dr. SELOVE. Yes. The critical question, of course, is this: Once one deposits this amount of material in the very upper layers of the atmosphere, how long does it take for it to come down to the Earth's surface?

I have observed that in the official statements that have been made by the U.S. Government there has been no direct statement that fallout from shots above 30 miles will be sharply reduced, although I must say it seems to me the statements are phrased in such a way that one easily reads them to mean that. I wonder whether Dr. Libby might enlighten us somewhat on this question as to just how much information we have on what the fallout would be from shots at such altitudes.

Representative HOLIFIELD. Dr. Libby's statement covers shots far out, half way to the Moon. Of course, he does not treat with the areas closer to the Earth. Let us assume, for the time being, we are talking in terms of 500 or 600 miles from the Earth. Is that satisfactory?

Dr. SELOVE. Yes.

Dr. LIBBY. I would think that Dr. Selove's statement is completely right, that half of it would come into the top layers. It is just a matter of the angle. As we understand it, the fallout particles from a shot outside the atmosphere have velocities which are above the escape velocity. You might think they would be dragged in by their weight, the Earth's field would pull them in, but our theoretical calculations which are pretty solid would seem to indicate that this is not the case. The force of the explosion is adequate to give them velocities greater than the escape velocity. So the question really is, How big does the Earth look at the site of the explosion? If you are 500 to 600 miles, it covers just about half the sky. So something like half will be caught. However, there is a problem; we don't know just now how rapidly stuff put in the very top of the atmosphere mixes. I have always been a proponent of the fact that it mixes pretty fast. There are others, Dr. Shelton and Dr. Machta, I believe, who take a somewhat different view, that it takes a long time. I don't think we are sure how long it would be before this stuff that was put in the very top of the atmosphere got down on the ground.

Let us assume that it gets down in less than 40 years, which is the lifetime of the cesium and strontium which we are most worried about, I think Dr. Selove's statement is essentially correct.

Representative HOLIFIELD. Senator Gore.

Senator GORE. Dr. Libby, scientists have many advantages over politicians. Politicians may have a few advantages over scientists. One of the advantages that a scientist has in this field is that he can make a statement which to the public may appear bizarre, and be called a genius. When a politician makes such a statement, he may very readily be classified as a crank. When I suggested some months ago that we reserve the right to test in outer space, I had just such an experience. You say that we have not had experience in testing in outer space, but that theoretically it is possible and may be promising.

Dr. LIBBY. Yes.

Senator GORE. Would it not be possible theoretically to obtain these same three essential pieces of information to which I referred in underground tests by telemetering the results from outer space? Could you not in fact telemeter back the workability? You could find out whether or not the mechanism worked, could you not?

Dr. LIBBY. Presumably some such plan would be used or have to be used.

Senator GORE. I am presuming that we will develop a rocketry capability sufficient to project such a weapon into space with a sta-

tion which could separate with telemetering instrumentation. Upon that presumption it would be possible to determine if the device worked.

Dr. LIBBY. Yes, sir.

Senator GORE. Could you not also determine the amount of the yield of the weapon?

Dr. LIBBY. This is one of the difficult points, Senator Gore. I am not sure we know enough at this time. I would think so, but I don't think we can say we are sure.

Senator GORE. I would not say we are sure about anything here.

Dr. LIBBY. The difficulties of getting the instrumentation to work in that short instance of time is a pretty tough problem. I would say we have a lot to learn before it can be done, but it looks very promising, and I see no theoretical blocks or no theoretical barriers that look insurmountable.

Senator GORE. Could you obtain this third essential information, the amount of radioactivity created by the detonation?

Dr. LIBBY. That might be more difficult. It is a little hard to say offhand. One would have to get samples back, I imagine. It probably can be done.

Senator GORE. You say we have had no experience. Have the Russians?

Dr. LIBBY. As far as we know, no, I would say.

Senator GORE. Do you know that they have or have not?

Dr. LIBBY. I don't think anybody in this country knows that the Russians have done this. I think we would know.

Senator GORE. Do you think we would know?

Dr. LIBBY. I would think so.

Senator GORE. Do we have means of detecting a detonation a million miles from the earth?

Dr. LIBBY. That I am afraid is difficult. I am probably wrong about saying we would know at that distance. We probably would not know at that distance. I keep thinking between here and the moon where you can see it.

Senator GORE. One of the professors and I got in differing fields yesterday. I have been informed that we did not have means of detecting such a test now, and you say that is true?

Dr. LIBBY. I think I am not knowledgeable enough to be sure on that. It probably is true. You are talking now about a million miles.

Senator GORE. Yes. Do you think the Russians have a rocketry capability of projecting a weapon that far from the earth?

Dr. LIBBY. I really don't know, Senator Gore. Maybe Dr. Shelton could answer better. We know what they say the weight they have projected beyond the moon. I don't know for certain this means they could have done what you say or not. What would you think?

Dr. SHELTON. Again I don't recall the exact weight of the satellite they called Lunik to either the moon or the sun, and as it turns out it is orbiting the sun, which is about 90 some million miles away from us. But we don't know the exact weight. I don't recall it, but I think that number is available. It could be like a thousand pounds.

Senator GORE. I will not belabor the point. There are two points I am trying to make here. One is that the Russians seem to have a superior rocketry capability. Is that correct?

Dr. SHELTON. I think there is some good evidence of that, yes.

Senator GORE. I will accept that. Secondly, we do not know whether they have or have not conducted such tests—that is, positively—is that correct?

Dr. LIBBY. That is right.

Dr. SHELTON. Yes.

Senator GORE. Doctor, would it be permissible for you to tell us here, and I do not know whether it is permissible, you be your own judge, why the 50 kilometer point of demarcation was fixed in the President's letter?

Dr. LIBBY. I can give you a little background, Senator Gore. The conference of experts which met in Geneva last summer to discuss the technical aspects of control and inspection and detection and identification carried their considerations only to 50 kilometers. They didn't go above this. They rather arbitrarily limited their considerations to 50 kilometers. That background may suggest to you that this was involved in the mentioning of that number. You see, it is quite a housekeeping job to set up a control system for a treaty to cease nuclear testing, and to make a proposal which you don't know pretty well just how you are going to go about it is difficult. The experts conference did deal with explosions up to that altitude, and rather arbitrarily stopped at about that point.

Senator GORE. You think we have means of detection of explosions to an altitude of 30 miles?

Dr. LIBBY. The experts seem to think so. The only part of the experts' report I disagree with was their capability of underground detection. It seemed to me they made assumptions there which were not justified, that they could detect underground. But the rest of the report seems to me to be pretty solid. I think they are right.

Senator GORE. Has the Commission received any reports from Turkish pilots or others of the appearances of auroras, in the sky over Russia?

Dr. LIBBY. None that I know of. Of course, we had some from the Argus shots. They were not particularly over Russia.

Dr. SHELTON. I am not in a position to answer that question as to whether we have or there have been observed auroras or peculiar glows over Russia.

Senator GORE. You say you cannot answer it?

Dr. SHELTON. I am not capable of comment on that.

Senator GORE. Dr. Libby, do you know if the Killian group—if that is a fair way to describe it—has conducted studies about the possibility of hiding nuclear tests underground and in outer space?

Dr. LIBBY. Yes. They have been concerned with all of these matters, Senator Gore, and made very interesting studies.

Senator GORE. Are those studies classified?

Dr. LIBBY. They have not been published yet, Senator, I believe.

Senator GORE. Is that classification done upon the recommendation of the Atomic Energy Commission?

Dr. LIBBY. We are not directly involved in that, and I would answer no to that, because we are not directly involved.

Senator GORE. Has the Atomic Energy Commission made any recommendation that these reports be declassified?

Dr. LIBBY. We would like to see as much information on this kind of thing come out as can without revealing weapons information and that kind of thing. So we want to see them come out.

Senator GORE. Mr. Chairman, I have not seen the reports and even if I had seen them, I would not be the proper person to judge whether they should be declassified. But I do suggest that this committee ought to have copies of these reports, and I do not believe the committee has been provided copies of these reports. Thank you.

Representative HOLIFIELD. I would say we have one more panel, and it is necessary to hear one more set of witnesses, and we wish to conclude this morning because of other commitments.

Representative VAN ZANDT. Dr. Libby, as I understand it was agreed that we do not have devices that will detect nuclear explosions in outer space. Is that your understanding?

Dr. LIBBY. What I said was that it seemed to me we would see them out around maybe as far as the moon, but out beyond that I don't believe any such devices exist.

Representative VAN ZANDT. Dr. Selove, you said that any nuclear explosion in outer space would add to the contamination in the stratosphere, is that correct?

Dr. SELOVE. I indicated a mechanism by which 50 percent of the fallout could be expected to be deposited in the upper reaches of the atmosphere. As a matter of fact, one can think of a mechanism whereby 100 percent could be deposited even for explosions at several hundred miles. That is, the radioactive fisson fragments which are produced in an explosion are electrically charged, and they will make circular orbits around the magnetic lines of force in the same way described for electrons in the Argus test. One can imagine a mechanism whereby even for an explosion at several hundred miles all of the raidoactive material could be deposited at the top of the atmosphere. Whether it comes down through the atmosphere before it has decayed away radioactively or not, there seems not to be enough evidence to say.

Representative HOLIFIELD. If there are no further questions, gentlemen, we express our thanks and our appreciation for your testimony once again. You have brought some very valuable testimony to the public in your testimony this morning.

At this time we will ask Dr. Charles Dunham, Chief of the Division of Biology and Medicine of AEC, Dr. Francis J. Weber, Chief of the Division of Radiological Health of the Public Health Service, and Rear Admiral Parker, representing the Defense Department, to come forward to the witness table. We will have a discussion of surveillance and monitoring and research programs, and the organization of these programs. You may proceed.

STATEMENT OF DR. CHARLES DUNHAM,[1] CHIEF, DIVISION OF BIOLOGY AND MEDICINE, ATOMIC ENERGY COMMISSION

Dr. DUNHAM. Mr. Holifield, first I would like to correct something for the record. There may have been a misunderstanding of something that Mr. Hollister said in his statement about short-lived isotopes. I just want to make it clear that if weapons testing in the

[1] See biographical sketch, p. 10.

last 5 years is repeated every 5 years for the next 70 years, a person would get 14 times as much dose of radioiodine to the thyroid or strontium 89 to the bones as he would have up to date. What he was trying to bring out was that it is not a cumulative thing, and you do not build up more and more. I just wanted to clear that up.

Representative HOLIFIELD. Thank you for that clarification. I so understood.

Dr. DUNHAM. In the matter of the collection and dissemination of information and the development of research data with respect to the effects of fallout, I have placed in your hands simply to refresh your memories—this material was in my main document Tuesday morning—an outline of how we distribute our funds within the Atomic Energy Commission. I do not know how much detail you wish me to go into at the present time.

I think it is apparent that much of the material which has been presented to you during the last few days was developed with Atomic Energy Commission funds. On the other hand, much of it has been developed with Department of Defense funds and also with funds from other departments, including the U.S. Public Health Service.

I have here a table (see p. 1821) which indicates that in fiscal year 1958 we spent approximately $1,700,000 on fallout sampling and analysis, including money spent on collections from the stratosphere. In fiscal 1959, the current fiscal year, we are spending approximately $2,648,000 and for 1960, in our present budget presentation to the Congress, there is $3,564,000.

Representative VAN ZANDT. Doctor, at this point, have you projected your planning for the future?

Dr. DURHAM. We have projected our thinking in these areas for the next 5 years. We have done some thinking in this area for the next 2 or 3 years. It will, of course, depend on what happens in the field of weapons testing. If weapons testing continues by either the United States or some other country on a large scale with the production of large amounts of fallout, this program would probably have to continue to be expanded very greatly. If on the other hand with no further important increase of fallout produced with future weapons testing, this would tend to taper off. From a strictly scientific standpoint, if there are no contaminating tests for the next 1 or 2 years, we can answer a great many questions about fallout, how it behaves and material which would be tremendously useful in terms of any possible nuclear war.

What has happened in the past with our fallout studies has been that just as we began to get certain types of information, somebody

shot off another weapon here or another weapon there, and it sort of fouled things up generally. We are in a very unique position right now if things stay quiet a little longer to settle many of these questions. I think you found that whereas there has been general agreement on some of the major issues that were points of difference 2 years ago, that there are still some differences in terms of the mechanisms, and particularly the mechanisms of uptake of fallout once it has appeared on the surface of the earth, mechanisms of uptake into the soil, into the plants and into the diet.

I gave you at the same time a table which outlines in more detail the actual research arrangements in this area (see p. 1831). If you would like, I will ask Mr. Holland to comment on any particular aspects of that program which you would like to have comments on.

U.S. ATOMIC ENERGY COMMISSION—DIVISION OF BIOLOGY AND MEDICINE

Research project and budget summary

	Sci. M. Y.	Actual fiscal year 1958	Sci. M. Y.	Estimated fiscal year 1959	Sci. M. Y.	Estimated fiscal year 1960
Table I. Fallout sampling and analysis	100	$1,708,573	121	$2,648,000	129	$3,564,000
Table II. Program activities related to the development of radiation standards	1,265	25,155,251	1,403	30,898,000	1,496	34,902,000
Research related to radiation standards of permissible exposure (radiation biology)	854	15,738,263	919	18,760,000	970	21,134,000
Research related to radiation standards involving environmental contamination and waste disposal	176	2,708,122	214	4,064,000	224	4,499,000
Research related to prophylaxis and treatment of radiation injury	119	3,150,623	140	3,632,000	157	4,059,000
Dosimetry and instrumentation	94	2,027,703	95	2,083,000	98	2,267,000
Radiation protection, physical	4	867,616	6	1,440,000	17	1,762,000
Radiation measurements, general	18	662,924	29	919,000	30	1,181,000
Table III. Additional research including beneficial applications of atomic energy	360	8,474,246	426	9,696,000	475	10,534,000
Total biology and medicine program	1,725	35,338,070	1,950	43,242,000	2,100	49,000,000

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Breakdown on fallout sampling and analyses program by function

	Estimate, fiscal year 1959
I. Development of stratospheric and ground level sampling devices:	
Armour Research Foundation of the Illinois Institute of Technology	$30,000
Chicago, University of, Midway Laboratories	25,000
Del Electronics Corp	50,000
General Mills, Inc	300,000
Georgia Institute of Technology	6,000
Meteorology Research, Inc	10,000
Western Precipitation Corp	50,000
Subtotal	471,000
II. Stratospheric and surface air sampling and analysis:	
Air Force, U.S. Department of	220,000
Health, Education, and Welfare, U.S. Department of, PHS	75,000
Health and Safety Laboratory, NYOO, AEC [1]	150,000
Navy, U.S. Department of, Naval Research Laboratory [2]	40,000
Subtotal	485,000
III. Measurement of fallout deposition on the earth's surface and measurement of fallout on and in plants and animals including man:	
Agriculture, U.S. Department of, Soil Conservation	47,000
Air Force, U.S. Department of, Air Research and Development Command [2]	50,000
Arkansas, University of	21,000
California, University of, at Los Angeles	273,000
Columbia University, Lamont Geological Observatory [2]	150,000
General Electric Co., Hanford Atomic Products Division	200,000
Health, Education, and Welfare, U.S. Department of, NIH	4,000
Health, Education, and Welfare, U.S. Department of, BSS	26,000
Health and Safety Laboratory, NWOO, AEC [1 2]	600,000
Interior, U.S. Department of, Geological Survey	30,000
Los Alamos Scientific Laboratory [2]	25,000
Miami, University of, Marine Laboratory	20,000
Michigan, University of	12,000
Mount Washington Observatory	4,000
New Zealand, Government of, Division of Nuclear Sciences	10,000
Washington, University of, Applied Fisheries Laboratory [2]	140,000
Subtotal	1,612,000
IV. Compilation and meteorological interpretation of fallout data:	
Commerce, U.S. Department of, Weather Bureau	80,000
Total, fallout sampling and analyses program	2,648,000

[1] Split function.
[2] Radiochemical analyses are a part of these programs.

Representative VAN ZANDT. Doctor, do you know of any other nation in the world conducting an activity similar to ours in this particular field of fallout?

Dr. DUNHAM. The British are doing some very fine work. I don't think they have available the funds we have for some of these studies but they are doing very excellent work. We have very little information on what, if anything, the Russians are doing.

Representative VAN ZANDT. In the free world, it is a British-United States effort?

Dr. DUNHAM. Very largely, yes. Very definitely.

Representative DURHAM. Dr. Dunham, the $49 million is included in your authorization bill, this year, is that correct?

Dr. DUNHAM. That is correct. That is now in the budget before the Congress.

Representative DURHAM. It has been approved by the Budget Bureau?

Dr. DUNHAM. That is correct, sir. I call your attention to the fact that in the next fiscal year we are asking for a sum of approximately $35 million in an area of research which is directly related to the problem of developing radiation standards.

Representative DURHAM. The funds in the first column, the research goes further in this field. That is, research related to radiation standards of permissible exposure.

Dr. DUNHAM. Yes.

Representative DURHAM. You are trying to come up with some of the answers on radiation?

Dr. DUNHAM. Yes; that is right. This row here, the $18.7 million figure for fiscal 1959, and the $21 million for 1960, supports work like that of Dr. Russell who has made such an important contribution in the field of genetic effects. It goes to support Dr. Brues and people of that sort on the matter of somatic effects.

Representative HOLIFIELD. But it has other points than just the radiation detection system, does it not?

Dr. DUNHAM. Below this top line is strictly research or laboratory research. This above the line, the smaller figure is what is actually spent for sampling and analysis and trying to find out where the fallout is and where it is going.

Representative VAN ZANDT. Cannot we say other than the amount of money being spent on fallout sampling and analysis the rest is being spent on the peacetime uses of the atom.

Dr. DUNHAM. The rest of it is all applicable to the problems inherent in the peacetime development of atomic energy, because you have the same fission product produced by atomic reactors that you do from an atomic weapon.

Representative HOLIFIELD. But the things you do learn in that field are related to fallout evaluation?

Dr. DUNHAM. That is very true. It is just like work in cancer research, Mr. Holifield. At the Memorial Hospital at New York, the American Cancer Society supports research there. The National Cancer Institute supports the research there. We support research there, because they are using radiation. It is all grist to the same mill.

Representative VAN ZANDT. Is it proper to say here that the amount of money being spent other than on fallout sampling and analysis is spent in the development of the peaceful use of the atom for biology, medicine, agriculture, and industry? Is that right?

Dr. DUNHAM. This part of the budget has very little to do with the peaceful uses in industry in terms of isotope development for industry. I feel that everything above the line here which says additional research, which includes straight cancer research and things of that sort, is essentially for an agency which is developing more radiation than has ever been developed before. More curies of radioactivity, that is. It must have a research program of this sort to understand what it is dealing with and to face its day to day problems.

Chairman ANDERSON. Doctor, do you like that term, research related to radiation hazard? For instance, suppose somebody asked

what is the cost of the legislative establishment of Congress up here. If somebody printed the figure on cost of legislation and things related to it, you would have to list every lobbyist in Washington and add all those totals to show what Congress cost. Don't you think we should talk about what we are actually doing on radiation standards of permissible exposure.

Dr. DUNHAM. The actual standards are a result largely of the deliberations of national bodies, such as the National Committee on Radiation Protection, and the International Commission. These use the results of these research efforts and research efforts by other agencies to develop their recommendations for radiation protection. That is why I say the research is related to it. Under here is an item, radiation protection—physical—we actually do research on materials that can be used to protect individuals from radioactivity, and the shielding properties of certain substances, and that sort of thing.

Chairman ANDERSON. Is that all included in the $18 million?

Dr. DUNHAM. That is all included in the $30 million. It is an item here of $1,440,000.

Chairman ANDERSON. Have you any study that shows exactly how much you are spending on trying to measure fallout data?

Dr. DUNHAM. That is the top row.

Chairman ANDERSON. That is the analysis of it?

Dr. DUNHAM. Yes.

Chairman ANDERSON. That is really all the money that is directly spent in that field?

Dr. DUNHAM. That is directly spent in actual measurement of fallout and how much is where.

Chairman ANDERSON. In your position, did you receive the report of the General Advisory Committee that has been referred to a day or two ago?

Dr. DUNHAM. Yes, sir.

Chairman ANDERSON. Did you think it a clear report and a satisfactory one?

Dr. DUNHAM. I thought, considering the time that was available to them, they made a very considered statement, yes, sir.

Chairman ANDERSON. What time was available to them?

Dr. DUNHAM. I think they had——

Chairman ANDERSON. Isn't all time available to them? They have been in continuous operation for years and years and years. What was the time available to them?

Dr. DUNHAM. For making the particular study and report.

Chairman ANDERSON. They had to get it in for this hearing.

Dr. DUNHAM. That I don't know.

Chairman ANDERSON. What do you mean by the time available to them?

Dr. DUNHAM. They met in Washington, I think, and for a period of 3 days, and received information which was provided by the Division of Biology and Medicine, provided by Admiral Parker and AFSWP, and provided by the Surgeon General of the Public Health Service.

Chairman ANDERSON. We won't quibble. It was 2½ days, wasn't it?

Dr. DUNHAM. Yes, I guess it was.

Chairman ANDERSON. In that 2½ days you gave them the material and they digested it and came out with this report?

Dr. DUNHAM. That is right.

Chairman ANDERSON. They said all the information on fallout had been released. I referred the other day to the Langham report that is not released. Were they told how much had been released, and how much was not released, so they would not have made that erroneous statement about all the information being released?

Dr. DUNHAM. May I look at a copy of their statement before I answer the question?

Chairman ANDERSON. Yes.

Dr. DUNHAM. I think they used the word "significant" fallout data, and I don't pretend to know what their interpretation of the word "significant" is.

Chairman ANDERSON. Then you can't tell whether the report was good or bad until you know what significant meant. For instance, they don't apparently feel this Cowan-Langham-Anderson report from the scientists at Los Alamos is significant; although more measuring has been done there than anywhere else in the world.

Dr. DUNHAM. They have done an awful lot of measuring, but I question your conclusion that they did not consider the Langham report significant.

Chairman ANDERSON. They said "We find the Atomic Energy Commission has released all significant fallout data to other agencies and to the public." We just had testimony from Dr. Langham that it is not released. He gave us some reasons why. I accept the reasons as valid. But the significant fact is—I should not use the word "significant"—the essential fact is that they are not released. Now comes the GAC, in order to get out this story about the 5 percent, and says all significant data have been released. It was not released. Why didn't you fire the report back to them and say "Take another look at them," or isn't that part of the AEC function?

Dr. DUNHAM. I don't know that they have seen Dr. Langham's report. They may have.

Chairman ANDERSON. What is that?

Dr. DUNHAM. I don't know that they saw Dr. Langham's report.

Chairman ANDERSON. They saw enough of it so they classified it. If you have any question about it, Dr. Libby understands they classified, Dr. Langham was on the stand—weren't you here when he testified yesterday—and I asked him about it?

Dr. DUNHAM. I thought we were still talking about the General Advisory Committee, sir.

Chairman ANDERSON. Let us see if we can start again. The General Advisory Committee report which Mr. McCone sent to the committee under date of May 5, 1959, contains this statement in their report which was dated May 4:

> We find that the Atomic Energy Commission has released all significant fallout data to other agencies and to the public.

Dr. Langham testified yesterday—I have to go back and say I tried my best to get the Langham report and reevaluation of the earlier report which was kept classified about 2 years, although the people that prepared it recommended its declassification, and those of us who

studied it could not find a single thing in it that needed to stay classified, but they nonetheless kept it under wraps until a couple or 3 or 4 weeks ago—time goes pretty rapidly these days—but I knew Dr. Langham was preparing a reevaluation. That became available to the Atomic Energy Commission. I had hoped it would be released. I kept pushing on it and I got a letter, the letter I think went into the files or was at least made available yesterday, explaining that it could not be declassified. It had to remain classified. Do you find under that basis that all significant fallout data has been released to the public?

Dr. DUNHAM. It is possible that that sentence and the one that follows are mutually incompatible.

Chairman ANDERSON. Who wrote this section of the report? Was this suggested to the General Advisory Committee by any particular group as tentative language that they might or might not adopt?

Dr. DUNHAM. Not so far as I know.

Chairman ANDERSON. Did your office help in the preparation of this document?

Dr. DUNHAM. I was consulted personally on other aspects of it, but not on that particular part.

Chairman ANDERSON. Was anyone in your office consulted on that particular aspect of it?

Dr. DUNHAM. Not so far as I know, sir.

Chairman ANDERSON. You evaluate the fallout hazards from tests to date and do not make any further predictions like the panel we have had here before us did. Do you think it would have been well to make any evaluations of what would happen if fallout continued?

Dr. DUNHAM. When?

Chairman ANDERSON. When the GAC turned in its report. That is what we are talking about. Do you think the GAC might have looked at that? They did not give the possibility of fallout increasing. They didn't even give that the cold courtesy of a passing glance. Why not?

Dr. DUNHAM. I am not the GAC, sir.

Chairman ANDERSON. I realize that. You have the report. It will be passed to the public. Why not kick it back to the GAC and say this is a funny study; give us one. Did they make any evaluation of strontium 90? As I read it, the only evaluation they gave on strontium 90 is that it is about like drinking water. We had some testimony that strontium 90 had gotten up to eight strontium units which was a very important discovery. Yes, up to 40 if we keep on testing. It is about eight at certain places now. Then when the General Advisory Committee gave its report, which got front page play lots of places, it says it is interesting to note that human beings have lived in many parts of the world with heavy radiation in regard to internal effects of strontium 90 to injection, the amount of strontium 90 which has been found in food and water—and I stop there to say that includes the bread test and milk test, and I assume they were brought up to date on that, because you briefed them on it—in regard to internal effects of strontium 90, the amount of strontium 90 which has been found in food and water is less of a hazard than the amount of radium normally present in public drinking water supply in certain places in the United States, and in public use for many decades.

Doctor, would you say when you think when these units get up to eight strontium units that is about what you find in the public drinking supply in most parts of the country?

Dr. DUNHAM. Did you say most parts of this country?

Chairman ANDERSON. I intended to say in certain places. I want to try to get you down where you can agree with me.

Dr. DUNHAM. Actually what they are referring to is the drinking water which serves a population of several hundred thousand people.

Chairman ANDERSON. Where?

Dr. DUNHAM. In the central west.

Chairman ANDERSON. Where?

Dr. DUNHAM. From deep wells.

Chairman ANDERSON. Where?

Dr. DUNHAM. Mostly Illinois has been studied. There are several times in many instances the recommended permissible amount—they contain several times the recommended permissible amount of radium.

Chairman ANDERSON. Do you think that this statement given here is designed to let the public rest easy? Does this warn them that they are up to 8 strontium units and may get up to 40? You mentioned food and water both. Was there anything in the report on milk and on bread that worried you in the slightest?

Dr. DUNHAM. Worried me?

Chairman ANDERSON. Yes. You are head of the division.

Dr. DUNHAM. I am concerned, yes.

Chairman ANDERSON. I think they passed it off by saying it is only going to kill a few thousand. Don't you believe that you ought to ask them what they mean by the amount of radium is not any worse—the amount found in food and water—than the amount present in the public drinking supply in certain places in the United States, and in public use for many decades? Couldn't they profitably take another look at that one? What I am trying to get at, Doctor, we are constantly told, don't worry about this strontium, it is no worse than the luminous watch dial. I have worn a luminous wristwatch for a good many years so I assume I have all the strontium 90 exposure that I can get because of the radium exposure out of this watch. But along comes somebody who finds something in food that might be disturbing. Is there no place in the Atomic Energy Commission setup where that needs to be called to the American public or must it depend upon the rather vague line of communication that has to exist between the newspapers and the Atomic Energy Commission?

Dr. DUNHAM. I believe my public statements and the papers I have written on the subject of fallout have been quite frank in stating the facts.

Chairman ANDERSON. Doctor, I would agree with that. I think you have done a fine job. I have said so publicly before. I hope I can continue to say so publicly. I am just hoping that the public gets a chance to find out if there is any real hazard to this. It strikes me that when the hazard gets up to 8 strontium units and could go to 40 that it is somebody's job to talk about it, and somebody's job who talks about it with enough scientific background so that the public might listen. I have great sympathy for a newspaper story, because I have difficulty finding what it is all about. I think that if I have difficulty with two or three people trying to explain it to me, that

somebody else might have difficulty, and that these lines of communication are not too wide open. I am just hoping that it could be possible for the General Advisory Committee to be told very politely that this was a fine résumé of conditions as of 2 years ago, and that it might be still the same as now, but that we are interested in what might be going on 10 years from now, and it is not against the law for them to take a look at that.

Dr. DUNHAM. I will pass this on to them.

Chairman ANDERSON. I hope you will. I hope you will pass it away from yourself, because the criticism is not directed to you and I am sure you understand that.

Dr. DUNHAM. Thank you.

Representative HOLIFIELD. If there are no questions at this time, you have completed your summarization, Dr. Dunham?

Dr. DUNHAM. I believe I would like to reserve further comments until after Dr. Weber and Admiral Parker have talked.

(Material submitted by Dr. Dunham follows:)

U. S. ATOMIC ENERGY COMMISSION
DIVISION OF BIOLOGY AND MEDICINE

Research Project and Budget Summary

		Actual FY 1958	Estimated FY 1959	Estimated FY 1960
Table I	FALLOUT SAMPLING AND ANALYSIS	$ 1,708,573	$ 2,648,000	$ 3,564,000
Table II	PROGRAM ACTIVITIES RELATED TO THE DEVELOPMENT OF RADIATION STANDARDS	25,155,251	30,898,000	34,902,000
	Research Related to Radiation Standards of Permissible Exposure (Radiation Biology)	15,738,263	18,760,000	21,134,000
	Research Related to Radiation Standards Involving Environmental Contamination and Waste Disposal	2,708,122	4,064,000	4,499,000
	Research Related to Prophylaxis and Treatment of Radiation Injury	3,150,623	3,632,000	4,059,000
	Dosimetry and Instrumentation	2,027,703	2,083,000	2,267,000
	Radiation Protection - Physical	867,616	1,440,000	1,762,000
	Radiation Measurements - General	662,924	919,000	1,181,000
Table III	ADDITIONAL RESEARCH INCLUDING BENEFICIAL APPLICATIONS OF ATOMIC ENERGY	8,474,246	9,696,000	10,534,000
	Total Biology and Medicine Program	$35,338,070	$43,242,000	$49,000,000

May 1959

TABLE I

U. S. ATOMIC ENERGY COMMISSION
DIVISION OF BIOLOGY AND MEDICINE

BREAKDOWN ON FALLOUT SAMPLING AND ANALYSES PROGRAM BY FUNCTION

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
I. DEVELOPMENT OF STRATOSPHERIC AND GROUND LEVEL SAMPLING DEVICES			
Armour Research Foundation of the Illinois Institute of Technology Evaluation of Collecting Systems Used in Determining Radioactive Fallout.	$ 5,824	$ 30,000	$ 30,000
Chicago, University of - Midway Laboratories Study of High Altitude Sampling Techniques	36,867	25,000	30,000
Del Electronics Corporation Research and Development on an Electrostatic Stratospheric Dust Sampler	-0-	50,000	50,000
General Mills, Inc. Stratospheric Particle Collection Studies	255,492	300,000	300,000
Georgia Institute of Technology An Investigation of the Particle-Collecting Capability of Thermal Precipitators at Simulated High-Altitude Conditions.	-0-	6,000	6,000
Meteorology Research, Inc. Study of Routine Stratospheric Sampling	-0-	10,000	10,000
Western Precipitation Corporation A Conceptual Study of Possible Collecting Systems for Use in Stratospheric Sampling.	-0-	50,000	50,000
Sub-Total.....	$ 298,183	$ 471,000	$ 476,000

BREAKDOWN ON FALLOUT SAMPLING AND ANALYSES PROGRAM BY FUNCTION

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
II. STRATOSPHERIC AND SURFACE AIR SAMPLING AND ANALYSIS			
Air Force, U. S. Department of			
Project "Ash Can"	$ 163,309	$ 220,000	$ 750,000
Health, Education and Welfare, U. S. Department of, Public Health Service			
Establishment of a Radiation Surveillance Network	36,252	75,000	75,000
Health and Safety Laboratory - New York Operations Office, AEC			
Collection and Analysis of Fallout Samples 1/	114,868	150,000	178,000
Navy, U. S. Department of - Naval Research Laboratory			
Radioactivity Monitoring Program 2/	-0-	40,000	40,000
Sub-Total	$ 314,429	$ 485,000	$1,043,000
III. MEASUREMENT OF FALLOUT DEPOSITION ON THE EARTH'S SURFACE AND MEASUREMENT OF FALLOUT ON AND IN PLANTS AND ANIMALS INCLUDING MAN			
Agriculture, U. S. Department of			
Collection and Preparation of Samples of Soils, Plants, and Animals for Calcium and Strontium Analysis	$ 22,312	$ 47,000	$ 52,000
Air Force, U. S. Department of, Air Research and Development Command			
VHA Tracer and Strontium-90 Studies (Hardtack) 2/	-0-	50,000	50,000

1/ Split Function
2/ Radiochemical Analyses are a part of the programs denoted by 2/.

BREAKDOWN ON FALLOUT SAMPLING AND ANALYSES PROGRAM BY FUNCTION

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
III. MEASUREMENT OF FALLOUT DEPOSITION ON THE EARTH'S SURFACE AND MEASUREMENT OF FALLOUT ON AND IN PLANTS AND ANIMALS INCLUDING MAN - continued			
Arkansas, University of Radioactive Fallout	$ -0-	$ 21,000	$ 25,000
California, University of, at Los Angeles Assessment of the Nature of Fallout Patterns and of the Biological Availability of Fallout-Derived Fission Products	269,117	273,000	272,000
Columbia, University, Lamont Geological Observatory Distribution of Certain Fission Product Activities 2/	127,873	150,000	150,000
General Electric Company, Hanford Atomic Products Division Environmental Studies	171,000	200,000	210,000
Health, Education and Welfare, U. S. Department of, National Institute of Health Interdepartmental Committee on Nutrition for National Defense	4,000	4,000	5,000
Health, Education and Welfare, U. S. Department of, Bureau of State Services. Expansion of the Biological Sampling Program Initiated in Nevada	-0-	26,000	50,000
Health and Safety Laboratory, New York Operations Office (AEC) Collection and Analysis of Fallout Samples 1/ 2/	344,604	600,000	900,000
Interior, U. S. Department of Geological Survey Aerial Radiological Surveys	-0-	30,000	30,000

1/ Split Function
2/ Radiochemical Analyses are a part of the programs denoted by 2/.

BREAKDOWN ON FALLOUT SAMPLING AND ANALYSES PROGRAM BY FUNCTION

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
III. MEASUREMENT OF FALLOUT DEPOSITION ON THE EARTH'S SURFACE AND MEASUREMENT OF FALLOUT ON AND IN PLANTS AND ANIMALS INCLUDING MAN - continued			
Los Alamos Scientific Laboratory Fallout Sampling and Analysis 2/	$ 21,872	$ 25,000	$ 25,000
Miami, University of - Marine Laboratory Geochemical Studies in the Radioactive Nuclides in Sea Water and Sediments	9,933	20,000	20,000
Michigan, University of Rain Scavenging of Particulate Matter from the Atmosphere	-0-	12,000	12,000
Mount Washington Observatory Study of the Collection of Radioactive Debris by Cloud Particles and Precipitation	-0-	4,000	4,000
New Zealand, Government of - Department of Scientific and Industrial Research, Division of Nuclear Sciences The Estimation of Sr-90, Ba-140 and Possibly Cs-137 in Every Rain at Gracefield, New Zealand.	-0-	10,000	10,000
Washington, University of - Applied Fisheries Laboratory Marine Surveys to Measure Radiation and Its Distribution and Movement in the Sea from Weapons Tests. 2/	100,000	140,000	150,000
Sub-Total	$1,070,711	$1,612,000	$ 1,965,000

2/ Radiochemical Analyses are a part of the programs denoted by 2/.

BREAKDOWN ON FALLOUT SAMPLING AND ANALYSES PROGRAM BY FUNCTION

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
IV. COMPILATION AND METEOROLOGICAL INTERPRETATION OF FALLOUT DATA			
Commerce, U. S. Department of - Weather Bureau			
Transport, Deposition and Meteorological Effects of Atomic Test Debris in the Atmosphere	$ 25,250	$ 80,000	$ 80,000
Sub-Total	25,250	80,000	80,000
TOTAL - FALLOUT, SAMPLING AND ANALYSIS PROGRAM	$1,708,573	$2,648,000	$3,564,000

Program Activities Related to the Development of Radiation Standards

TABLE II

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(1) Research Related to Radiation Standards of Permissible Exposures (Radiation Biology)	$15,738,263	$18,760,000	$21,134,000
Off-Site Research Projects			
Agnes Scott College			
A Comparison of Irradiation Effects on Wet and Dry Ciliate Cysts in Nitrogen, Oxygen and Air	$ 3,529	$ 4,000	$ 4,000
Albany Medical College of Union University			
A Study of the Concentration of Seven Elements (Iron, Zinc, Copper, Molybdenum, Manganese, Chromium and Nickel) in the Viscera of Rats Before and After Total Body X-Irradiation	-0-	12,000	13,000
Albert Einstein Medical Center			
Enzymatic and Biochemical Studies of Animal Skin Which has been Irradiated with Beta Particles	13,000	22,000	22,000
Amherst College			
Genetic Effects of Acute and Chronic Low Level Irradiation with Cobalt-60	19,730	20,000	20,000
Arkansas, University of			
Developmental-Genetic Study of the Effects of X-Ray Irradiation in *Drosophila virilis* and *Bufo valliceps*	4,062	5,000	5,000
Armour and Company			
The Connection Between Cross-Linking and Physiological Function in Connective Tissues	-0-	6,000	6,000
Beth Israel Hospital Association, Inc.			
The Use of I-131 in Treatment of Heart Diseases and Follow-up Studies on Biological Effects of Radiation	16,000	18,000	18,000

Program Activities Related to the Development of Radiation Standards - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(1) Research Related to Radiation Standards of Permissible Exposures (Radiation Biology) - continued			
Off-Site Research Projects - continued			
Biochemical Research Foundation Search for Antimutagens	$ -0-	$ 20,000	$ 21,000
Djorksten Research Foundation Fundamentals of Aging - Insolubilization of Protein as a Function of Aging	2,750	13,000	13,000
Boston University - School of Medicine Radiation-Induced Changes in Nucleic Acids and Their Hydrolysis Products	2,625	-0-	-0-
Boston University The Effect of Irradiation on the Growth and Functioning of Transplanted or Regenerated Adrenocortical Tissue in the Rat	13,889	14,000	15,000
Brigham Young University The Effects of X-Irradiation Upon Embryonic Development in the Paradise Fish, Macropodus opercularis	2,625	-0-	-0-
Brown University Radiation Effects on the Cytoplasm of Habrobracon Eggs	6,000	6,000	7,000
Brown University Biosynthesis of Proteins in Mouse Liver	-0-	10,000	11,000
California Institute of Technology The Genetic and Cytological Effects of High Energy Radiation	42,591	43,000	68,000
California, University of - Agricultural Experiment Station Effects of Sr-90 Administered During the Growth Period of the Dog	60,951	125,000	200,000

Program Activities Related to the Development of Radiation Standards - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(1) Research Related to Radiation Standards of Permissible Exposures (Radiation Biology) - continued			
Off-Site Research Projects - continued			
California, University of - Agricultural Experiment Station			
The Effect of Radiation on Work Capacity and Longevity of the Dog	$ 99,488	$ 100,000	$ 105,000
California, University of - School of Medicine			
Studies on the Induction of Thyroid Cancer and on the Nature of Metabolic Blocks Following Irradiation	24,236	30,000	30,000
California, University of			
Investigation of the Mechanism of the Effect of Ultraviolet Light on Enzymes and Viruses	5,572	6,000	6,000
California, University of			
Radiosensitivity of the Hen's Oviduct	7,000	-0-	-0-
California, University of (L. A.)			
The Study of Plant Virus as Approached by the Study of the Normal Plant Proteins	15,153	16,000	16,000
California, University of			
Age and Vulnerability to X-Rays	-0-	55,000	55,000
Cedars of Lebanon Hospital			
Chemical Studies on Connective Tissues of Animals Aged Prematurely by Irradiation (Assessment of Biochemical Age)	11,550	12,000	13,000
The Chicago Medical School - Division of Oncology			
A Study of the Latent Tumor Cells as Produced by Beta Radiation and a Comparison of the Latent Tumor State with that Produced by Chemical Carcinogens	11,000	12,000	13,000

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Program Activities Related to the Development of Radiation Standards -- continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(1) Research Related to Radiation Standards of Permissible Exposures (Radiation Biology) -- continued			
Off-Site Research Projects - continued			
Chicago, University of The Genetic Functioning of Heterochromatin	$ 12,555	$ 15,000	$ 17,000
Chicago, University of Studies on the Mechanism of Action of Ionizing Radiations	5,957	-0-	-0-
Chicago, University of Studies in Photobiochemistry and Bioenergetic Problems	2,234	-0-	-0-
Chicago, University of Photochemical Reactions of Chlorophyll and of Related Photoactive Pigments	9,748	10,000	10,000
Chicago, University of Bacteriological Aspects of Radiation Sickness	44,092	45,000	45,000
Chicago, University of A Study of the Effect on Gastric Tissues of Irradiation Therapy in Peptic Ulcer	12,024	12,000	12,000
Chicago, University of Investigation of the Pathogenesis of Radiation Nephritis, Clinical and Experimental	-0-	9,000	9,000
Chicago, University of The Effect of Whole Body Ionizing Radiation on the Quality of Antibody	5,500	-0-	-0-

Program Activities Related to the Development of Radiation Standards - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(1) Research Related to Radiation Standards of Permissible Exposures (Radiation Biology)- continued			
Off-Site Research Projects - continued			
Chicago, University of The Biological Effects of Stressful Agents as Related to Diet	$ -0-	$ 8,000	$ 8,000
Chicago, University of Investigations of Cellular Biology, with Special Reference to Cell Division, by Means of Microbeams of Ultraviolet Radiation	39,975	40,000	40,000
Children's Medical Center - Childrens Cancer Research Foundation The Effect of Irradiation on Induction of Pituitary Tumors	20,000	22,000	23,000
Christian Brothers College Uranium Complexes with Amino Acids and Peptides	4,150	5,000	5,000
City of Hope Medical Center A. The Effect, upon the Mutation Rate, of Removal at Time of Irradiation of Peroxides from Irradiated Germ Cells; B. A Comparison of Patterns of Free Amino Acids and Other Metabolites in Several Minute Stocks of Drosophila Melanogaster at Various Stages of Development and the Influence of X-Irradiation upon these Patterns	4,300	-0-	-0-
Colorado State University The Effects of X-Irradiation on Embryonic Hematopoietic Tissue	1,837	8,000	8,000
The University of Colorado The Biological Half-Life of Radiolead and the Quantitative Effect of $Na_2CaEDTA$ on the Total Body Radiolead Burden in Dogs	-0-	20,000	20,000

Program Activities Related to the Development of Radiation Standards - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(1) Research Related to Radiation Standards of Permissible Exposures (Radiation Biology) - continued			
Off-Site Research Projects - continued			
Colorado, University of Radiation Effects in Biochemistry and Organic Chemistry	$ -0-	$ 25,000	$ 28,000
Columbia University A Study of the Mechanism of Induction in a Lysogenic Organism Which Converts the Latent Bacterial Virus to Virulence	-0-	15,000	17,000
Columbia University The Population Genetics of Species of Drosophila	10,870	12,000	12,000
Columbia University Studies of Mutations in Populations of Wild House Mice	7,380	25,000	25,000
Columbia University Biological Action of Ionizing Radiation. Instrumentation for Research	205,309	200,000	200,000
Columbia University Effect of Ionizing Radiation on Nerve Tissue	10,000	19,000	12,000
Columbia University, New York Lamont Geological Observatory Strontium and Calcium Metabolism in Humans	13,325	30,000	15,000
Columbia University Study of the Action of Radiation on Deoxypentose Nucleic Acids Having Biological (Transforming) Activity	3,879	8,000	9,000

Program Activities Related to the Development of Radiation Standards - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(1) Research Related to Radiation Standards of Permissible Exposures (Radiation Biology) - continued			
Off-Site Research Projects - continued			
Columbia University			
Study of the Incorporation of Deuterium into Deoxyribonucleic Acids and of the Biological Effects of Such Incorporation	$ -0-	$ 11,000	$ 12,000
Cornell University - New York State Agricultural Experiment Station			
The Induction and Testing of Somatic Mutations in Apples, Grapes and Other Economic Plants	3,240	-0-	-0-
Cornell University Medical College			
Studies of Protein Synthesis in the Gastrointestinal Tract of Normal and Irradiated Mice	-0-	14,000	15,000
Cornell University			
The Investigation of the Genetic Structure of Populations	7,418	19,000	20,000
Cornell University			
Cytological and Genetic Studies of Bacteria as Related to Effects of Radiation	1,404	12,000	-0-
Creighton University School of Medicine			
Ca-45 and Sr-85 Metabolism in Man	-0-	13,000	13,000
Delaware, University of			
The Relation of Genome Number to Radiosensitivity in Habrobracon	11,256	12,000	12,000
Duke University			
The Effects of Ultraviolet Light and Gamma Rays on Cell Lipids and the Physiological Action of Irradiated Lipids	26,013	23,000	24,000

Program Activities Related to the Development of Radiation Standards - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(1) Research Related to Radiation Standards of Permissible Exposures (Radiation Biology) - continued			
Off-Site Research Projects - continued			
Edsel B. Ford Institute for Medical Research Comparative Metabolic Effects of Certain Hormones and X-Irradiation	$ 5,750	$ 12,000	$ 13,000
Emory University Studies of the Influence of Oxygen Level and Temperature on the Effects of Ionizing Radiation	7,532	9,000	9,000
Florida State University A Study of Gene and Chromosome Changes Induced by Ionizing Radiation in Drosophila melanogaster	7,548	10,000	10,000
Florida State University An Attempt to Photoreactivate Enzymes in vitro	-0-	7,000	8,000
Florida, University of, College of Medicine Genetic Effects of Chronic X-Irradiation Exposure in Mice	20,200	2,000	-0-
Florida, University of - Cancer Research Laboratory Effect of Radiation on the Uptake of Large Organic Molecules by the Liver and Spleen of the Mouse	8,654	10,000	11,000
Florida, University of Mutation Research with Gamma Radiation at a Specific Locus in a Higher Plant	-0-	10,000	11,000
Florida, University of - Agricultural Experiment Station Recovery of Radiation Induced Micromutations in Oats by Recurrent Selections	6,000	8,000	9,000

Program Activities Related to the Development of Radiation Standards - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(1) Research Related to Radiation Standards of Permissible Exposures (Radiation Biology) - continued			
Off-Site Research Projects - continued			
Fordham University Fate of Thiamine and Thiamine Analogs in the Animal Body. Mechanism of Thiamine Inhibition by Thiamine Analogs	$ 5,750	$ 6,000	$ 6,000
George Washington University A Study of Certain Factors Affecting the Susceptibility of Cultured Human Tissue to the Lethal Action of Ionizing Radiation and Carcinolytic Drugs	-0-	17,000	18,000
George Washington University The Dose-Incidence Relationship of Beta Radiation-Induced Skin Cancer in the Rat	20,663	25,000	25,000
George Washington University Studies of the Effects of Radiation on the Biosynthesis and Degradation of Nucleoproteins and Its Modification by Various Agents	20,976	14,000	15,000
Hahnemann Medical College and Hospital The Biochemical Properties of Microsomes and the Effects of Radiation on Them	5,000	-0-	-0-
Hahnemann Medical College and Hospital The Effect of Whole Body X-Irradiation on the Physical, Chemical and Biochemical Properties of Nucleoprotein	5,000	11,000	11,000
Harvard University Prenatal X-Ray and Childhood Neoplasia: A Cohort Study	-0-	34,000	34,000
Harvard University Radiation Effects on the Lung	12,880	18,000	18,000

Program Activities Related to the Development of Radiation Standards - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(1) Research Related to Radiation Standards of Permissible Exposures (Radiation Biology) - continued			
Off-Site Research Projects - continued			
Health, Education and Welfare, Department of - Bureau of State Services			
Study of Methods for the Control of Radioactive Dust and Gases in Mines	$ 10,000	$ 10,000	$ 10,000
Health, Education and Welfare, Department of			
Studies on Long Range Radiation Effects on Human Beings	55,462	105,000	100,000
Idaho State College			
A Micromanometric Study of Sensitized Photodecompositions	5,292	-0-	-0-
Illinois, University of, College of Medicine			
A Reevaluation of Radiation Injury (B Rays) of the Skin by a Direct Method Approach	17,000	8,000	-0-
Indiana University Foundation			
The Influence of Radiation in Altering the Incidence of Mutations in Drosophila	24,375	35,000	35,000
Indiana University Foundation			
Cellular Heredity in Paramecium	19,290	21,000	23,000
Institute for Cancer Research			
Studies on the Effects of Ultraviolet Radiation on Cell Structure and Behavior	-0-	29,000	16,000
Iowa State College			
Quantitative Study of Lifetime Sickness and Mortality and Progeny Effects Resulting from Exposure of Animals to Penetrating Irradiation	107,854	114,000	123,000

Program Activities Related to the

Program Activities Related to the Development of Radiation Standards - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(1) Research Related to Radiation Standards of Permissible Exposures (Radiation Biology) - continued			
Off-Site Research Projects - continued			
Iowa State College Genetic Results of Irradiating Swine	$ -0-	$ 53,000	$ 100,000
Iowa State College Effect of Ionizing Radiation on the Viability and Antigenicity of Pathogens of Domesticated Animals	17,250	18,000	19,000
Iowa, State University of Radioiodine Studies of Fetal, and Other Thyroids	7,000	8,000	8,000
Iowa, State University of A Quantitative and Morphologic Study of Radiation Induced Cataracts	15,000	17,000	17,000
Jefferson Medical College of Philadelphia The Effect of Embryonic Irradiation on Adult Life Expectancy and Adult Pathology	24,035	18,000	18,000
Johns Hopkins University The Action of Radiation and Other Mutagenic Agents - (1) in Inducing Mutation in Drosophila Females, and (2) in Controlling the Action of a Specific Gene Responsible for Suppressing Uncontrolled Growth	6,500	14,000	15,000
Johns Hopkins University The Effects of Ionizing Radiations on Gene and Chromosome Mutation Rates in Normal Human Cells in Tissue Culture	4,500	28,000	30,000
Johns Hopkins University A. The Transformation of E. coli B from Virus Sensitive to Virus Resistant or Vice Versa; B. Chemical and Nutritional Studies of Bacterial Viruses	18,090	19,000	19,000

Program Activities Related to the Development of Radiation Standards - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(1) Research Related to Radiation Standards of Permissible Exposures (Radiation Biology) - continued			
Off-Site Research Projects - continued			
Johns Hopkins University Biochemical Changes Resulting from Mutations Induced by X-Rays, and Ultraviolet, and Nitrogen Mustard	$ 11,000	$ 12,000	$ 12,000
Johns Hopkins University - McCollum-Pratt Institute A Study of the Effects of Radiation on Bioluminescent Materials	-0-	15,000	15,000
Johns Hopkins University Modification by Supplementary Agents of the Rates of Induced Chromosome and Gene Changes	7,800	9,000	9,000
Johns Hopkins University Studies on the Synthesis of the Phage Chromosome	10,238	20,000	22,000
The Kaiser Foundation Investigation of Energy Transfer in Photochemical Reactions	-0-	14,000	14,000
Kansas, University of Immunological Studies of Radiation-Induced Damage to Biological Systems	9,443	10,000	10,000
Kansas, University of Study of Deposition and Excretion of Bone-Seeking Radioisotopes	19,778	22,000	22,000
Kresge Eye Institute Effects of Neutrons and Other Radiations on the Ocular Lens	18,653	23,000	24,000
Long Island Biological Association The Study of Spontaneous and Induced Genetic Changes in Mammalian Cells Grown in Tissue Cultures.	31,084	32,000	34,000

Program Activities Related to the Development of Radiation Standards - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(1) Research Related to Radiation Standards of Permissible Exposures (Radiation Biology) - continued			
Off-Site Research Projects - continued			
Long Island Biological Association, Inc.			
Adaptive Value of Experimental Populations Exposed to Radiations	$ 28,055	$ -0-	$ -0-
Longwood College			
Effects of Radiation on Root Growth of Higher Plants	7,550	7,000	7,000
Marquette University, School of Medicine			
The Pathological Effects of Radioactive Isotopes of Calcium and Strontium on Bone and Soft Tissue	15,947	16,000	17,000
Marquette University			
Temperature Prevailing During Exposure as a Modifying Factor in the Dose-Response Relationship of X-rayed Mammalian Skin	2,212	-0-	-0-
Massachusetts General Hospital			
Effects of Radioactive Iodine on Biology of the Thyroid Gland	11,085	15,000	15,000
Massachusetts General Hospital			
Studies of the Effects of Ionizing Radiation on the Ultrastructure of Developing Nervous Tissue as Revealed by Electron Microscopy	-0-	28,000	28,000
Massachusetts General Hospital			
The Mechanism of the Destruction of Bone	-0-	20,000	20,000
Massachusetts General Hospital			
Mechanisms of Protein and Nucleic Acid Synthesis, and Their Possible Relationship to Radiation Damage	75,000	88,000	85,000

Program Activities Related to the Development of Radiation Standards - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(1) Research Related to Radiation Standards of Permissible Exposures (Radiation Biology) - continued			
Off-Site Research Projects - continued			
Massachusetts Institute of Technology Radium, and Mesothorium Poisoning, and Dosimetry and Instrumentation Techniques in Applied Radioactivity	$ 160,203	$ 145,000	$ 145,000
Massachusetts Institute of Technology Fundamental Studies on the Effects of Ionizing Radiations on Bacteria	14,000	14,000	14,000
Massachusetts, University of The Effects of Ultraviolet Radiations on the Leakage of Substances from Yeast Cells	6,000	6,000	6,000
Meharry Medical College Autoradiographic Study of the Distribution of Ag^{III} in the Rat Following Administration by Various Routes	5,714	-0-	-0-
Miami, University of, School of Medicine A Study of Factors Affecting the Selective Uptake and Retention of Zn-65 by the Dorsolateral Prostate of the Rat, and a Study of the Role of Zinc in the Physiology of the Prostate	14,231	16,000	16,000
Michigan State University Effects of Irradiation on Developing Plant Embryos	2,625	-0-	-0-
Michigan, University of Effects of Radiation on the Intermediary Metabolism of Mammalian Skin	9,000	-0-	-0-

Program Activities Related to the Development of Radiation Standards - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(1) Research Related to Radiation Standards of Permissible Exposures (Radiation Biology) - continued			
Off-Site Research Projects - continued			
Michigan, University of, Medical School Effects of Irradiation on the Localizing Response (to Antigen) of Different Tissues in Immunity	$ 10,000	$ 10,000	$ 10,000
Michigan, University of Effect of Irradiation on the Pituitary Adrenal Axis	2,500	-0-	-0-
Michigan, University of Studies with an X-Ray Monochromator and X-Ray Irradiation Service Operations	5,000	-0-	-0-
Michigan, University of Resonance in Radiation Effects	-0-	24,000	27,000
Michigan, University of Study of Consanguineous Marriage in Japan	26,876	-0-	-0-
Michigan, University of Development of Information Concerning (1) Human Mutation Rates. (2) The Accumulation of Deleterious Recessive Genes in Human Populations, and (3) The Manner of Action of Selective Factors on Both Contemporary and Primitive Human Populations	82,164	100,000	110,000
Michigan, University of The Biochemical Effects of Radiation. The Effect of Ionizing Radiation on Nucleic Acid Metabolism	37,121	50,000	52,000

Program Activities Related to the Development of Radiation Standards - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(1) Research Related to Radiation Standards of Permissible Exposures (Radiation Biology) - continued			
Off-Site Research Projects - continued			
Minnesota, University of Effect of Ionizing Radiation Upon Tissue Metabolism	$ 20,000	$ -0-	$ -0-
Minnesota, University of The Cause and Nature of an Aplastic Anemia of the Bovine and Its Relation to Radiation Pancytopenia	21,375	22,000	22,000
Mississippi, University of Chromosome Breakage in Oocytes of Drosophila	8,694	-0-	-0-
Montana State University 1. The Morphogenesis of Radiation-Induced Tumors in Graptopetalum paraguayehse. 2. The Effect of Chronic and Acute Ionizing Radiation on the Growth of Kalanchoë "Brilliant Star"	-0-	11,000	11,000
Montefiore Hospital, New York 1. Dynamics of Strontium Distribution in the Body 2. Strontium and Calcium Metabolism in Humans	38,792	25,000	20,000
U. S. Naval Radiological Defense Laboratory (2) Effects of Modifying Agents and Dose Fractionation on Radiation-Induced Leukemia, Nephrosclerosis and Other Late Lesions (3) The Physiology of Aging in the Hypocalorically Age Retarded Rat	24,527	-0-	-0-
Nebraska, University of Effects of Cranial X-Irradiation on Psychological Processes in Rats	9,386	10,000	10,000
Nebraska, University of Effects of X-Rays and Thermal Neutrons on Plant Metabolism	5,985	7,000	7,000

Effects of X-Rays and Thermal Neutrons on Plant Metabolism	5,985	7,000	7,000

Program Activities Related to the Development of Radiation Standards - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(1) Research Related to Radiation Standards of Permissible Exposures (Radiation Biology) - continued			
Off-Site Research Projects - continued			
New England Deaconess Hospital The Effects of Ionizing Radiation on the Developing Mammalian Nervous System	$ 12,100	$ 18,000	$ 20,000
New England Deaconess Hospital Acute and Chronic Radiation Injury	220,018	250,000	260,000
University of New Hampshire			
Radiation Induced Viability Mutations in the Honey Bee	-0-	9,000	10,000
New Jersey State Department of Health Epidemiological Investigation of the Radium Dial Painters	4,500	-0-	-0-
New Jersey State Department of Health Epidemiological Investigation of the Radium Dial Painters	-0-	70,000	70,000
New York Society for the Relief of the Ruptured and Crippled, Margaret M. Caspary Clinic, Hospital for Special Surgery			
A Study of Demineralization with Special Reference to Scoliosis	-0-	25,000	25,000
New York, State University of, Research Foundation Effects of Irradiation on the Calcifying Mechanisms of Epiphyseal Cartilage	18,252	8,000	9,000
New York University - Bellevue Medical Center Aerosol Retention Studies	2,125	-0-	-0-

Program Activities Related to the Development of Radiation Standards - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(1) Research Related to Radiation Standards of Permissible Exposures (Radiation Biology) - continued			
Off-Site Research Projects - continued			
New York University, College of Medicine Histochemical Studies of Metabolic Alterations in Rats Receiving Lethal and Sublethal Doses of Radiation, with Emphasis on Terminal Vascular Bed	$ 15,310	$ 16,000	$ 16,000
North Carolina, University of The Double Isotope Effect of Ca-45 and Sr-89 on the Pattern of Distribution in the Body, Particularly in Bone	5,240	-0-	-0-
North Carolina, University of A Study of Genetic Recombination as Influenced by Mutagenic and Non-mutagenic Environmental Agents	7,928	8,000	8,000
North Texas State College Some Studies on the Changes in the Activity of X-Irradiated Nerves	5,132	11,000	11,000
Northwestern University, Medical School The Effects of Irradiation on Renal Transport Systems	9,950	12,000	12,000
Northwestern University Studies on the Radiation Genetics of *Drosophila melanogaster* Females	7,500	8,000	8,000
Northwestern University, Medical School The Effects of Radiation on Pure Proteins and Nucleic Acids	12,907	15,000	16,000
Notre Dame, University of Mechanisms Involved in the Actions of Radiations on Living Cells	13,165	15,000	15,000
Oklahoma, University of, Research Institute The Cytology and Genetics of Radiation Resistance in Bacteria	7,644	8,000	8,000

The Cytology and Genetics of Radiation Resistance in Bacteria	7,644	8,000	8,000

Program Activities Related to the Development of Radiation Standards - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(1) Research Related to Radiation Standards of Permissible Exposures (Radiation Biology) - continued			
Off-Site Research Projects - continued			
Oregon State College Accelerated Evolution in the Alkali Bee Nomia melanderi through Irradiation	$ -0-	$ 6,000	$ 6,000
Oregon, University of Investigations in Population Genetics and Ecology	2,800	-0-	-0-
Pavia, University of - Pavia, Italy Mutation Rates and Mutational Loads in Man	-0-	20,000	22,000
Pennsylvania, University of The Internal Organization of Normal and Phage-Infected Cells as Influenced by Radiation	13,000	13,000	14,000
Pennsylvania, University of Mutation Rates in Mormoniella	6,750	15,000	15,000
Philadelphia General Hospital The Effect of X-Ray Radiation of the Lipids of the Skin	14,800	15,000	10,000
Pittsburgh, University of A Study of Physiological Aging in Cells in Tissue Culture	14,835	16,000	16,000
Pittsburgh, University of Lung Hazards from Inhaled Radioactive Particulate Matter	27,700	30,000	30,000
Pittsburgh, University of The Study of the Effects of Radiation on the Immune Response	35,000	37,000	40,000
Pittsburgh, University of Study of the Correlation of Radiation Effects with Physical and Chemical Changes in Viruses	13,932	15,000	15,000

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Program Activities Related to the Development of Radiation Standards - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(1) Research Related to Radiation Standards of Permissible Exposures (Radiation Biology) - continued			
Off-Site Research Projects - continued			
Pittsburgh, University of Genetic Potential of Certain populations of Drosophila persimilis from the Sierra Nevada of California	$ 9,750	$ 11,000	$ 11,000
Reed College The Effect of Ionizing Radiation on Biochemical Compounds	-0-	7,000	7,000
The Retina Foundation A Comparison of the Fine Structure of the Normal and the Irradiated Lens	5,430	6,000	6,000
Rochester, University of The Effects of Ionizing Radiations on Peripheral Nerve	-0-	10,000	10,000
Rochester, University of Individual Response to Ionizing Radiation in Animals and Patients	40,000	45,000	50,000
Rochester, University of Dynamic Aspects of Skeletal Metabolism	25,500	20,000	20,000
Roscoe B. Jackson Memorial Laboratory Quantitative Population Genetics of Mice Under Irradiation	11,100	24,000	25,000
Roscoe B. Jackson Memorial Laboratory The Occurence of Chromosomal Aberrations in Pre-Spermatocytic Cells of Irradiated Male Mice	10,500	18,000	18,000
Roscoe B. Jackson Memorial Laboratory Physiological Studies on Induced Congenital Deformities in Mice	14,606	15,000	15,000

Program Activities Related to the Development of Radiation Standards - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(1) Research Related to Radiation Standards of Permissible Exposures (Radiation Biology) - continued			
Off-Site Research Projects - continued			
Roscoe B. Jackson Memorial Laboratory Attempt to Delineate Inborn Anemias in Mice	$ 12,955	$ 28,000	$ 28,000
Roscoe B. Jackson Memorial Laboratory Rate of Recovery from Radiation Injury and Its Relationship to After Expectation of Life in Mice	-0-	27,000	29,000
Rutgers, the State University Histological and Physiological Effects of Irradiation on Plant Tissue	8,475	11,000	11,000
St. Louis University Spontaneous and Radiation Mutagenesis and Mutability in Drosophila	-0-	11,000	13,000
Sloan Kettering Institute for Cancer Research - Memorial Hospitals Biological Effects of Radiation, and Related Biochemical and Physical Studies	236,853	250,000	250,000
Smithsonian Institution A Study of the Biochemical Effects of Ionizing and Nonionizing Radiation on Plant Metabolism During Development	-0-	24,000	28,000
Smithsonian Institution A Biochemical Investigation of Radiant Energy as it Affects Photomaturation in Green Plants	9,600	10,000	10,000
Smithsonian Institution Specific Biological Indicators of Ionizing Radiation and the Mechanism of Its Action	14,904	15,000	15,000

Program Activities Related to the Development of Radiation Standards - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(1) Research Related to Radiation Standards of Permissible Exposures (Radiation Biology) - continued			
Off-Site Research Projects - continued			
Southern California, University of Effect of Radiation on Intestinal Absorption and Metabolism of Fats and Carbohydrates	$ 24,736	$ 25,000	$ 25,000
Southern California, University of A Study of the Effect of X-Radiation on the Absorption of Glycerides Utilizing Tracer Technique	1,250	-0-	-0-
Southern Illinois University The Effects of X-Rays and Ultraviolet Radiations on the Multiple Manifestations of a Gene Together with Genetical Analysis of the Radiation Induced Variations and the Effects of Extracts from Unirradiated Cells on the Repair of the Genotypes of Irradiated Cells	11,200	12,000	12,000
Stanford University, School of Medicine Secretion of Calcium and Strontium by the Salivary Glands	3,500	8,000	8,000
Syracuse University Effects of X-Radiation Upon the Renal-Endocrine System	38,684	25,000	25,000
Syracuse University Production of Asporogenic Mutants. A Study of the Biochemistry of Bacterial Sporogenesis with Asporogenic Variants	7,000	10,000	10,000
Syracuse University The Use of Ionizing Radiation for Studying Enzyme Complexes	4,140	9,000	9,000
Tennessee, University of The Response of the Reticulo-Endothelial System to X-Irradiation	5,616	6,000	7,000

Program Activities Related to the Development of Radiation Standards - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(1) Research Related to Radiation Standards of Permissible Exposures (Radiation Biology) - continued			
Off-Site Research Projects - continued			
Tennessee, University of, College of Medicine A Study of the Effects of Cobalt-60 Gamma Irradiation on Infection and Immunity	$ 8,933	$ 10,000	$ 11,000
Tennessee, University of A Survey of the Effects of Radiation on Host-Parasite Relations	8,694	10,000	10,000
Tennessee, University of, College of Medicine The Metabolism of Alkaline Earth Metals by Bone	8,610	9,000	10,000
Tennessee, University of Studies in Iodide Metabolism	22,545	25,000	25,000
Texas Technological College The Effects on Rat Behavior of Developmental Aberrations Induced by Ionizing Radiation in utero	7,478	-0-	-0-
Texas, University of, M. D. Anderson Hospital and Tumor Institute The Effects of Radiations on the Genetic System of Organisms in Relation to Their Physiological and Biochemical Systems	11,000	13,000	13,000
Texas, University of Direct and Indirect Effects of Radiation on Genetic and Developmental Systems of Vertebrates	7,356	15,000	15,000
Texas, University of - M. D. Anderson Hospital and Tumor Institute The Role of Nucleic Acid in Amino Acid Incorporation into Protein and in Enzyme Synthesis, and Genetic Implications	12,000	13,000	13,000

Program Activities Related to the Development of Radiation Standards - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(1) Research Related to Radiation Standards of Permissible Exposures (Radiation Biology) - continued			
Off-Site Research Projects - continued			
Texas, University of, Southwestern Medical School Ultraviolet Irradiation Damage in Living Cell Systems	$ -0-	$ 16,000	$ 17,000
Texas, University of Effects of Various Types of Irradiation on Growth Responses, Metabolism and Electrical Pattern of the Avena coleoptiles and Earthworms	6,000	-0-	-0-
Texas, University of Physical and Radiobiological Investigations with 22 MEV X-Rays and Electrons, as Compared with Cobalt-60 and 250 Kv X-Rays	17,100	22,000	25,000
Texas, University of Research on Direct and Indirect Effects of Radiations on the Genetic Systems of Organisms	34,560	35,000	35,000
Texas, University of The Genetic and Biochemical Effects of Radiation on Bacteria	6,000	6,000	6,000
Tufts College Study of the Effects of Radiation on Growth	33,500	34,000	34,000
Utah, University of Toxicity Studies of Plutonium and Other Radioactive Substances in Animals	297,312	325,000	325,000
Utah, University of Radiation Effects on the Photosynthesis and Metabolism of Higher Plants	15,477	16,000	16,000

Program Activities Related to the Development of Radiation Standards - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(1) Research Related to Radiation Standards of Permissible Exposures (Radiation Biology) - continued			
Off-Site Research Projects - continued			
Utah, University of Metabolism of Trace Elements in Animals and Man with Special Reference to Their Role in Erythropoiesis	$ 11,000	$ 12,000	$ 12,000
Vanderbilt University Study of the Absorption and Metabolism of Lipids and Vitamins, and the Alterations Which Occur in Acute Radiation Injury	26,000	27,000	27,000
Vanderbilt University Fetal Irradiation and the Patterns of Behavior Development	7,500	8,000	8,000
Medical College of Virginia A Study of the Comparative Effects of Ionizing Radiation and Aging on the Mammalian Lens of the Eye	-0-	36,000	36,000
Virginia, University of Radiation Effects on Growing Plants	8,118	12,000	12,000
Washington, State College of A Study of Factors that Govern Radiosensitivity in Plants	12,053	13,000	14,000
Washington University A Study of the Uptake, Turnover, and Metabolism of Chemical Constituents of Bone	-0-	7,000	7,000
Washington, University of Investigations of Physiological Capabilities of the Hematopoietic and Other Cell Turnover Systems in Mice Exposed to Low-Level Chronic Ionizing Radiation	-0-	14,000	14,000

Program Activities Related to the Development of Radiation Standards - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(1) Research Related to Radiation Standards of Permissible Exposures (Radiation Biology) - continued			
Off-Site Research Projects - continued			
Washington, University of			
The Metabolism of Molecular Hydrogen, Deuterium and Tritium	$ 4,800	$ 10,000	$ 11,000
Wayne State University, School of Medicine			
Studies on the Effects of Maternally Administered Phosphorus-32 on Foetal and Post-Natal Development of the Rat	7,500	-0-	-0-
Wayne State University			
The Response of the Respiratory Tract and Lung to Inhaled Stable and Radioactive Isotopes of Certain Elements	10,743	86,000	90,000
Western Reserve University			
Investigations of the Biological Effects of Internally Deposited Radioisotopes and Related Radiobiology Studies	198,000	200,000	210,000
Wisconsin, University of			
Genetic Effects of Cumulative Irradiation in Rats	-0-	21,000	23,000
Wisconsin, University of			
Mitochondrial Electron Transport and Oxidative Phosphorylation	21,450	21,000	25,000
Wisconsin, University of			
The Effect of Various Forms of Irradiation of the Brain on Learned and Unlearned Behavior of Monkeys and Chimpanzees	29,500	20,000	20,000
Wistar Institute of Anatomy and Biology			
Effects of Deuterium on Normal and Virus Infected Tissue Culture Cells	-0-	10,000	12,000

Program Activities Related to the

Program Activities Related to the Development of Radiation Standards - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(1) Research Related to Radiation Standards of Permissible Exposures (Radiation Biology) - continued			
Off-Site Research Projects - continued			
Worcester Foundation for Experimental Biology Effects of Co-60 Irradiation on Rabbit Eggs in vitro	$ 16,500	$ 13,000	$ 13,000
Worcester Foundation for Experimental Biology The Separation and Characterization as Regards Radiation Sensitivity of the Proteins of Lymphoid Tissue	15,000	15,000	15,000
Worcester Foundation for Experimental Biology Investigation of the Effects of Radiation on the Biosynthesis and Metabolism of Adrenocortical Steroids	59,460	60,000	60,000
Yale University Relationship of Genes to Biochemical Reactions in Neurospora	25,000	25,000	25,000
Yale University Investigations on the Cytogenetic Effects of Radiations	17,800	18,000	19,000
Yale University Radiation Effects on Mammalian Chromosomes in Tissue Cultures	25,866	23,000	25,000
Yale University Irradiation of Viruses and Large Molecules	19,138	32,000	34,000
Yerkes Laboratory of Primate Biology, Inc. Behavioral Effects of Ionizing Radiation on Chimpanzees of Various Ages	4,607	-0-	-0-
Fund Estimate for Planned Additional or Expanded Project Requirements	---	340,000	1,100,000
Sub-Total	$ 4,016,843	$ 5,481,000	$ 6,452,000

Program Activities Related to the Development of Radiation Standards - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(1) Research Related to Radiation Standards of Permissible Exposures (Radiation Biology) - continued			
On-Site Research Projects			
Argonne Cancer Research Hospital			
Metabolism of Bone-seeking Radioelements and Means of Modifying Radiation Effects	$ 16,102	$ 47,000	$ 47,000
Biological Studies and Therapeutic Application of High Energy Sources	73,101	56,000	56,000
Immunologic and Metabolic Studies in Tumor Growth	93,733	86,000	96,000
Study of the Biologic Activity of Nucleic Acids	68,670	78,000	87,000
Effects of Ionizing Radiation on Protein Metabolism in Humans and Experimental Animals	68,670	137,000	136,000
Clinical and Fundamental Studies on the Normal and Neoplastic Thyroid	140,125	136,000	145,000
Tissue Culture of Normal and Abnormal Blood-Forming Tissue	-0-	44,000	44,000
Preparation of Radiopharmaceuticals for Patient Administration	16,102	21,000	21,000
Argonne National Laboratory			
Measurements of Radioactive Isotopes in vivo	13,689	15,000	16,000
Fundamental Studies on the Dynamics of Radioelement Metabolism in Humans and Animals	33,816	41,000	43,000

Program Activities Related to the
Development of Radiation Standards - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(1) Research Related to Radiation Standards of Permissible Exposures (Radiation Biology) - continued			
On-Site Research Projects - continued			
Argonne National Laboratory - continued			
Studies on the Determination of Beta and Gamma Ray Emitting Isotopes in Humans, Animals and Foods	$ 285,559	$ 446,000	$ 447,000
Fundamental Studies on Tumor Induction Utilizing Known and Potential Carcinogens (e.g. plutonium)	64,546	85,000	111,000
General Biological Research Including Environmental Studies on Animal Colonies, and Research in Animal Disease Control Necessary to Conduct Proper Lifetime Studies on Laboratory Animals	54,154	40,000	49,000
Cell Division is Being Studied in Many Kinds of Biological Organisms. The Aim is to Determine the Importance of Radiation Alterations in Cell Division in Radiation Toxicity	21,275	27,000	34,000
Skeletal Metabolism of Essential Trace Elements	8,078	9,000	9,000
Non-ionizing Radiations are Being Investigated Largely Through Use of the Argonne Spectrograph, Particularly to Discover Similarities of Their Action (Mutations and Cell Death) to Those of the Ionizing Radiations, Which Yield Their Energy to Tissue Through a Different Physical Effect	54,152	70,000	89,000
Studies Primarily Concerned with the Metabolism of Nucleic Acids, Vitamins, Amino Acids, Proteins and General Cellular Processes. Particular Emphasis is Placed on Those Projects Which Yield Information Required for Understanding the Biochemical and Physiological Changes Resulting from Radiation Exposure or Which May be Useful for Treatment of Radiation Damage	462,155	697,000	881,000

Program Activities Related to the Development of Radiation Standards - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(1) Research Related to Radiation Standards of Permissible Exposures (Radiation Biology) - continued			
On-Site Research Projects - continued			
Argonne National Laboratory - continued			
Cell Particulates, the Microscopic and Sub-microscopic Parts in Cells, are the Subject of a Systematic Series of Investigations to Find out Which of Them Participates, and to What Extent, in Radiation Damage. Studies Include Efforts to Improve Chemical, Optical, and Electron Microscopic Methods of Investigation	$ 186,633	$ 231,000	$ 253,000
Bone Resorption Studies in Rats	-0-	-0-	5,000
The Life Shortening and Carcinogenic Effects of Whole Body X and Gamma Irradiation are Measured in Relation to Genetic, Physiological, and Dose Rate Variation. The Accumulation of Radiation Induced Injury is Evaluated to Establish Radiation Standards (25 Projects)	315,245	416,000	545,000
Comparison Between the Effectiveness of Neutrons and Gammas and the Necessary Therapy in Each Case is Required for Evaluating the Hazards of Industrial Reactor Operation as Well as Weapons Detonations (15 Projects)	155,688	167,000	192,000
Atomic Bomb Casualty Commission			
Population Studies; Japanese Demographic and Public Health Statistics	25,150	35,000	41,000
Genetic Studies; Developmental Abnormalties in Japanese Children	56,991	79,000	85,000
Medical Surveillance of In Utero and Other Exposed Cases	179,335	225,000	273,000

Program Activities Related to the Development of Radiation Standards - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(1) Research Related to Radiation Standards of Permissible Exposures (Radiation Biology) - continued			
On-Site Research Projects - continued			
Atomic Bomb Casualty Commission - continued			
Mortality Studies, Pathological and Autopsy Analyses Among Japanese	$ 435,883	$ 513,000	$ 657,000
Morbidity Studies; Japanese Adult Medical Surveillance	504,408	611,000	727,000
Study of Disease that May be Influenced by Radiation	209,165	190,000	226,000
Special Studies of Steroid, Hormones and Enzymes Metabolisms	37,361	52,000	54,000
Brookhaven National Laboratory			
The Biological Effects of Neutrons, Including its RBE, Penetration and Absorption in Tissues and Potential Therapeutic Usefulness by B-10 Capture	463,000	500,000	650,000
A Study of the Biological Effects of High Energy Particles Using the Very High Energy Equipment at BNL	91,000	95,000	120,000
A Study of the Irradiated Intestine; the Changes in Secretion, Absorption, Membrane Integrity and Cellular Growth	93,000	95,000	130,000
Carcinogenic Activity of Local B-Ray Skin Doses	45,000	49,000	50,000
The Marshallese Survey Conducted Annually by Personnel From BNL and Elsewhere	86,000	85,000	85,000

Program Activities Related to the Development of Radiation Standards - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(1) Research Related to Radiation Standards of Permissible Exposures (Radiation Biology) - continued			
On-Site Research Projects - continued			
Brookhaven National Laboratory - continued			
Eight Main Projects are Devoted to Defining the Genetic, Physiological, Cellular and Macromolecular Factors Important in Radiation Damage. By Comparing Effects in Both P ants and Animals an Attempt is Made to Determine if There Are Mechanisms of Radiation Damage Common to All Biological Systems. The Development of this Fundamental Information is Prerequisite to the Establishment of Basic Concepts of Radiation Injury	$ 834,000	$ 900,000	$ 980,000
University of California at Los Angeles			
The Effect of Radiation on Viruses and Bacteriophage; the Effect of Radiation on Macromolecules	111,463	130,000	125,000
The Effects of Radiation on the Nervous System	147,396	100,000	110,000
The Effects of Radiation on the Function of the Gastro-Intestinal Tract	77,673	80,000	90,000
The Effects of Radiation on Subcellular Particles; Effects of Radiation on the Lipoproteins of Radiated Animals; Effects of Radiation on the Life Span of Rats; Effects of Radiation on Animals Exposed to High and Low Concentrations of Oxygen	164,845	164,000	188,000
Pathological Studies of Radiation Effects (Service)	61,230	64,000	91,000
Effects of Radiation on the Incidence Rates of Leukemia in the Mouse	76,405	76,000	84,000

Program Activities Related to the Development of Radiation Standards - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(1) Research Related to Radiation Standards of Permissible Exposures (Radiation Biology) - continued			
On-Site Research Projects - continued			
General Electric Company - Hanford			
Thirteen Projects are Devoted to Studies on the Deposition and Metabolism in Plants and Animals of Fission Products and Other Radioisotopes. A Large Fraction of the Effort is Directed Toward Development of Safe Methods for the Production of Plutonium	$ 1,047,638	$ 1,062,000	$ 1,090,000
Lawrence Radiation Laboratory			
Medical Research; Biochemical and Physiological Response	129,880	172,000	196,000
Medical Research; Detrimental Chemical and Physiological Effects	55,663	62,000	63,000
An Evaluation of the Effect of Maternal Influences, Radiation Energy and Type of Radiation on Mutation Yield	37,109	28,000	29,000
Los Alamos Scientific Laboratory			
Twenty-six Projects are Devoted to Research on the Biochemical Effects of Radiation, the Metabolism of Fission Products and Their Removal from the Body, the Relative Biological Effectiveness of Different Types of Chronic Irradiation, Preparation of Radio-Actively Labeled Compounds and of Chemical Radiation Dosimeters, and Use of the Whole Body Counter in Surveying the Human Population and the U. S. and Canadian Milk Supply for Cesium-137 as Well as in Clinical and Diagnostic Techniques in Humans	912,944	955,000	978,000

Program Activities Related to the Development of Radiation Standards - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(1) Research Related to Radiation Standards of Permissible Exposures (Radiation Biology) - continued			
On-Site Research Projects - continued			
Oak Ridge Institute of Nuclear Studies			
Effects of Ionizing Radiation on Tissue	$ 33,727	$ 37,000	$ 42,000
Oak Ridge National Laboratory			
Biological Research; Several Major Programs are Designed to Detail the Genetic Effects of Radiation Including the Induction of Mutations and the Study of Their Detrimental Effects. Other Efforts Include the Estimation of Life Shortening and Leukemia Induction that Follows Prompt and Protracted Irradiation (13 Projects)	2,286,418	2,579,000	2,699,000
Experimental and Theoretical Investigations into the Fundamental Interaction of Radiation and Matter	390,090	413,000	413,000
University of Rochester			
The Effects of Radiation on the Biosynthesis of Hemoglobin	51,300	50,000	52,000
Study of the Morphological and Physiological Alterations of the White Cells Using Normal and Irradiated Animals	22,762	29,000	28,000
Biological Effects of Inhaled Radioactive Materials	228,781	242,000	264,000
Metabolism of Bone Deposition, Mobilization and Transport of Radioelements	107,101	118,000	136,000

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Program Activities Related to the Development of Radiation Standards - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(1) Research Related to Radiation Standards of Permissible Exposures (Radiation Biology) - continued			
On-Site Research Projects - continued			
University of Rochester - continued			
Induction of Tumors with Radioactive Agents	$ 53,556	$ 82,000	$ 44,000
Inhalation Studies of Fission Products	4,284	4,000	5,000
The Biological Effects of Internally Deposited Radioisotopes, Radiation Toxicology Studies in Rats	41,092	48,000	50,000
The Chronic Effects of Sr-90 in the monkey	33,884	45,000	46,000
Measurement of Whole Body Radioactivity	33,427	40,000	40,000
Drosophila melanogaster as a Tool in Radiobiologic and Toxicologic Investigation	7,239	8,000	9,000
Fate of Radium and Its Daughter Products	19,990	19,000	22,000
Aerosol Problems	44,713	44,000	47,000
Studies on the Hemorrhagic State and Metabolism of Irradiated Animals	24,500	21,000	20,000
Four Projects are Devoted to the Estimation of Recovery from Radiation Injury and Therapeutic Methods. One Project is Directly Testing the Validity of Radiation Standards Presently in Operation	237,098	240,000	242,000
Toxicological Studies of Elements and Compounds of Interest to the AEC	88,426	89,000	95,000
Sub-Total	$12,721,429	$13,279,000	$14,682,000

Program Activities Related to the Development of Radiation Standards

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(2) Research Related to Radiation Standards Involving Environmental Contamination and Waste Disposal -	$2,708,122	$4,064,000	$4,499,000
Off-Site Projects			
Agriculture, U. S. Department of, Agricultural Research Service			
Equipment and Methods for Decontamination of Agricultural Lands Contaminated by Radioactive Fallout	$ -0-	$ 25,000	$ 25,000
Agriculture, U. S. Department of, Forest Service			
Obtaining Accurately Dated and Located Turpentine Samples for the Contemporary Natural C-14 Survey.	-0-	9,000	9,000
Agriculture, U. S. Department of			
Accumulation and Movement of Fission Products in Soils and Plants	35,689	90,000	125,000
Arizona, University of			
Utilization of Phosphorus from Biological Material and Uptake of Strontium by Various Type Crops	5,784	6,000	6,000
Bermuda Biological Station for Research, Inc.			
The Plankton Ecology and Related Chemistry and Hydrography of the Sargasso Sea	17,205	17,000	17,000
Brown University			
Further Investigations Concerning the Effects of Cosmic Ray Heavy Nuclei and of Microbeams on Mammalian Skin	9,500	10,000	10,000
California, University of			
Studies on the Effects of Air Ions on Microorganisms	3,830	8,000	8,000
California, University of			
Study of the Decontamination of Soils Containing Radioactive Elements and Salts	16,728	20,000	21,000

Program Activities Related to the Development of Radiation Standards - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(2) Research Related to Radiation Standards Involving Environmental Contamination and Waste Disposal - continued			
Off-Site Projects - continued			
California, University of Study of the Internal or Metabolic Factors and the External or Environmental Factors Affecting Ion Absorption by Plants	$ 9,678	$ 10,000	$ 11,000
California, University of - L. A. Study of the Behavior of Certain Synthetic Chelating Agents in Biological and Soil Systems	-0-	11,000	11,000
California, University of, Scripps Institution of Oceanography A Study of the Transfer of Heavy Metals from Seawater to Sediments, with Emphasis on Members of the Natural Radioactive Series	-0-	35,000	35,000
California, University of, Scripps Institution of Oceanography Investigation of Mixing Processes in the Atmosphere and Sea Utilizing Stable and Radioactive Isotopes	-0-	65,000	65,000
Central Michigan College of Education A Study of the Ecology and Taxonomy of Fresh-water Midges (Tendipedidae: Diptera) of Michigan with Special Reference to their Role in the "Turnover" of Radioactive Substances in the Hydrosol	10,501	12,000	13,000
Chicago, University of Research on Tritium in the Atmosphere	15,000	25,000	25,000
Colorado, University of Ecology of Selected Stands on the East Slope of the Front Range in Colorado	27,200	30,000	30,000

Program Activities Related to the Development of Radiation Standards - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(2) Research Related to Radiation Standards Involving Environmental Contamination and Waste Disposal - continued			
Off-Site Projects - continued			
Columbia University Pathological Effects in Pacific Reef Fishes Exposed to Radioactive Fallout	$ 9,614	$ 10,000	$ 5,000
Columbia University, Lamont Geological Observatory Studies of Circulation of the Deeper Oceanic Water	49,700	55,000	60,000
Connecticut, University of Ion Interactions in Plant Tissues	2,500	6,000	6,000
Cornell University Fission Product Metabolism and Response in Laboratory and Domestic Animals	15,000	45,000	60,000
Dartmouth College Material Transfer Between Plants through Adjacent Root Systems	3,450	8,000	9,000
Duke University The Path of Radial Movement of Minerals from Soil to Xylem in Roots	2,712	3,000	4,000
Emory University Long Range Effects of Radiation on Natural Populations and Communities of the Granite Outcrops	12,708	13,000	13,000
Emory University Ecological Effects of Radiation at the Lockhead Reactor Site	13,432	35,000	50,000

Program Activities Related to the Development of Radiation Standards - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(2) Research Related to Radiation Standards Involving Environmental Contamination and Waste Disposal - continued			
Off-Site Projects - continued			
Florida, University of			
Rates, Amounts, Nuclide Origin and Effects of Radiation Acquired by Fresh Water Microorganisms Under Experimental Control	$ 10,860	$ 11,000	$ 12,000
Georgia, University of			
Study of the Ecological Change on the AEC Savannah Area Through the Use of Indices for Total Community Function and Measurements of the Biomass of Key Populations	20,500	18,000	18,000
Hawaii, University of, Hawaii Marine Laboratory			
Investigation of the Possible Effects of Ingestion of Radioactive Fish and the Nature and Biology of Toxins Found in Certain Fishes	-0-	12,000	13,000
Hawaii, University of, Eniwetok Marine Biological Laboratory			
Technical and Administrative Functions of Eniwetok Marine Biological Laboratory Operation	24,275	15,000	25,000
Hawaii, University of - Hawaii Marine Laboratory			
Radioisotope Uptake in Marine Organisms with Special Reference to the Passage of such Isotopes as are Liberated from Atomic Weapons through Food Chains Leading to Organisms Utilized as Food by Man	35,816	37,000	37,000
Interior, Department of, Fish and Wildlife Service			
Accumulation of Fission Products by Marine Fish and Shellfish	23,999	45,000	-0-

Program Activities Related to the Development of Radiation Standards - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(2) Research Related to Radiation Standards Involving Environmental Contamination and Waste Disposal - continued			
Off-Site Projects - continued			
Jefferson Medical College of Philadelphia Effects of Radioactive Particulates in Lung Tissue	$ 22,000	$ 22,000	$ 22,000
Johns Hopkins University Cell Membrane Permeability and Accumulation of Ions	15,180	16,000	16,000
The Johns Hopkins University Fundamental Investigations of the Behavior of Aerosols	6,613	15,000	15,000
Kent State University Study of Changes in Certain Biotic Communities and Animal Populations Through Field Investigations	2,642	5,000	5,000
Marine Biological Laboratory Studies on the Physiology of Marine Organisms Using Radioisotopes	10,160	12,000	13,000
Massachusetts Institute of Technology, Boston, Massachusetts Planetary Transport Processes in the Stratosphere	-0-	30,000	30,000
Meteorology Research, Inc. Pasadena, California Cloud Electrification Study	-0-	15,000	15,000
Michigan State University An Investigation of the Ecological Factors Related to the Distribution of Algae in an Andean Transect at the Equator	3,394	-0-	-0-

Program Activities Related to the
Development of Radiation Standards - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(2) Research Related to Radiation Standards Involving Environmental Contamination and Waste Disposal - continued			
Off-Site Projects - continued			
Michigan State University of, Agriculture and Applied Science I. The Absorption and Utilization of Radioactive Minerals Applied to the Leaves of Plants II. The Absorption and Utilization of Ruthenium by Plants III. The Leaching of Nutrients from Leaves of Plants	$ 24,950	$ 26,000	$ 27,000
Michigan, University of - Medical School Effect of I-131 on the Fetus	-0-	18,000	20,000
Mississippi Southern College The Composition of the Plankton Communities Occurring Along a Salinity Gradient	2,933	-0-	-0-
Missouri, University of The Mechanism of Active Absorption of Radio-isotopes by Plant Cells	-0-	3,000	4,000
National Academy of Sciences - ONR Committee on Oceanography	11,250	10,000	10,000
U. S. Navy, Bureau of Ships Examination of Marshallese (People)	13,330	-0-	-0-
Navy, U. S. Department of, Office of Naval Research Support for Oceanographic Research in the Eniwetok Area	2,300	-0-	-0-
Navy, U. S., Office of Naval Research Studies on the Effects of Natural and Artificial Radioactivity on the Electrical Properties of the Atmosphere	-0-	15,000	15,000

Program Activities Related to the Development of Radiation Standards - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(2) Research Related to Radiation Standards Involving Environmental Contamination and Waste Disposal - continued			
Off-Site Projects- continued			
New Mexico Highlands University Selective Demuding Action of Atomic Radiation on Vegetation on Some Nevada Sites and Gross Changes in the Surviving Flora	$ 19,800	$ 35,000	$ 50,000
New York University Tissue Reactions to Intrapulmonary Radiation	7,500	15,000	15,000
New York University Determination of the Variable Concentrations of Trace Elements in Human Tissue and Body Fluids	21,000	21,000	21,000
Nevada, University of A. The Relation of Fallout and Other Factors to Soil and Plant Contamination and the Assimilation of Fission Products and Other Materials by Range Livestock. B. Radiochemical Analyses Associated with Part A.	22,500	35,000	25,000
North Carolina State College Studies in the Ecology of Fresh-water Algae in North Carolina	5,775	10,000	10,000
North Carolina State College of Agriculture and Engineering The Mineral Composition of Plants Grown on Southeastern Soils as Related to Soil Characteristics, Lime, and Fertilizer Additions and Root Distributions, with Particular Emphasis on Sr-90	6,264	13,000	13,000
North Carolina State College of Agriculture and Engineering The Genetic and Developmental Effects of Ingested Radioactives in Habrobracon	5,101	7,000	7,000

Program Activities Related to the
Development of Radiation Standards - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(2) Research Related to Radiation Standards Involving Environmental Contamination and Waste Disposal - continued			
Off-Site Projects - continued			
Northwestern University Study of Citric Acid Metabolism in Relation to the Skeleton and Endocrine Factors Affecting the Skeleton	$ -0-	$ 21,000	$ 23,000
Oregon State College The Effects of Soil Temperature and Morphological Age of Plants on the Uptake and Assimilation of Radioactive Phosphorus	-0-	8,000	-0-
Oregon State College Relationship of Soil Properties to Chemical Reactions and Other Phenomena Involving Sulfur	7,000	8,000	8,000
Ohio Agricultural Experiment Station Detailed Characterization of Soil and Vegetation on Selected Sites to Serve as Basis for Future Evaluations of Effects of Radioactive Contamination	29,655	33,000	35,000
Ohio Agricultural Experiment Station Bioclimatic and Soils Investigations in Forest Environments	21,278	25,000	25,000
Polytechnic Institute of Brooklyn Determination of Activity of Alkaline Earth Cations in Biological and Other Aqueous Media by Means of Multilayer Membrane Electrodes	-0-	15,000	16,000
University of Puerto Rico Agricultural Experiment Station Radioactive Iron Studies with Soils and Crops of Puerto Rico	-0-	5,000	5,000

Program Activities Related to the Development of Radiation Standards - Continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimate FY 1960
(2) Research Related to Radiation Standards Involving Environmental Contamination and Waste Disposal - continued			
Off-Site Projects - continued			
Rochester General Hospital A Study of the Uptake, Turnover, and Metabolism of the Chemical Constitutents of Bone	$ 2,052	$ -0-	$ -0-
South Carolina, University of Research Concerning the Accumulation of Radioactivity by Plants	-0-	5,000	5,000
South Carolina, University of An Ecological Study of the Flora and Fauna of the Savannah River Plant Area	13,483	-0-	-0-
South Carolina, University of An Ecological Study of the Fishes of the Savannah River Drainage	15,453	15,000	15,000
South Carolina, University of An Ecological Study of the Vascular Shore Plants of the PAR Reservoir, Savannah River Plant	-0-	9,000	9,000
South Dakota State College Metabolism of Selenium and Sulphur in Plants	4,772	5,000	5,000
Southern California, University of The use of Radio Iron to Determine the Mechanisms of Utilization and Transport of Iron in the Mammalian Organism	9,600	11,000	11,000
Southern Research Institute Body Retention of Carbon-14	10,000	11,000	11,000

Program Activities Related to the Development of Radiation Standards - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(2) Research Related to Radiation Standards Involving Environmental Contamination and Waste Disposal - continued			
Off-Site Projects - Continued			
Stanford University, George Vanderbilt Foundation Marine Biological Survey of Western Pacific, with Special Emphasis in the Palau Islands (Survey Area Includes New Guinea Region, Phillippine, Caroline and Mariannas Islands)	$ 10,181	$ -0-	$ -0-
Stanford University, George Vanderbilt Foundation Marine Biological Survey in the Tropical Western Pacific	-0-	5,000	-0-
Stephen F. Austin State College The Effect of Radiation on a Natural Population of Peromyscus Gossypinus	3,498	-0-	-0-
Syracuse University The Genetic Effect of Incorporated Isotopes: The Transmutation Effect	12,000	12,000	12,000
Tennessee, University of Vegetation Studies Related to Disposal of Radioactive Wastes	19,628	22,000	22,000
Tennessee, University of Studies on the Basic Theory of Tracer Experiments	10,096	18,000	19,000
Texas A. & M. Research Foundation A Study of Some Factors Involved in the Disposal of Radioactive Wastes at Sea	28,500	35,000	35,000
Texas A. & M. Research Foundation Radiophosphorus Incorporation into Nucleic Acids in Autolyzing Tumor Cells	-0-	15,000	15,000

Program Activities Related to the Development of Radiation Standards - Continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(2) Research Related to Radiation Standards Involving Environmental Contamination and Waste Disposal - continued			
Off-Site Projects - continued			
Tulane University Dynamics of Mammal Populations	$ 15,272	$ 15,000	$ 16,000
Utah State University Effect of Radioactive Elements and Radiation on Ewes Maintained on Different Levels of Nutrition	9,900	-0-	-0-
Utah State University Use of Radioisotopes in Studying Lime-Induced Chlorosis	8,500	9,000	9,000
Virginia, University of The Metabolic Exchange of Radioactive Isotopes in Isolated Cell Systems	8,028	9,000	9,000
Wabash College A Continuing Ecological Survey of the W. C. Allee Memorial Woods and the Bordering Sugar Creek	10,835	10,000	10,000
Wake Forest College, Bowman Gray School of Medicine Formation of Tissue Phospholipids and Toxicity of P-32 as Related to Dietary Factors	16,170	18,000	18,000
The Weizmann Institute of Science, Rehovoth, Israel Tritium Concentrations in Natural Water Sources in Eastern Mediterranean Area	-0-	25,000	25,000
West Virginia University Radionuclide Mineral Metabolism in Chicks which Differ Genetically and are Being Produced Under "Nutritional Stress" Conditions	5,775	12,000	12,000

	5,775	12,000	12,000

Program Activities Related to the Development of Radiation Standards - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(2) Research Related to Radiation Standards Involving Environmental Contamination and Waste Disposal - continued			
Off-Site Projects - continued			
Western Reserve University A Study of the Physiological Function and Histological Changes of Thyroids Irradiated with Radioactive Iodine	$ 5,427	$ 10,000	$ 10,000
Wisconsin, University of Radioisotope Exchange Studies in Lakes	7,185	8,000	8,000
Woods Hole Oceanographic Institution Studies on the Background Radiation and Flow of Deep Ocean Currents	9,527	-0-	-0-
Woods Hole Oceanographic Institution Radioelement Studies in the Oceans	-0-	35,000	40,000
Woods Hole Oceanographic Institution Biological and Radiochemical Studies of Coastal Plankton Populations	52,000	70,000	70,000
Wyoming, University of The Chemical Characteristics and Physiological Effects of Selenium	-0-	10,000	10,000
Wyoming, University of Investigations of the Interrelationship of Sulphur, Phosphorus, and Calcium in Selenium Metabolism in Plants and Animals	2,300	-0-	-0-
Fund Estimate for Planned Additional or Expanded Project Requirements	---	200,000	400,000
Sub-Total	$926,488	$1,718,000	$1,964,000

Program Activities Related to the Development of Radiation Standards - continued

Laboratory and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimate FY 1960
(2) Research Related to Radiation Standards Involving Environmental Contamination and Waste Disposal - continued			
On-Site Research Projects			
Argonne National Laboratory			
There are 19 Projects Dealing with the Long-range Effects of Internally Deposited Fission Products such as Strontium-90, Radium-226, Plutonium-239, Polonium-210, Carbon-14, etc. Particular Emphasis is Placed on Carcinogenosis and Life Shortening Directed Toward Establishing Radiation Standards.	$ 140,216	$ 203,000	$ 337,000
Statistics Obtained from Human Populations Subjected to Environmental Radiation - Either from Natural, Occupational, or Medical Sources - are Being Utilized to Establish Radiation Standards	42,525	55,000	57,000
Meteorology Studies; Micrometerological Research with Respect to the Diffusion of Gases and Particulates in the Lower Layers of the Atmosphere	154,542	170,000	170,000
Brookhaven National Laboratory			
Investigation of the Physical and Theoretical Aspects of Atmospheric Turbulence Close to the Ground, with Emphasis on the Dispersion and Deposition of Particulates and Gases	132,000	138,000	138,000

Program Activities Related to the Development of Radiation Standards - continued

Laboratory and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(2) Research Related to Radiation Standards Involving Environmental Contamination and Waste Disposal - continued			
On-Site Research Projects - continued			
University of California at Los Angeles			
Biological Cycling: A Study of the Differential Availability in Biological Cycling of Mixed Fission Products of the Several Biologically Significant Nuclides	$ -0-	$ 168,000	$ 168,000
General Electric Company - Hanford			
Accumulation and Transfer of Radioisotopes by Ecological Systems	44,000	43,000	43,000
Health Physics Studies and Earth Sciences Studies Designed to Control Potential Radiological Hazards in the Hanford Plant Area	426,107	468,000	485,000
Health and Safety Laboratory			
Health Studies: Methods and Procedures of Implementing Environmental Health Surveys and Other Research	221,510	226,000	207,000
Knolls Atomic Power Laboratory			
Health Physics Developmental Studies	86,223	73,000	-0-
Oak Ridge National Laboratory			
Ecological Research on the Long Range Effects of Radioactive Contamination of the Environment	169,142	250,000	225,000

Program Activities Related to the Development of Radiation Standards - continued

Laboratory and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(2) Research Related to Radiation Standards Involving Environmental Contamination and Waste Disposal - continued			
On-Site Research Projects - continued			
University of Tennessee			
Projects Devoted to the Study in Large Animals of Fission Product Metabolism, and the Toxicity of Gamma Rays and Fission Neutrons Arising from Test Site Operations, and in Particular with the Pathology and Physiology of Radiation Damage. A Portion of the Program is devoted to Soil-Plant relationships as it is Pertinent to the Fate of Fission Products in the Food Chain.	$ 256,336	$ 400,000	$ 523,000
University of Washington			
Field Studies of Radiation Biology at or near the Test Sites in the Marshall Islands	105,000	140,000	150,000
Enivetok Marine Biological Laboratory			
Biological Research Associated with the Testing Program at Enivetok and with Fundamental Investigations in non-testing Periods	4,033	12,000	32,000
Sub-Total	$1,781,634	$2,346,000	$2,535,000

Program Activities Related to the Development of Radiation Standards

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(3) Research Related to Prophylaxis and Treatment of Radiation Injury	$3,150,623	$3,632,000	$4,059,000
Off-Site Research Projects			
The Agricultural and Technical College of North Carolina Biochemical and Immunological Comparisons of Irradiated Mice and Rats Treated with Bone Marrow Transplants	3,450	8,000	8,000
Boston University, Boston, Massachusetts Preparation of a Substance for the Prevention of Vascular Damage Due to Radiation	-0-	9,000	9,000
Buffalo, University of - School of Medicine Department of Bacteriology and Immunology Immunological Factors in Bone Marrow Transplants	20,760	32,000	30,000
California, University of Growth-Promoting Agents in Adult Tissues	11,198	-0-	-0-
California, University of - L.A. The Metal Binding Capacity of Tissues and Tissue Fractions and Its Relationship to Antioxidant Activity	-0-	11,000	13,000
Chicago, University of The Physiological Factors Involved in Antibody Synthesis and in the Modification of the Immune Process by X-Irradiation.	35,000	38,000	38,000
The Children's Medical Center - Children's Cancer Research Foundation The Nature of Bleeding in Pancytopenia with Special Regard for Thrombocytopenia and the Vascular Defect	71,769	70,000	70,000
Emory University The Effect of Radioprotective Mercaptans on Free Radical Reactions	6,021	12,000	10,000

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Program Activities Related to the Development of Radiation Standards - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(3) Research Related to Prophylaxis and Treatment of Radiation Injury - continued			
Off-Site Research Projects - continued			
Fordham University The Study of Genetic and Environmental Factors in the Biosynthesis of Enzymes and Proteins	$ -0-	$ 38,000	$ 23,000
Health, Research, Inc., Roswell Park Memorial Institute The Role of Serum Erythropoietic Factor in the Anemia of Malignancy	10,850	11,000	-0-
University of Illinois The Effect of Phenol Ethers on Lesions of the Skin Produced by Beta Radiation (Strontium 90)	-0-	15,000	20,000
Jefferson Medical College of Philadelphia Transplantation of Preserved Marrow between Animals and From One Human to Another	20,240	22,000	22,000
Johns Hopkins University Blood Coagulation, Hemorrhagic Disease	10,044	11,000	12,000
Johns Hopkins University Investigation of the Mechanism of Bone Deposition and Related Physiological Studies	17,000	-0-	-0-
Johns Hopkins University The Mechanism of the Activation of Latent Epidemic Typhus Infections in the Laboratory Animals and in Humans by X-ray	9,000	-0-	-0-
Marquette University, Milwaukee, Wisc. Biological Effects of X-Irradiation of Exclusively the Circulating Blood	6,930	14,000	15,000

Program Activities Related to the

Circulating Blood	6,930	14,000	15,000

Program Activities Related to the Development of Radiation Standards - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(3) Research Related to Prophylaxis and Treatment of Radiation Injury - continued			
Off-Site Research Projects - continued			
Marquette University, School of Medicine Relation of Rickettsial and Viral Infections to Radiation Injury	$ 7,500	$ 16,000	$ 16,000
Mary Imogene Bassett Hospital The Collection, Storage and Use of Human Bone Marrow	40,032	30,000	30,000
Massachusetts General Hospital The Metabolism of Calcium and Strontium as Disclosed by Tracer Studies on Patients with Thryoid and Related Diseases	10,000	-0-	-0-
Massachusetts General Hospital The Collection, Storage and Use of Human Cadaver Marrow	20,000	28,000	45,000
University of Minnesota Radioiodine Labelled Alizarin	-0-	13,000	15,000
Minnesota, University of, Medical School The Effects of Gram Negative Bacterial Endotoxin on Irradiated Rabbits	13,005	13,000	13,000
Montefiore Hospital, New York The Development of Chelating Agents for Enhancing the Urinary Excretion of Strontium	20,000	20,000	15,000
Montefiore Hospital, Pittsburgh, Pennsylvania Synthesis of Nucleic Acid Purines in Bone Marrow	16,000	25,000	25,000
U. S. Naval Radiological Defense Laboratory Modification of Radiation Injury by Injected Isologous, Homologous and Heterologous Bone Marrow	9,969	-0-	-0-

Program Activities Related to the Development of Radiation Standards - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(3) Research Related to Prophylaxis and Treatment of Radiation Injury - continued			
Off-Site Research Projects - continued			
New England Center Hospital Physiopathology of Platelets and Development of Platelet Substitutes	$ 24,805	$ 26,000	$ 28,000
New England Center Hospital Bone Marrow Research Project	35,000	40,000	40,000
New York Medical College Preparation of Radioactive Heparin	12,500	15,000	15,000
New York, State University of The Effect of X-Irradiation on a "Megakarocytoses" Produced in Gelatin-Treated Animals	6,319	8,000	8,000
Oklahoma State University, Research Foundation Studies on Glutathione and 2-Aminoethanethiol	-0-	9,000	9,000
Parke, Davis and Company Factors Elaborated by Animal Tissues Which Stimulate Rate of Regeneration of Hematopoietic Organs of Animals Exposed to Total Body Irradiation with Gamma Rays.	80,000	80,000	80,000
Pennsylvania, University of Changes in the Capillary Fragility and the Colloidal Properties of Blood Following Irradiation	10,700	12,000	12,000

Program Activities Related to the Development of Radiation Standards - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(3) Research Related to Prophylaxis and Treatment of Radiation Injury - continued			
Off-Site Research Projects - continued			
Peter Bent Brigham Hospital A Program for the Study of Transplantation of Marrow, Tissues, and Whole Organs of Related Topics in Surgical Research	$ -0-	$ 50,000	$ 50,000
Presbyterian - St. Luke's Hospital Mobilization of Radioactive Emitters from Bone	500	25,000	25,000
Rice Institute Endocrine and Metabolic Studies Utilizing Radioisotopes and Labelled Hormones	18,770	19,000	20,000
Rochester, University of An Investigation into the Pharmacological Properties of Anti-Irradiation Drugs	-0-	25,000	27,000
Sloan-Kettering Institute for Cancer Research Factors Affecting Platelet Production and Destruction	2,598	7,000	7,000
Southwest Foundation for Research and Education An Investigation of the Production and the Possible Isolation of Substances Capable of Stimulating Recovery from Radiation by Utilizing Techniques of *in vitro* Maintenance of Spleen and Other Organs.	50,000	65,000	40,000
Tennessee, University of The Origin and Fate of Thiocyanate Ion in Metabolism	5,935	9,000	10,000

Program Activities Related to the Development of Radiation Standards - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(3) Research Related to Prophylaxis and Treatment of Radiation Injury - continued			
Off-Site Research Projects - continued			
Tennessee, University of Protective Action of Bone Marrow Perfusates and AET in Irradiated Monkeys	$ 45,000	$ 45,000	$ 45,000
Tufts College Enzyme Studies on White Blood Cells and Blood Platelets	6,963	7,000	7,000
Tufts College Medical School Investigation of Enzymic, Biochemical and Other Cytological Activities of Human Leukocytes	21,734	20,000	22,000
Virginia, Medical College of The Homotransplantation of Fetal Blood Cells and Tissues in Animals Receiving Fatal Whole Body Radiation	-0-	25,000	25,000
Washington, University of Erythrocyte Preservation and Metabolism	7,000	15,000	18,000
Western Biological Laboratories An Unidentified Factor in Liver which Prolongs Survival of Animals Administered Multiple Sublethal Doses of X-Irradiation	6,498	7,000	7,000
Yale University, School of Medicine Biologic Implications of Isologous and Heterologous Bone Marrow Repopulation in Irradiated Animals	15,194	8,000	13,000
Fund Estimate for Planned Additional or Expanded Project Requirements	---	-0-	100,000
Sub-Total	$ 708,284	$ 953,000	$1,037,000

Program Activities Related to the Development of Radiation Standards - continued

Laboratory and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(3) Research Related to Prophylaxis and Treatment of Radiation Injury- continued			
On-Site Research Projects			
Argonne Cancer Research Hospital			
Studies on Recovery from Radiation Injury	$ 133,164	$ 131,000	$ 140,000
Studies on Erythropoietic Hormone in Cancer and other Diseased patients	81,202	79,000	78,000
Mechanism of Metabolic Defects in Iron-deficient States in Cancer and Related Diseases	63,100	79,000	78,000
Argonne National Laboratory			
Fundamental Immunologic Studies Related to Antibody Formation	81,763	73,000	128,000
Biological Research: Includes 22 Projects Whose Aims Are the Understanding of Mechanisms and the Development of Methods Pertinent to either the Reversal of Radiation Damage or the Removal of Internally Deposited Radioactive Elements.	244,758	275,000	280,000

Program Activities Related to the Development of Radiation Standards - continued

Laboratory and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(3) Research Related to Prophylaxis and Treatment of Radiation Injury - continued			
On-Site Research Projects - continued			
Brookhaven National Laboratory			
Effect of Platelets and Blood Fractions on Recovery	$ 96,000	$ 100,000	$ 100,000
The Function of the Bone Marrow in the Recovery of Dogs from Radiation Sickness	97,000	100,000	100,000
The Use of Aloe Vera for Treatment of B-ray Burns	52,000	50,000	50,000
The Effect of Radiation on Antibody Products	88,000	90,000	100,000
University of California, Los Angeles			
Biological Research: In order to From a Basis for Developing Methods for Reversing Radiation Damage, Four Projects are Designed to Specify the Biochemical Compounds which may be Initially Altered by Radiation and Lead to Physiological Damage. Particular Attention is Directed Toward Cellular Fats and Enzymes.	168,924	167,000	155,000
Lawrence Radiation Laboratory			
Medical Research: Human Physiology, Experimental Medicine	864,397	815,000	942,000

Program Activities Related to the Development of Radiation Standards - continued

Laboratory and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(3) Research Related to Prophylaxis and Treatment of Radiation Injury - continued			
On-Site Research Projects - continued			
Oak Ridge Institute of Nuclear Studies			
Experimental Treatment of Massive Total Body Irradiation Followed with Bone Marrow Transplantation	$ 59,821	$ 66,000	$ 163,000
Oak Ridge National Laboratory			
Various Methods for Repairing or Protecting Cells from Radiation Damage are Being Pursued. Particular Emphasis is Placed on Studies of Factors Which Can Modify the Immunological Capability so as to Permit Replacement of Radiation-damaged Cells in Man.	328,209	568,000	618,000
University of Rochester			
Studies on Therapy of the Radiation Syndrome	41,961	43,000	45,000
Studies of the Endrocrine Imbalances in the Irradiated Animal	41,960	43,000	45,000
Sub-Total	$2,442,339	$2,679,000	$3,022,000

Program Activities Related to the Development of Radiation Standards - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(4) Dosimetry and Instrumentation	$2,027,703	$2,083,000	$2,267,000
Off-Site Research Contracts			
Airborne Instruments Laboratory Automatic Scanning of Nuclear Emulsions	$ 75,869	$ -0-	$ -0-
Allen B. Dumont Laboratories, Inc. Photomultiplier Tube Development	90,410	100,000	100,000
Armour Research Foundation of the Illinois Institute of Technology A Wide-Range Detector and Monitoring Instrument	1,983	15,000	15,000
Armour Research Foundation of the Illinois Institute of Technology Magnetic Recorder for Nuclear Pulse Application	-0-	29,000	30,000
U. S. Army, Corps of Engineers Program of Research and Development on Scintillation Crystals	18,703	12,000	12,000
Borden Company, Philadelphia Research Laboratory Proposal for Plastic Scintillators	25,500	50,000	50,000
Columbia University Evaluation of Aerosol Filters and Impactors Having Chemically Different Surfaces	3,650	-0-	-0-
U. S. Department of Commerce - National Bureau of Standards Biophysics and Instrumentation	288,596	291,000	278,000
Eder Engineering Company, Inc. Electrical Printing Counter	1,484	-0-	-0-

Program Activities Related to the Development of Radiation Standards - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(4) Dosimetry and Instrumentation - continued			
Off-Site Research Contracts - continued			
Levinthal Electronic Products, Inc. Study of Scintillation and Other Related Properties of NaI Crystals	$ 32,738	$ 32,000	$ 30,000
Louisville, University of Synthesis and Properties of Organic Scintillators	8,100	10,000	-0-
Massachusetts General Hospital Positron Scanning in Organs Other than Brain	10,000	10,000	10,000
The Methodist Hospital - The Texas Medical Center Further Investigation of the Preoperative External Localization of Intra-Cranial Lesions, Using Sodium Iodide Gamma-Ray Spectrometry Techniques in Conjunction with a Graphic Recording Automatic Scanning Device	12,500 12,500	25,000 25,000	25,000 25,000
Michigan, University of High Resolution Detection of Nuclear Radiations	7,750	-0-	-0-
New York University Measurement of Environmental Radiation	15,000	20,000	20,000
Notre Dame, University of Fundamental Research on Photoemission	10,000	10,000	10,000
Pilot Chemicals, Inc. Research and Development Work on Plastic Scintillators Containing High Z Materials	1,368	32,000	32,000

Program Activities Related to the Development of Radiation Standards - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(4) Dosimetry and Instrumentation - continued			
Off-Site Research Contracts - continued			
Polytechnic Institute of Brooklyn Color-Forming Dosimeters for Ionizing Radiation	$ -0-	$ 15,000	$ 15,000
Radiochemistry, Incorporated Medium Low Level Soft Beta Counting Equipment	52,500	-0-	-0-
Radio Corporation of America, RCA Laboratories Photomultiplier Tube Research and Development	65,308	100,000	100,000
Radio Corporation of America Multiplier Phototube Development	3,189	-0-	-0-
Riggs Nucleonics Company Design and Build One Production Prototype of a Combination Gamma Radiation Detector and Standard Broadcast Transistorized Radio Receiver	2,525	-0-	-0-
St. Procopius College I - Special Problems in Nuclear Instrumentation, II - Ionization of Gases	72,502	60,000	50,000
Sloan Kettering Institute for Cancer Research Equivalence of Absorbed Radiation Energy and Cavity Ionization	15,789	15,000	15,000
Taylor University Diffusion of Radon Through Semi-Permeable Materials	-0-	8,000	8,000
Vanderbilt University Establishment and Operation of Low Level Whole Body Counting Facility	30,847	22,000	25,000

Facility	30,847	22,000	25,000

Program Activities Related to the Development of Radiation Standards - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(4) Dosimetry and Instrumentation - continued			
Off-Site Research Contracts - continued			
Vanderbilt University			
Nuclear Physics Studies on Instrumentation Problems	$ 10,000	$ -0-	$ -0-
Fund Estimate for Planned Additional or Expanded Project Requirements	---	---	100,000
Sub-Total	$ 868,811	$ 881,000	$ 950,000
On-Site Research Projects			
Argonne National Laboratory			
Basic and Applied Studies of the Physical Parameters Controlling Scintillation and Ionization Chamber Dosimetry for Neutrons and Gamma Rays	92,501	100,000	119,000
Advanced Techniques in Low Level Counting of Biological Samples and of Humans for the Analysis of Fallout and Uptake of Radioactive Materials at or near Natural Background Exposures	30,707	25,000	36,000
Investigations of liquid scintillation Methods for the Measurement of Low Energy Beta Emitters	9,881	10,000	11,000

Program Activities Related to the Development of Radiation Standards - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(4) Dosimetry and Instrumentation - continued			
On-Site Research Projects - continued			
Brookhaven National Laboratory			
Search for and Development of New Detectors and Improved Techniques for Radiation Monitoring in Biological Research	$ 20,000	$ 40,000	$ 40,000
Dosimetric Studies in the Evaluation of Biologically Significant Exposures to X-Ray, Gamma, Neutron and High Energy Radiations	80,000	95,000	100,000
California, University of, at Los Angeles			
Chemical Dosimetry of X, Gamma, Fast and Slow Neutron Radiations	41,105	25,000	32,000
Solid State Dosimetry and Depth-Dose Studies of Fast Neutrons from Reactors and Atomic Detonations	21,489	24,000	30,000
Instrumentation and Development for Measurement and Analysis of Local Fallout in the Vicinity (200-300 miles) of Nevada Test Site	92,162	69,000	101,000
General Electric Company - Hanford			
Development of Radiation Measurement Techniques for the Special Problems Encountered in Radiation, Research and Protection Associated with a Plutonium Production Plant	157,000	162,000	186,000
Application of Chemical Dosimetry as Related to Chemical Changes in Humans on Irradiation	21,000	21,000	23,000

Program Activities Related to the Development of Radiation Standards - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(4) Dosimetry and Instrumentation - continued			
On-Site Research Projects - continued			
General Electric Company - Hanford - continued			
Development of Health Physics Instruments Including Personnel, Survey, and Fixed Monitors Required for Radiation Health in a Plutonium Production Plant	$ 121,634	$ 133,000	$ 129,000
Health and Safety Laboratory			
Design and Construction of Improved World-Wide Fallout Sample Counting Facility	7,481	66,000	-0-
Consultative, Laboratory and Staff Services Supporting the DBM's Instrumentation and Dosimetry Program Including Purchase of and Shipment of Equipment for Special Projects	60,915	33,000	39,000
Low Level Activity Measurements Facilities for the Analysis of Radioactive Uptake in Humans from Environmental Contamination and of Fallout Samples	30,457	50,000	56,000
Instrumentation of Research Project Investigating Decontamination of and Counter-Measures Against Fallout	24,580	13,000	16,000
Maintenance of Health and Safety Laboratory World-Wide Fallout Counting Equipment	44,350	50,000	55,000
Automatization of Counting and Data Handling Equipment for World-Wide Fallout Sample Analysis	76,411	65,000	74,000

Program Activities Related to the Development of Radiation Standards - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(4) Dosimetry and Instrumentation - continued			
On-Site Research Projects - continued			
Health and Safety Laboratory - continued			
Instrumentation for Environmental Surveys of Terrestrial and Cosmic Radiations and of Health Hazards Involved in Feed Material Processes	$ 30,992	$ 30,000	$ 39,000
Research and Development of New Detectors, Components and Circuits to Provide a Basis for Future Design Criteria of Improved Radiation Counting and Detecting Instruments	61,983	59,000	67,000
Oak Ridge National Laboratory			
Theoretical and Experimental Studies Involving Information Theory as Applied to Specific Interactions of Nuclear Radiations with Biological Materials as Related to Physical Dosimetry	64,007	62,000	90,000
Rochester, University of			
Development of Health Physics Instrumentation as Related to the Specific Hazards Associated with the University of Rochester Project	69,967	70,000	74,000
Sub-Total	$1,158,892	$1,202,000	$1,317,000

Program Activities Related to the Development of Radiation Standards - continued Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(5) Radiation Protection - Physical	$ 867,616	$1,440,000	$1,762,000
Off-Site Research Projects			
U. S. Army, Chemical Corps Research and Development Command Proposed Scope of Tests of Respiratory Protection Afforded by Readily Available Expediency	$ -0-	$ 5,000	$ -0-
Department of Commerce, Bureau of Standards Investigation of the Various Radiation Shielding Problems of Interest to the AEC	20,614	22,000	22,000
Harvard University Research and Development of Protective Respiratory Equipment	-0-	33,000	35,000
U. S. Department of Interior, U. S. Geological Survey Aerial Radiological Survey	10,000	70,000	70,000
Fund Estimate for Planned Additional or Expanded Project Requirements	---	---	800,000
Sub-Total	$ 30,614	$ 130,000	$ 927,000

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Program Activities Related to the Development of Radiation Standards - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(5) Radiation Protection - Physical - continued			
On-Site Research Projects			
Brookhaven National Laboratory			
The Development of Instruments and Procedures to Use Neutrons in Patients Including Dosimeters	$ 147,000	$ 150,000	$ 175,000
Instrumentation for Monitoring a Hospital Located Adjacent to a Reactor	94,000	100,000	125,000
Albuquerque Operations Office			
Civil Effects Test Operations at Nevada Test Site and Pacific Proving Ground Including Decontamination Studies, Attentution of Structures, Measurement of Scattering and Re-radiation, and Aerial Radiological Surveys in and Near the Test Area	317,779	570,000	-0-
University of California at Los Angeles			
Correlation of ground Readings with Aerial Readings, Documentation of Plant and Animal Uptake, Establishment of Isodose Contours at NTS	101,245	82,000	75,000
University of Rochester			
Biomedical Field Tests of Plutonium Inhalation at Nevada Test Site	16,342	-0-	-0-
Oak Ridge National Laboratory			
Prompt Gamma and Neutron Studies to Investigate the Shielding of Typical Japanese Structures for Correlation with Field Data From the Atomic Bomb Casualty Commission and Other Laboratories (Ichiban Project)	160,656	408,000	460,000
Sub-Total	$ 837,002	$1,310,000	$ 835,000

Sub-Total	$ 837,002	$1,310,000	$ 835,000

Program Activities Related to the Development of Radiation Standards

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(6) Radiation Measurement - General	$ 662,924	$ 919,000	$ 1,181,000
Off-Site Research Projects			
Department of the Interior - U.S. Geological Survey			
Aerial Radiological Surveys at AEC Installations	$ 10,000	$ 230,000	$ 240,000
Department of Commerce - Bureau of Standards			
Assistance to NCRP	6,000	9,000	10,000
Technical Operations Inc.			
A Series of Research Measurements of Shelter Factors Offered by the the AEC Headquarters Building	-0-	34,000	-0-
On-Site Research Projects			
Lawrence Radiation Laboratory			
Identification and Evaluation of Radiation Hazards Associated with Accelerators, including Studies Related to Shielding Requirements	92,772	78,000	79,000
Health and Safety Laboratory			
Periodic and Special Industrial Hygiene and Health Physics Surveys; Background Radiation Studies; Assistance in the Formulation of Codes for Regulation of Radiation Hazards; Development of Standards of Control	267,589	204,000	198,000

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Program Activities Related to the Development of Radiation Standards - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
(6) Radiation Measurement - General - continued			
On-Site Research Projects - continued			
Oak Ridge National Laboratory			
Studies Leading to Data Used in Calculating the MPC Values Contained in Handbooks of the NCRP and ICRP (Internal Dosimetry)	$ 250,994	$ 320,000	$ 610,000
U. S. Weather Bureau			
Meteorological Services to AEC	35,569	44,000	44,000

TABLE III

TABLE III

Additional Research Including Beneficial Applications of Atomic Energy Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
Off-Site Research Projects	$ 2,501,278	$ 3,095,000	$ 4,063,000
Agriculture, U. S. Department of The Intermediary Metabolism of Proteins and Amino Acids in Avian and Mammalian Species	37,761	9,000	-0-
Alabama Polytechnic Institute Radioisotope Studies on Amino Acid Imbalances and Other Factors Affecting the Metabolism of Amino Acids in Microorganisms and Animals	-0-	2,000	-0-
Albert Einstein College of Medicine The Metabolism of Adenosine Triphosphate and Related Compounds in the Erythrocyte	10,000	10,000	10,000
Allen B. DuMont Laboratories, Inc. The Construction of a Nuclear Track Viewing Device	100	45,000	45,000
American Meat Institute Foundation of the University of Chicago Relation of Vitamin B-12 to Nucleic Acid Metabolism	8,700	8,000	8,000
American Municipal Association A Study of the Impact of Peaceful Applications of Atomic Energy on the Community	-0-	15,000	15,000
Amherst College Studies on Nucleic Acid and Free Nucleotide Synthesis in Normal Tissue and in Tumor Tissue, Using C-14	10,009	12,000	13,000
Arizona, University of The Synthesis of Fatty Acids in a Higher Plant	6,289	7,000	7,000
Arkansas, University of The Utilization of Radioisotopes by Vertebrate Embryos	7,600	8,000	8,000

Additional Research Including Beneficial Applications of Atomic Energy - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
Off-Site Research Projects - continued			
Arkansas, University of The Use of Radioisotopes in Studies of Microbial Metabolism	$ 6,166	$ 7,000	$ 7,000
Arkansas, University of Studies of the Phosphorylation Cycle in the Intact Animal Using Radioactive Phosphorus	11,445	14,000	14,000
Arkansas, University of - School of Medicine To Investigate, Develop and Evaluate Radioisotopes for Teletherapy	10,000	10,000	-0-
Baltimore City Hospitals The Use of Positron Emitting Isotopes in the Localization of Intracranial Lesions	-0-	25,000	30,000
Bermuda Biological Station for Research, Inc. Metabolic Changes During Development and Growth in Marine Invertebrates	-0-	12,000	13,000
Boston University, School of Medicine Enzymology of the Formed Elements of Human Blood. Dynamics and Biosynthesis of Carbon Labelled Substrates by Human Leucocytes *in vitro*	13,000	11,000	12,000
Bowling Green University A study of Productivity of Aquatic Habitats Near Bowling Green, Ohio	9,800	11,000	11,000
Brigham Young University Studies on the Oxygen Evolving System in Photosynthesis	5,660	5,000	5,000
California Institute of Technology The Biological Synthesis of Protein	12,000	13,000	13,000
California, University of The Uptake, Distribution and Fate of Labelled Compounds Used as Tracers in Plants	7,482	10,000	10,000

Tracers in Plants	7,482	10,000	10,000

Additional Research Including Beneficial Applications of Atomic Energy - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
Off-Site Research Projects - continued			
California, University of Carbohydrate Metabolism as Studied with C-14 Labelled Compounds	$ 24,500	$ -0-	$ -0-
California, University of The Use of Radioactive Isotopes and Other Indicators to Study Absorption and Distribution of Herbicidal Chemicals in Plants	2,450	-0-	-0-
California, University of Tracer and Enzymatic Studies on the Metabolism of Plants and Bacteria	21,095	22,000	22,000
California, University of Mechanism of Biosynthesis of Polycyclic Compounds	13,000	17,000	17,000
California, University of Intermediary Metabolism of Organic Compounds and Biological Synthesis in Farm Animals	38,000	39,000	39,000
California, University of An Investigation into the Utility of Radiation Sterilized Soil as a Medium for Growth of Plants and Microorganisms.	-0-	13,000	15,000
California, University of - School of Veterinary Medicine Distribution Studies of the Reticuloendothelial System at Various Stages of Development in Relation to the Problem of the Dissociation of Liver Functions	763	-0-	-0-
Cedars of Lebanon Hospital To Investigate, Develop and Evaluate Radioisotopes for Teletherapy	10,000	10,000	-0-
Chicago, University of Development of an Image Amplifier	4,710	60,000	60,000

UNIVERSITY OF MICHIGAN LIBRARIES

Additional Research Including Beneficial Applications of Atomic Energy - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
Off-Site Research Projects - continued			
Chicago, University of Radiosensitivity of the Lymphocytes	$ 9,000	$ 10,000	$ 10,000
Chicago, University of Biosynthesis of Radioactive Drug Compounds	16,250	21,000	22,000
Chicago, University of Investigation of the Mechanism of Antibody Synthesis by the Tracer Technique	9,250	10,000	10,000
Chicago, University of The Entropy of Amino Acids and Proteins	7,042	15,000	15,000
Children's Hospital Society of Los Angeles Iron Storage and Reutilization	10,000	10,000	10,000
Cincinnati, University of Preparation of a Handbood of Medical Procedures for Radiation Accidents and Hazards	2,847	6,000	-0-
City of Hope, Medical Center A Study of Leukopoietic Factors in Blood	-0-	18,000	18,000
Clemson Agricultural College Overwintering of Xanthomonas pruni, the Causal Organism of Bacterial Spot of Peaches	-0-	3,000	3,000
Colorado Agricultural and Mechanical College The Potential Productivity and Energy Budgets of High Mountain Lakes in North-Central Colorado	11,661	15,000	18,000

Additional Research Including Beneficial Applications of Atomic Energy - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
Off-Site Research Projects - continued			
Colorado, University of Immunophysiologic Studies with Electrolytes, Including Mg-28, in Patients	$ 14,974	$ 16,000	$ 17,000
Colorado, University of - Medical Center A Study of the Combined Action of Certain Chemical Inhibitors of Metabolism with X-Radiation and Other Ionizing Radiations on the Growth of Certain Transplantable Malignant Tumors	12,997	15,000	15,000
Columbia University Development of Instruments and Procedures for the More Accurate Evaluation of the Cumulative Occupational Dose in Radiology	-0-	10,000	10,000
Columbia University Use of Radioactive Silver III on Indwelling Balloon Catheters for the Treatment of Gastric Hyperacidity and Bladder Papillomatosis	7,130	10,000	10,000
Columbia University The Activation of Hydrogen by Biological Catalysts	16,140	17,000	17,000
Columbia University Study of the Condensing Units in Nucleic Acid Biosynthesis	-0-	18,000	18,000
Columbia University To Identify Precursors and End Products Containing Radiocarbon, in Studies of the Role of Glucose, Ascorbic Acid, etc., in Metabolism	5,300	15,000	16,000
Columbia University Pathways of Alkaloid Biosynthesis	10,097	6,000	8,000
Columbia University Nucleic Acid and Protein Synthesis in Individual Cells and Chromosomes Studied by Radioactive Tracers and Autoradiographs	7,663	16,000	16,000

Additional Research Including Beneficial Applications of Atomic Energy - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
Off-Site Research Projects - continued			
Columbia University - College of Physicians and Surgeons The Turnover of Specific Proteins, Protein Fractions, and Nucleic Acids in Normal and Malignant Human Testis and Kidney	$ 9,000	$ -0-	$ -0-
Connecticut, University of A Study of Alkaloidal Synthesis in Claviceps Purpurea (Ergot)	2,270	5,000	5,000
Cornell University The Biosynthesis of Purines in the Higher Plant, with Reference to Caffeine	3,518	6,000	7,000
Duke University Metabolic Studies with Tracer Techniques	17,456	16,000	16,000
Duke University - School of Medicine The Biology of Brain Tumors	4,527	-0-	-0-
Emory University To Investigate and Evaluate Radioisotopes for Teletherapy	9,957	10,000	-0-
Farnsworth Electronics Co. (Div. of International Tel. & Tel. Corp.) Proposal for a Special Image Intensifier	-0-	20,000	20,000
Florida, University of - Agricultural Experiment Station A Study of Techniques for the Use of Gamma Radiation for Plant Improvement and Testing the Nutritive Value and Storage Characteristics of Irradiated Agricultural Products	13,000	-0-	-0-
Florida, University of A Study of the Biochemical Results of Certain Treatments Given Patients Suffering from Phenylpyruvic Acid Oligophrenia	12,662	15,000	15,000

Additional Research Including Beneficial Applications of Atomic Energy - Continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
Off-Site Research Projects - continued			
Florida, University of - Agricultural Experiment Station Metabolism and Transport of Carbohydrates in Plants	$ 7,850	$ 9,000	$ 9,000
Fordham University Hydrolysis and Enzymatic Degradation of Proteins	16,000	12,000	12,000
Georgetown University A study of Intermediary Carbohydrate Metabolism by Means of Labelled Compounds	8,500	9,000	9,000
Georgetown University - School of Medicine Source of the Liver Glycogen Resulting from the Administration of Cortisone	1,620	3,000	4,000
George Washington University The Effect of Anesthetic Drugs; (1) on Changes in Blood Volume and Cardiac Output; (2) on Cerebral Blood Flow and Metabolic Rate, Utilizing Tracer Material	-0-	20,000	20,000
George Washington University Continuous Blood Volume Recording with Tracers in Patients under Anesthesia and During Surgery, with Special Regard to Speci ic Physiological Conditions	15,500	20,000	20,000
Georgia, Medical College of Investigation of the Results of Treating Crippling Emphysema with I-131	1,429	-0-	-0-
Georgia, Medical College of The Effects of Nutritional Deficiencies on the Synthesis of Phospholipids and Nucleoproteins in the Rat	2,223	-0-	-0-

Additional Research Including Beneficial Applications of Atomic Energy - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
Off-Site Research Projects - continued			
Georgia, Medical College of A Study of Factors Influencing the in vitro Synthesis of Phospholipid and Ribonucleic Acid in Rat Liver Particulate Matter	$ 2,358	$ 5,000	$ 5,000
Georgia, University of The Role of Mineral Elements in Poultry Nutrition	3,750	8,000	9,000
Harvard University The Permeability of Membranes with Special Reference to Absorption by the Intestine; Metabolic Studies on Leukocytes with Particular Reference to Phagocytosis.	73,319	-0-	-0-
Harvard University Factors Affecting Metabolic Pathways	12,880	19,000	19,000
Harvard University - Medical School Alkaline Earth Metabolism in Human Patients	11,900	22,000	25,000
Hawaii, University of The Utilization and Evaluation of Isotope Techniques for the Determination of Algal Productivity in the Tropical Pacific; The Role of Benthic Algae in the Central Pacific	11,327	12,000	12,000
Health Research, Inc. Veterans Admin. Hospital, Albany, N.Y. Intracellular Distribution and Enzymatic Function of Cobalt	6,163	8,000	8,000
Howard University Kinetic and Mass Spectrometric Studies of Biophysical Systems with Radioactive and Stable Isotopes	16,200	17,000	17,000
Howard University The Relation of Ascorbic Acid to Cholesterol	8,700	9,000	10,000

	8,700	9,000	10,000

Additional Research Including Beneficial Applications of Atomic Energy - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
Off-Site Research Projects - Continued			
Howard University	$	$	
A Study of the Chronic Hemodynamic Alterations Induced in Dogs with Various Cardiac Lesions Following Production of Experimental Hypertension	$ 9,000	$ -0-	$ -0-
Inter-American Institute of Agricultural Sciences			
Atomic Energy Applied to Agriculture in Latin America; a. Use of Isotopes; b. Gamma Field	100,000	100,000	110,000
Illinois, University of			
Experimental Research on Synthesis of Boron-Containing Dyes	7,500	15,000	20,000
Illinois, University of			
Function and Metabolism Studies with Carbon-14 Labelled Biotin	-0-	11,000	12,000
Illinois, University of			
Intermediary Metabolism of Carbohydrates	10,075	10,000	11,000
Illinois, University of			
Metabolism of Amino Acids Labelled with Radioactive Carbon	5,325	5,000	6,000
Illinois, University of			
Utilization of Carbon-14 in Studies of the Metabolism of Lactose	10,000	8,000	8,000
Illinois, University of			
Nutritional Biochemistry on the Metabolism of Vitamins and Amino Acids	8,375	9,000	9,000
Illinois, University of			
Tracer Studies of Carbohydrate Interconversions by Microbial Enzymes	13,500	14,000	15,000

Additional Research Including Beneficial Applications of Atomic Energy - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
Off-Site Research Projects - continued			
Illinois, University of - College of Medicine Study on the Effects of Intragastric Irradiation with Beta Rays from Ruthenium 106 - Rhodium 106 in Patients with Malignant Disease	$ 3,057	$ 7,000	$ 8,000
Indiana University Foundation Energy Transport in Bacterial Cell-Free Extracts	7,750	9,000	9,000
Indiana University Foundation Biosynthesis and Specificity of Normal and Immune Proteins	6,900	8,000	8,000
Institute for Cancer Research - Lankenau Hospital Origin and Fate of Amino Acids in Plants and Animals	6,000	-0-	-0-
Institute for Cancer Research - Lankenau Hospital Metabolic Studies on Ethionine and Derivatives	10,000	10,000	10,000
Institute of Living, Research Laboratory Metabolic Studies on Nervous Tissue	13,300	15,000	15,000
Iowa State College Comparative Toxicity of Rare Earth Compounds; Chronic Toxicity Studies of Rare Earths	27,986	55,000	60,000
Iowa State College Plant Biochemistry of Boron	-0-	5,000	5,000
Iowa State College Synthesis of Organic Compounds	6,850	-0-	-0-
Iowa State College A Study of the C-14 Method of Evaluating Primary Production in Iowa Lakes	10,235	12,000	13,000

Iowa Lakes	10,235	12,000	13,000

Additional Research Including Beneficial Applications of Atomic Energy - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
Off-Site Research Projects - continued			
Jefferson Medical College of Philadelphia Metabolic and Cytologic Changes Induced by Metallic Carbonyls	$ 19,880	$ 22,000	$ 22,000
Johns Hopkins University Purification of Intrinsic Factor in Gastric Juice	6,642	7,000	7,000
Johns Hopkins University Factors Influencing the Metabolism of Copper and Iron	9,000	10,000	10,000
Johns Hopkins University Pathways of Metabolism in Embryonic Development	-0-	30,000	32,000
Johns Hopkins University Studies on Forebrain After Its Irradiation by a Monoenergetic Beam of High Energy Deuterons	23,021	18,000	18,000
Johns Hopkins University The Influence of SH Groups and Their Inhibitors on Radiocobalt Distribution in Organs and Intracellular Organnelles of the Mouse	3,770	-0-	-0-
Johns Hopkins University - School of Medicine A study of Relative Diffusion Rates of Isotopes from Capillaries	10,000	11,000	11,000
Kansas State College Use of Radioactive Tracers in Investigations of the Mode of Action of Insecticides with Emphasis on Potential Systematic or Chemotherapeutic Action	8,300	9,000	9,000
Kansas State College Mode of Action of Anthelmintics	8,575	5,000	7,000
Kansas, University of Investigation of Organic Substances Tagged with I-131 by Human Thyroid Gland in vivo	9,092	10,000	10,000

Additional Research Including Beneficial Applications of Atomic Energy - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
Off-Site Research Projects - continued			
Long Island Biological Association			
Symposium on Quantitative Biology "The Ecology of Animal Population and Demography"	$ 7,500	$ -0-	$ -0-
Louisiana State University			
Investigations of the Toxin Theory of Plant Disease Using Labelled Plant Pathogens	2,376	-0-	-0-
Lovelace Foundation for Medical Education and Research			
Indirect Blast Injuries	144,221	145,000	160,000
Marquette University - School of Medicine			
Nucleolytic Enzymes	17,082	15,000	15,000
Maryland, University of			
Studies on the physiology and the Nutrition of Lactating Ruminants	6,000	13,000	13,000
Massachusetts Eye and Ear Infirmary			
Study of the Healing of Corneal Wounds with Radioactive Isotopes with Special Reference to the Plastic Artificial Cornea	17,746	25,000	30,000
Massachusetts Eye and Ear Infirmary			
A Study of the Metabolism of the Ocular Lens with the use of Radioactive Compounds	10,000	15,000	16,000
Massachusetts General Hospital			
External Localization of Brain Tumors Employing Positron-Emitting Isotopes	28,080	38,000	38,000
Massachusetts General Hospital			
Establishment of a Beryllium Case Registry	10,000	10,000	6,000
Massachusetts General Hospital			
The Use of Thermal and Epithermal Neutrons in the Treatment of Neoplasms	22,500	45,000	45,000

The Use of Thermal and Epithermal Neutrons in the Treatment of Neoplasms	22,500	45,000	45,000

Additional Research Including Beneficial Applications of Atomic Energy - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
Off-Site Research Projects - continued			
Massachusetts Memorial Hospitals Physiological and Therapeutic Investigations and Fundamental Blood Studies Using Radioactive Isotopes	$ 40,000	$ 20,000	$ -0-
Meharry Medical College Effect of Beta Rays and X-Irradiation on Growth and Spread of Free Tumor Cells	13,560	15,000	15,000
Meharry Medical College Use of Radioactive Gold in Treatment of Tumors	34,920	30,000	-0-
Methodist Hospital - Texas Medical Center Investigation of the Presence of Unidentified Substances Produced in Benign and Malignant Nodules of the Human Thyroid Gland and Their Presence in the Circulating Blood	12,851	16,000	18,000
Michigan, University of Immunological Study of Tumors	25,000	28,000	28,000
Michigan, University of Clinical Evaluation of Teletherapy	46,448	55,000	55,000
Michigan State University Study of Productivity in a Stream Ecosystem	-0-	10,000	11,000
Michigan State University The Role of Various Aliphatic Acids in Pyrimidine Biosynthesis	5,140	6,000	6,000
Michigan State University Tracer Studies of Carbohydrate Interconversions by Microbial Enzymes	-0-	12,000	14,000

Additional Research Including Beneficial Applications of Atomic Energy - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
Off-Site Research Projects - continued			
Minnesota, University of Study of Mechanisms of Radioactive Isotopic Localization in Tissues of the Central Nervous System	$ 9,000	$ -0-	$ -0-
Minnesota, University of Localization of Porphyrins in Tumors and Other Tissues	-0-	15,000	15,000
Minnesota, University of Studies on the Relationships of Porphyrins, Tumors, and X-Rays	25,000	-0-	-0-
Minnesota, University of Studies in Lipid Metabolism by Means of Radioactive Tracers	10,201	10,000	$ 11,000
Minnesota, University of Study of Enzymic Phosphorylation Reactions with O-18 and P-32	10,875	12,000	12,000
Minnesota, University of Fluorescent and Flash-Photolytic Studies of the Role of Rhodopsin in Scoptic Vision	-0-	13,000	15,000
Minnesota, University of Effects of Radioactive Substances on Plant Pathogens and other Microorganisms	22,641	10,000	10,000
Minnesota, University of The Genetic Basis and Practical Significance of Mutations Induced in Oats and Barley with Ionizing Radiations	13,743	15,000	16,000
Missouri, University of Determination of Thyroid Activity in Farm Animals by the Use of Radioactive Tracers	5,175	-0-	-0-

Additional Research Including Beneficial Applications of Atomic Energy - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
Off-Site Research Projects - continued			
Missouri, University of			
A Study of the Inheritance of Productive Processes in Domestic Animals by Endocrine Methods Using Radioactive Isotopes as Tracers	$ 17,750	$ 18,000	$ 19,000
Montefiore Hospital for Chronic Diseases			
A Study of the Distribution and Excretion of Lanthanum and the Rare Earth Elements	25,436	30,000	30,000
Montevideo, University of			
A Study of the Origin of Precursors of Desoxyribonucleic Acid Purines and Pyrimidines in Bone Marrow	-0-	10,000	10,000
Morehead State College			
Germination and Seedling Growth of Irradiated Seeds of Several Dominant Species of the United States Deciduous Forest Region	3,359	4,000	5,000
National Academy of Sciences			
Developmental Biology Conference Series: (1) Conference on Dynamics of Proliferating Tissue (with emphasis on Hematopoiesis) to be held at BNL from September 4 through 7, 1956; (2) Conference on Morphogenesis and Mitogenesis, to be held at ANL, September 24 through 26, 1956.	2,820	-0-	-0-
National Academy of Sciences			
Program Relating to Procurement of Animals Used in Various Phases of Biological, Medical, and also Cultural Research and Testing	2,176	5,000	5,000
U. S. Naval Radiological Defense			
Project 32.3 - Radiological Countermeasures System Operations Training and Evaluation. Project 32.4 - Fallout Distribution and Radiation Contours - Low Yield Detonation	66,131	5,000	-0-

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Additional Research Including Beneficial Applications of Atomic Energy - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
Off-Site Research Projects - continued			
U. S. Navy, Office of Naval Research Bio-Sciences Information Exchange (Under Jurisdiction of Smithsonian Institution)	$ 17,489	$ 35,000	$ 35,000
U. S. Navy Toxicological Information Center	-0-	10,000	10,000
Nebraska, University of Evaluation of Effects of Radiations on Quantitative Characters in Corn, Soybeans, and Other Crops as Related to Breeding Improved Varieties: I. Corn Research II. Soybean Research	8,000	9,000	9,000
New England Center Hospital Development of Small in vivo Detectors	17,825	15,000	15,000
New York State Psychiatric Institution Thyroxine Binding Protein and Hormone Utilization	-0-	15,000	15,000
North Carolina State College of Agriculture and Engineering A Study of the Effect of the Diet on Lipid Metabolism Using C-14	9,500	5,000	5,000
North Carolina State College of Agriculture and Engineering Radiation Genetics and Radiation Resistance in Peanuts	18,000	18,000	18,000
North Carolina, University of A Study of Ion Transport Across Smooth Muscle Cell Membrane	3,888	-0-	-0-
North Dakota, University of A Study of Methionine as a Source of Methyl and Sulfur in Intermediary Metabolism	7,559	8,000	9,000
Northwestern University Medical School Synthesis and Metabolic Studies of Rings A and/or B Labelled Estradiol and Related Compounds	13,500	16,000	16,000

Synthesis and Metabolic Studies of Rings A and/or B Labelled Estradiol and Related Compounds	13,500	16,000	16,000

Additional Research Including Beneficial Applications of Atomic Energy - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
Off-Site Research Projects - Continued			
Northwestern University Medical School			
A Study of Nucleoprotein Metabolism in Normal, Degenerating, Regenerating and Stimulated Nerve Cells with Radioisotopic Tracers	$ 6,170	$ 9,000	$ 9,000
Northwestern University, Medical School			
Studies on the Direct Oxidation of Galactose by Galactose Oxidase	7,500	10,000	11,000
Oberlin College			
Studies on the physiology of Ion Accumulation and Electrolyte Balance in Living Cells	1,397	-0-	-0-
Oklahoma Research Institute, University of			
A Study of the Effects of Auxins on the Metabolism of Plants	8,402	8,000	9,000
Oklahoma State University - Agricultural Experiment Station			
The Use of Radioisotopic Methods to Study the Biosynthesis of Indole-Like Compounds	-0-	6,000	6,000
Oklahoma State University - Agricultural Experiment Station			
Isotope Investigation of the Mechanism of Nitrate Reduction in Bacteria	2,250	-0-	-0-
Oklahoma Research Institute, University of			
Studies on the Role of Certain Polyphenolic Compounds in Plant Metabolism	7,800	8,000	9,000
Oregon State College, [illegible]			
Carbohydrate-Amino Acid Interrelationships, Using Isotopic Tracers	13,600	14,000	14,000
Oregon State College			
The Study of Normal and Altered Ruminant Liver Metabolism Utilizing Radioisotopic Techniques	-0-	7,000	9,000

Additional Research Including Beneficial Applications of Atomic Energy - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
Off-Site Research Projects - continued			
Oregon State College The Mode of Action of Labelled 2, 4-Dichlorophenoxyacetic Acid and Similar Agents	$ 12,000	$ 12,000	$ 12,000
Oregon State College Intermediary Metabolism of Organic Acids and Proteins in Certain Fruits Using Isotopic Tracers	6,989	7,000	7,000
Oregon, University of - Medical School	5,000	15,000	15,000
Intracellular Pickup of Radioactive Phosphorus by Prostatic Epithelial Cells	5,000	15,000	15,000
Oregon, University of - Medical School Studies of Hemic Effects of Radioisotopes, X-Rays and of Adrenocortical Hormones	28,728	30,000	30,000
Oregon, University of - Medical School Studies on Lipogenesis	15,000	17,000	17,000
Oregon, University of An Investigation of the Use of Na-24 in Cerebral Edema, Brain Tumors and Focal Epileptic Lesions	5,000	-0-	-0-
Palo Alto Medical Research Foundation Radioactive Colloids for Medical Research	3,711	15,000	-0-
Pennsylvania State University Pathways of Metabolism in Bovine Germ Cells	3,800	4,000	4,000
Pennsylvania State University Metabolism of Molecular Oxygen by Plants	-0-	5,000	7,000
Pennsylvania State University Investigations of Pathways in Plant Metabolism	14,700	15,000	15,000

Pennsylvania State University			
Investigations of Pathways in Plant Metabolism	14,700	15,000	15,000

Additional Research Including Beneficial Applications of Atomic Energy - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
Off-Site Research Projects - continued			
Pennsylvania, University of			
Kinetics and Mechanisms of Ion Transfer in the Heart and Studies of Altered Cardiovascular Physiology in Cariovascular Disease States	$ 23,250	$ 16,000	$ 17,000
Pennsylvania, University of			
The Metabolism of Carbon 14 Labelled Compounds by Micrococcus Pyrogenes, var. aureus.	7,675	10,000	10,000
Peter Bent Brigham Hospital			
Injury, Wounding and Convalescence; A Study by Isotopic and Metabolic Methods	39,930	-0-	-0-
Pittsburgh, University of			
Attempted Modification of Mammalian Tumor Radiosensitivity with Agents and Procedures Altering Host Sensitivity	11,000	-0-	-0-
Puerto Rico, University of			
Studies on Protein and Fat Metabolism in the Normal and in Those Patients with Malnutrition and Malabsorptive Syndromes	11,416	25,000	25,000
Purdue Research Foundation			
The Physiology of Hydrogen Bacteria	6,400	7,000	7,000
Purdue Research Foundation			
Use of Radioactive Isotopes in Studying Mold Metabolism with Emphasis on the Assimilatory Mechanisms of Penicillium Chrysogenum and Other Representative Molds	6,864	6,000	6,900
Purdue Research Foundation			
Active Transport of Ions in Nitella	-0-	10,000	10,000

Additional Research Including Beneficial Applications of Atomic Energy - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
Off-Site Research Projects - continued			
Purdue Research Foundation Genetic Effects of Thermal Neutron Irradiation in Homozygous Tomatoes	$ 4,588	$ 5,000	$ 5,000
Purdue Research Foundation Carbohydrate Catabolism in Plants	-0-	7,000	8,000
Radio Corporation of America High-Gain Cascaded Image Tubes	40,534	41,000	42,000
Rheumatic Fever Research Institute Metabolism of Biologically Active Amines	10,000	-0-	-0-
Rochester, University of Metabolism of Bone Sodium	15,000	17,000	17,000
Roscoe B. Jackson Memorial Laboratory Investigation of the Epizootic and Potentially Epizootic Diseases of Mice	20,000	-0-	-0-
Rutgers, The State University Irradiation as an Aid in Fruit Variety Improvements	3,500	-0-	-0-
Stanford University Biological and Medical Investigations with the 70 Mev Linear Electron Accelerator	46,544	60,000	60,000
Southern California, University of The Action of Ultraviolet Light on Purine and Protein Metabolism in Echinoderm Embryos	1,650	-0-	-0-
Tennessee, University of The Metabolism of Serine in the Intact Rat	6,000	6,000	7,000

Additional Research Including Beneficial Applications of Atomic Energy - continued

The Metabolism of Serine in the Intact Rat	6,000	6,000	7,000

Additional Research Including Beneficial Applications of Atomic Energy - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
Off-Site Research Projects - continued			
Texas Agricultural Experiment Station Ecological, Physiological and Toxicological Investigations with the Boll Weevil	$ -0-	$ 9,000	$ 11,000
Texas Agricultural Experiment Station The Biosynthesis of Polyunsaturated Fatty Acids by the Laying Hen	-0-	7,000	9,000
Texas Agricultural Experiment Station Radiochemical Studies with Ethylene in Plant Metabolism	-0-	18,000	18,000
Texas A and M College Biosynthesis, Metabolism and Mechanism of Action of Plant Growth Substances	10,000	-0-	-0-
Texas, University of Studies of the Metabolic Processes in Molds and Fungi with Carbon-14	10,779	11,000	12,000
Texas, University of - Southwestern Medical School The Metabolism of Doubly Labelled Serum Albumin	8,833	10,000	10,000
Tulane University of Louisiana Physiology of Serum Lipids	-0-	1,000	-0-
Utah, University of Pancreas Metabolism as Studied with Radioactive Tracers	7,916	10,000	11,000
Utah State University The Isolation of Plant Viruses by Chromatographic Techniques	7,275	8,000	8,000
Vanderbilt University Cytochemical Localization of the Slow Exchanging or Non-Exchangeable Brain and Kidney Potassium	6,825	7,000	7,000

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Additional Research Including Beneficial Applications of Atomic Energy - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
Off-Site Research Projects - continued			
Vermont, University of			
Relationship of the Pyruvate Oxidation System to Growth Stimulation by Antibiotics and Other Compounds	$ 4,349	$ 9,000	$ 9,000
Virginia Polytechnic Institute			
Kinetic Studies of the in vitro Metabolism of Uniformly C-14 Labelled Glucose by Rumen Microorganisms	6,536	7,000	7,000
Virginia, University of			
A Study of the Pituitary Glands of Thyroidectomized Newts	6,213	7,000	7,000
Washington Hospital Center			
Investigation of Distribution, Localization and Excretion of Tagged Triethylene Thiophosphoramide Following Injection by Various Routes	20,000	15,000	15,000
Westinghouse Electric Corporation			
Proposal for Research on Development of a High Gain Image Intensifier for Use with Scintillation Track Chambers	15,491	20,000	20,000
Washington, State College of			
The Problem of Simultaneous Two Directional Movement in the Phloem	6,385	-0-	-0-
Washington University			
Synthesis of Nucleotides and Related Compounds	15,450	16,000	16,000
Washington, University of			
Basic Training Program for Medical Students in the Use of Radioactive Isotopes in Biology and Medicine	13,902	8,000	8,000
Washington, University of			
Hemodynamics; Blood Dynamics as Measured by Simultaneous Multiple Port Scintillation Detection of I-131 Human Serum Albumin	12,000	-0-	-0-

Additional Research Including Beneficial Applications of Atomic Energy - continued

Additional Research Including Beneficial Applications of Atomic Energy - continued

Institution and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
Off-Site Research Projects - continued			
Washington, University of Erythropoiesis and Iron Metabolism	$ 18,910	$ -0-	$ -0-
Washington, University of Productivity Studies in Puget Sound	-0-	13,000	13,000
Washington, University of Studies of Isotopically Labelled Hormones	3,500	10,000	11,000
Wayne State University - College of Medicine Evaluation of Radioactive Isotope Gamma Ray Source for Medical Teletherapy	10,000	10,000	-0-
Wesleyan University Respiratory Pathways in Fungi and Actinomycetes	7,765	9,000	9,000
Western Reserve University Synthesis of Nucleic Acids by Escherichia Coli and Bacteriophage Systems	10,000	12,000	12,000
Western Reserve University Intermediary Metabolism of Carbohydrates by Bacteria	30,000	32,000	35,000
Western Reserve University A Study of Intermediary Metabolism with Isotopically Labelled Compounds in Perfused Organs, Whole Animals and Humans	28,950	30,000	30,000
West Virginia University Degradation Studies on C-14 Labelled Beta Carotene and in vivo Produced Vitamin A	2,750	5,000	6,000
William H. Johnston Associates Medium Low Level Soft Beta Counting Equipment	9,196	70,000	70,000

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Additional Research Including Beneficial Applications of Atomic Energy - continued

Laboratory and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
On-Site Research Projects	$ 5,972,968	$ 6,601,000	$ 6,471,000
Argonne Cancer Research Hospital			
Controlling Erythropoiesis in Cancer and Other Patients	$ 82,847	$ 73,000	$ 80,000
Metabolic Studies in Human Beings	247,657	199,100	205,600
Synthesis and Physiologic Study of Compounds Labelled with Radioisotopes	46,822	48,000	45,000
Surgical Studies Involving Local Topical, and Interstitial Application of Radioisotopes	131,392	120,000	127,000
Therapeutic Application of High Energy Sources	148,354	138,000	135,000
High Energy Sources - Development and Operation	441,131	363,000	361,000
The Synthesis and Metabolism of Ribonucleic Acid and Selected Proteins in Mammalian Tissue	-0-	61,000	61,000
Advanced Clinical Research Using Radioactive Materials	25,541	42,000	39,000
Steroid Studies in Man	62,785	72,000	79,000

Additional Research Including Beneficial Applications of Atomic Energy - continued

Laboratory and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
On-Site Research Projects - continued			
Argonne National Laboratory			
Eleven projects are devoted to studying the responses of plants to ionizing radiation from internal and external sources with particular attention being given to the role of growth hormones in radiation damage and to toxicity of incorporated isotopes.	$ 129,272	$ 86,000	$ 86,000
Studies are in progress on the general metabolism of amino acids proteins and other compounds; investigations in bone pertain to the anatomy and dynamics of resorption and phosphate chemistry. Studies are also devoted to the biosynthesis of radioactive compounds and growth hormone metabolism.	410,204	371,000	200,000
Brookhaven National Laboratory			
Evaluation of the damage done to biological structures by neutrons and other high energy particles	121,000	140,000	150,000
Studies using T^3 and thymidine to study the life cycle of tissue cells including tissue culture	192,000	241,000	290,000
Selective kinetic pathway including the metabolism of carbohydrates, fats and proteins in health and disease by various techniques	427,000	445,000	388,000
The movements of ions and molecules through membranes and tissues. Use of an analogue computer to study these processes	96,000	110,000	90,000

Additional Research Including Beneficial Applications of Atomic Energy - continued

Laboratory and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
On-Site Research Projects - continued			
Brookhaven National Laboratory - continued			
Localization of radioactive elements and metals having relation to disease and metabolism	$ 94,000	$ 105,000	$ 172,000
The use of radiation to develop tumors in experimental animals and the use of these tumors for experimental purposes	63,000	75,000	75,000
Thirteen projects are under way to study the structure of chromosomes as it affects genetic duplication, the basic biochemistry of plants, the basic physiology and metabolism of animals and the structure and function of enzymes. Information from these projects which employ isotopes is used to improve the efficiency of radiation-production of mutations valuable in plant breeding	319,000	625,000	625,000
University of California at Los Angeles			
Physiological Studies on the Effects of Blasts on Mammals	36,982	18,000	22,000
University of California Medical School			
Studies on the improvements of radiation therapy of cancer. These investigations include the use of x-ray and electron beams from the 70-Mev synchrotron. The program relates directly to increasing understanding of the biological effects of radiation treatments, particularly with its application to human cancer therapy	234,596	265,000	265,000
Idaho Operations Office			
Includes irradiation services in the MTR-ERT as an adjunct to research investigation in cancer and other medical problems.	95,441	55,000	60,000

Additional Research Including Beneficial Applications of Atomic Energy - continued

Additional Research Including Beneficial Applications of Atomic Energy - continued

Laboratory and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
On-Site Research Projects - continued			
Health and Safety Laboratory			
Investigation of principles and techniques for the measurement and and interpretation of ionizing radiation from all sources in support of environmental health surveys of accelerator facilities	$ -0-	$ 62,000	$ 10,000
Lawrence Radiation Laboratory			
Instrumentation of diagnostic tracer techniques and of high energy heavy particle therapy	70,663	60,000	66,000
Dosimetry for identification and evaluation of radiation hazards associated with accelerators	-0-	31,000	20,000
Isotopes, both stable and radioactive, and modern biophysical methods are used to study the mechanisms of cell division and reproduction, the mechanisms whereby green plants utilize the sun's energy as well as the steps involved in organic reactions of biological importance	469,641	594,000	507,000
Studies in connection with irradiation of the pituitary as a control of tumor growth and related studies of tumors in man	242,350	301,000	300,000
Oak Ridge Institute of Nuclear Studies			
Research related to internal use of isotopes for treatment of cancer. External application of isotopes for clinical teletherapy and brachytherapy; Biological, chemical and physical dosimetry studies. Distribution and metabolism of isotopes to be used for clinical treatment. Autoradiographic techniques for the evaluation of tissue distribution of internally deposited isotopes	875,135	963,000	945,000
Oak Ridge National Laboratory			
Laboratory and field studies on shielding, radiation measurements, development of accurate dosimetry under varying shield conditions in conjunction with Operation Hardtack	71,564	-0-	-0-

Additional Research Including Beneficial Applications of Atomic Energy - continued

Laboratory and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
On-Site Research Projects - continued			
Health and Safety Laboratory			
Investigation of principles and techniques for the measurement and and interpretation of ionizing radiation from all sources in support of environmental health surveys of accelerator facilities	$ -0-	$ 62,000	$ 10,000
Lawrence Radiation Laboratory			
Instrumentation of diagnostic tracer techniques and of high energy heavy particle therapy	70,663	60,000	66,000
Dosimetry for identification and evaluation of radiation hazards associated with accelerators	-0-	31,000	20,000
Isotopes, both stable and radioactive, and modern biophysical methods are used to study the mechanisms of cell division and reproduction, the mechanisms whereby green plants utilize the sun's energy as well as the steps involved in organic reactions of biological importance	469,641	594,000	507,000
Studies in connection with irradiation of the pituitary as a control of tumor growth and related studies of tumors in man	242,350	301,000	300,000
Oak Ridge Institute of Nuclear Studies			
Research related to internal use of isotopes for treatment of cancer. External application of isotopes for clinical teletherapy and brachytherapy; Biological, chemical and physical dosimetry studies. Distribution and metabolism of isotopes to be used for clinical treatment. Autoradiographic techniques for the evaluation of tissue distribution of internally deposited isotopes	875,135	963,000	945,000
Oak Ridge National Laboratory			
Laboratory and field studies on shielding, radiation measurements, development of accurate dosimetry under varying shield conditions in conjunction with Operation Hardtack	71,564	-0-	-0-

Additional Research Including Beneficial Applications of Atomic Energy - continued

in conjunction with Operation Hardtack	71,564	-0-	-0-

Additional Research Including Beneficial Applications of Atomic Energy - continued

Laboratory and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
On-Site Research Projects - continued			
Oak Ridge National Laboratory			
Medical Instrumentation for in vivo localization of diagnostic isotopic tracers	$ -0-	$ 60,000	$ 60,000
Oak Ridge Area			
Distribution of radioisotopes at reduced price for research in medicine, biology and agriculture	233,819	275,000	375,000
Rochester, University of			
Indium and Thallium Distribution and Excretion, Toxicity	43,840	47,000	50,000
Mercury Effects, Distribution and Excretion	43,840	50,000	50,000
Cancer Research - Localization of Tumor Antibodies	79,366	70,000	100,000
Plasma and Tissue Protein Metabolism in Normal and Experimental disease States	35,404	37,000	41,000
The Fine-Scale Morphology of Cells and Tissues as Revealed by the Electron Microscope	22,101	22,000	25,000
Development of Spectrochemical Analytical Methods	13,661	12,000	16,000
Aerosol Problems	44,713	44,000	47,000
Toxicological Studies of Elements and Compounds of Interest to AEC	88,426	89,000	95,000
Investigations of the Functions and Properties of the Cell Surface	88,426	89,000	95,000

Additional Research Including Beneficial Applications of Atomic Energy - continued

Laboratory and Descriptive Statement	Actual FY 1958	Estimated FY 1959	Estimated FY 1960
On-Site Research Projects - continued			
Rochester, University of - continued			
Flash Burns	$ 109,125	$ 108,000	$ 116,000
Experimental Planning and Statistics	35,870	35,000	38,000
Additional Research Including Beneficial Applications of Atomic Energy Total	$ 8,474,246	$ 9,696,000	$10,534,000

Representative HOLIFIELD. Mr. Van Zandt has a question.

Representative VAN ZANDT. Doctor, could you furnish for the record the expenditures by the AEC for the R. and D. effort being made in developing peaceful uses of the atom, going back as far as possible, and coming up to date, to show that the amount of money being spent by the AEC annually for the peaceful use of the atom is increasing considerably?

Dr. DUNHAM. Yes, I will be glad to provide that for the record.

(The information referred to follows:)

ATOMIC ENERGY COMMISSION—DIVISION OF BIOLOGY AND MEDICINE

Research costs related to the peacetime use of the atom

[In thousands of dollars]

Actual	Total costs	Less: Fallout sampling and analysis	Costs applicable to peacetime uses
1948	13,238	(1)	13,238
1949	16,806	(1)	16,806
1950	17,999	(1)	17,999
1951	20,677	(1)	20,677
1952	24,084	(1)	24,084
1953	24,966	(1)	24,966
1954	25,189	(1)	25,189
1955	27,825	(1)	27,825
1956	28,834	844	27,990
1957	31,519	1,193	30,326
1958	35,338	1,709	33,629
Subtotal	266,475	3,746	262,729
Estimated:			
1959	43,242	2,648	40,594
1960	49,000	3,564	45,436
Total	358,717	9,958	348,759

[1] Separate amounts not available for these years.

NOTE.—While the major portion of research is related to the performance of responsibilities within the agency, amounts shown above are equally applicable to the development of the peacetime use of atomic energy.

Representative HOLIFIELD. Dr. Weber, will you now proceed with your report? Can you summarize it? We must be out of this room before 1 o'clock. I am sorry to have to rush.

STATEMENT OF DR. FRANCIS J. WEBER,[1] CHIEF, DIVISION OF RADIOLOGICAL HEALTH, PUBLIC HEALTH SERVICE

Dr. WEBER. I have a general summary which would require about 15 minutes of reading or possibly less, and then a short summary which involves a page and which would require much less than that, depending on your wishes.

Representative HOLIFIELD. You can have more time, sir, if you can complete in about 15 minutes or 10 minutes.

Dr. WEBER. Yes; I am certain it can be done in that time. I would like to again express by appreciation for the honor conferred upon me for being given the opportunity to appear once more at this special hearing. Although I have been unable to attend every one of the ses-

[1] See biographical sketch, p. 155.

sions here this week, I have been present at a number of them, especially those which have a special relationship to our Public Health Service program.

This experience, supplemented by the abundant material delivered to the committee, is available to us and provides an unusual opportunity to bring ourselves up to date on all important aspects of the problem. Needless to say, the material is so voluminous and detailed that it will be necessary to study it closely in the coming days to develop the proper appreciation of its implications.

For the present, however, I think certain things can be fairly said with regard to the program content of these hearings, especially as such relates to what especially concerns us, namely, program planning in connection with control of radiation hazards. It is quite evident with respect to surveillance and monitoring programs that information that is both new and necessary for program planning has been gathered since the 1957 hearings.

As a result of expansion of this aspect of the activities, we have a more realistic idea of the present rates of fallout and how long this fallout is likely to persist. Due to the combined knowledge which our several agencies have shared, we also have a more exact notion of the problem areas. Techniques have been developed and refined during the past 2 years which make for improved practices in this respect. The chief problem is that of being able to expand this activity so that coverage in sufficient detail becomes possible. This is absolutely essential if health agencies are to follow the recommendations of your panel on the status and implications of testing as they apply to the use of recommendations of the National Committee on Radiation Protection and the International Commission on Radiological Protection.

As suggested to this committee, the application requires sufficient data to establish averages relative to specific isotopes and specific foods. Further, it is necessary to adjust the MPC's listed in the tables by radiation from other sources. Applications of the MPC principles also assume the existence of standard sampling and measurement methods which now in fact do not exist.

I might add, Mr. Chairman, Mr. Terrill of our staff who is technically competent to discuss that could expand on that if you wish. Dr. Straub of our staff in his presentation of certain Public Health Service data has shown the type of effort now being attempted in certain geographical areas by way of total environmental assessment of radioactivity derived from strontium 90.

This type of analysis should be extended to include not only other radio nuclides, but other sources of radiation. Almost all of the work in this aspect of surveillance and monitoring has been conducted by agencies of the Federal Government. We have on the first day, Mr. Chairman, as you know, submitted a report of our particular activities and I don't think there is any need to repeat that here.

Certain other groups have become concerned with the matter of measurement. At the preliminary meeting of the steering committee called to help with the agenda, I was requested, Mr. Chairman, to submit information on these groups. As a result, the following is submitted:

(*a*) A citizens group in St. Louis, Mo., has taken a particular interest in certain radiological health aspects and has formed the Greater St. Louis Citizens Committee for Nuclear Information. Our

staff has prepared a brief outline concerning the activities of that group. I am offering an outline description as well as a sample copy of their informational bulletin published in March 1959. This is appended to the report. (See vol. 3, app. B.)

(*b*) The Consumers Union conducted milk analyses in 48 cities of the United States during July and August of 1958. Their findings were consistent with ours and those of the Atomic Energy Commission.

(*c*) As you know, the Food and Drug Administration, a sister agency of the Public Health Service within the Department of Health, Education, and Welfare, conducts a monitoring program for foods and drugs. Description of their program has already been furnished to the committee for the record.

(*d*) Certain other groups have requested research grants and assistance related to the monitoring and surveillance program and are seeking by various approaches to make their contribution.

Representative HOLIFIELD. Will you summarize from now on? We are running short of time, and some of the members have to be elsewhere in a few minutes. We will accept your full statement for the record.

Dr. WEBER. Thank you very much.

(The statement referred to follows:)

STATEMENT OF FRANCIS J. WEBER, M.D., CHIEF, DIVISION OF RADIOLOGICAL HEALTH, PUBLIC HEALTH SERVICE, U.S. DEPARTMENT OF HEALTH, EDUCATION, AND WELFARE

I would like again to express my appreciation and the honor conferred upon me, Mr. Chairman and members of the committee, for being given the opportunity to appear once more at this special hearing. Although I have been unable to attend every one of the sessions here this week, I have been present at a number of them, especially those which have a special relationship to our Public Health Service program. This experience, supplemented by the abundant material deliverad to the committee and hence available to us, provides an unusual opportunity to bring ourselves up to date on all important aspects of the problem. Needless to say, the material is so voluminous and detailed, that it will be necessary to study it closely in the coming days to develop a proper appreciation of its implications. At the moment, however, I believe certain things can be fairly said with regard to the program content of these hearings, especially as such relates to what especially concerns us, namely program planning in connection with the control of radiation hazards.

I. SURVEILLANCE AND MONITORING PROGRAMS

It is quite evident that information that is both new and necessary for program planning has been gathered since the 1957 hearings. As a result of expansion of this aspect of the activities, we have a more realistic idea of the present rates of fallout and how long this fallout is likely to persist. Due to the combined knowledge which our several agencies have shared, we also have a more exact notion of problem areas. Techniques have been developed and refined during the past 2 years which make for improved practice in this respect. The chief problem is that of being able to expand this activity so that coverage in sufficient detail becomes possible. This is absolutely essential if health agencies are to follow the recommendations of your panel on status and implications of testing, as they apply to the use of recommendations of the National Committee on Radiation Protection and the International Commission on Radiological Protection. As suggested to this committee, the application requires sufficient data to establish averages relative to specific isotopes and specific foods. Further, it is necessary to adjust the MPC's listed in the tables by radiation from other sources. Applications of the MPC principles also assume the existence of standard sampling and measurement methods which now in fact do not exist.

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Dr. Straub, in his presentation of certain Public Health Service data, has shown the type of effort now being attempted in certain geographical areas by way of total environmental assessment of radioactivity derived from strontium 90. This type of analysis should be extended to include not only other radionuclides, but other sources of radiation. Almost all of the work in this aspect of surveillance and monitoring has been conducted by agencies of the Federal Government. Certain other groups have become concerned in the matter of measurement. At a preliminary meeting of the steering committee called to help with the agenda, I was requested, Mr. Chairman, to submit information on these groups. As a result, the following is submitted:

(*a*) The Consumers Union conducted milk analyses in 48 cities of the United States during July and August of 1958. Their findings were consistent with ours and those of the Atomic Energy Commission.

(*b*) A citizens group in St. Louis, Mo., has taken a particular interest in certain radiological health aspects, and has formed the Greater St. Louis Citizens Committee for Nuclear Information. Our staff has prepared a brief outline concerning the activities of that group. I am offering an outline description as well as a sample copy of their informational bulletin published in March 1959.

(*c*) As you know, the Food and Drug Administration, a sister agency of the Public Health Service within the Department of Health, Education, and Welfare, conducts a monitoring program on food and drugs. A description of their program has been furnished to the committee for the record.

(*d*) Certain other groups have requested research grants and assistance related to the monitoring and surveillance program, and are seeking by various approaches to make their contribution.

(*e*) Many States are already conducting surveillance and monitoring programs, on their own or in cooperation with the Public Health Service.

II. NEEDS FOR FURTHER RESEARCH

Your panels and individual witnesses have indicated that considerably more in the way of fundamental advances of the physical aspects and understanding of the fallout problem have been made than has been the case in the realm of health effects. This is quite understandable in view of the enormous complexity of the latter aspect. Health effects are complicated by both physical factors as well as factors peculiar only to biological systems. Another aspect to which the panels have alluded is that even though strontium 90 may be of major significance, the potential hazard of some of the shorter lived isotopes must be evaluated as they may contribute significantly to the overall health problem. Also, consideration must be given to the individual variations in response to the levels of radioactivity which may occur. This is especially true in terms of the low levels of radiation exposure, which are involved here. This is reflected in the difficulties in the establishment of standards governing exposure. We are not dealing with the simpler end points that prevail in other toxicological conditions. The matter is further complicated by the difficulty of establishing whether there exists a threshold in the very low range. In the case of genetic effects, a linear relationship is assumed and, hence, permissible levels are purposely kept very low.

III. ORGANIZATION OF PROGRAMS TO DEAL WITH THE POTENTIAL HAZARDS OF FALLOUT

I believe that the testimony indicates the areas where work needs to be extended and expanded, as well as leaving room for planning for the greater participation by public health agencies which you and the General Advisory Committee of the Atomic Energy Commission have mentioned. Such a program should include:

(*a*) *Expansion of surveillance and monitoring*

Although a very good start has been made in establishing a framework for a well thought-out program, more needs to be done in the way of specific environmental assessment, in order to provide a sound basis for control measures. As I mentioned earlier, our 1960 budget, if present plans go through, will allow at least a modest expansion of these efforts. I believe a close examination of Dr. Straub's presentation will indicate quite clearly, through an inspection of what is being done, of how that program could be expanded in profitable directions. In doing so, we could arrive more quickly at a position

of knowledge of the factors involved in radiological health comparable to other health problems of longer standing. We have much better laboratory coverage, for example, in all of the States and in various counties capable of doing a wide range of laboratory work, sufficient to help to assess the extent of our health problems. Although, in my own view, this is still far from complete, we are still able to provide a wide range of testing of the various phases of the environment such as bacterial and other contaminating factors in water, milk, and other media, on through the more complicated tests like infectious disease serology and culture for disease-producing agents like tuberculosis and others as well known. Although the effort in radiological health has only recently begun, we find there is a good nucleus in the various States we have visited, a nucleus which could be expanded. It is important that this be done because the first clue to conditions in the environment apropos radioactivity, as these hearings have emphasized, will be given through the specimen collection and subsequent laboratory analyses.

(*b*) *Research needs*

Here again, an extensive effort is going on in our respective agencies (PHS, AEC, DOD, and others), but much more needs to be done. With the techniques already well developed through the liberal support described here, and which have resulted in the development of dependable techniques of measurements, it should be possible now to conduct, on a more extensive scale, analyses of what amounts of radioactivity enter the human system. Studies of certain closed populations, for example, suggest themselves. In such studies, we should be able to measure intake quite exactly as well as measure excretion rates to determine the degree of retention of particular radionuclides. This could be done in different geographic areas where we have now come to a good understanding of extent levels of radioactivity through the work you have heard described here.

Along with this would go additional work that would contribute to the provision of valid criteria for standards. It is perfectly true that much of the available information at the present time can be related, in one way or another, to standards development. At the same time, we believe efforts should be intensified to (1) correlate all existent and future data relevant to the matter, and (2) initiate research in those special areas having special significance to standards development.

In so doing, epidemiological techniques will be among those that must be emphasized. These techniques are being applied now to certain aspects of the problem, of which the Colorado Plateau studies, the analysis of certain background situations such as radioactive drinking water exposures in certain areas of the Midwest and the Northeast, and the followup of the radium watchdial painters are examples. The studies I have mentioned are, by and large, concerned with low-level radiation exposure, although in some cases people are receiving exposure dosages near, or occasionally above, those considered permissible. I might add that this is difficult and that funds are not the only answer. Fortunately, however, it is a field where local and State health agencies, through the experience of their activities which you have mentioned, could give valuable assistance in helping to elucidate the problem. Activities now under way in certain States furnish concrete examples of such possibilities.

(*c*) *Radiological health control activities*

This should be a comprehensive program, seeking to understand and control all sources of radiation, of which that derived from fallout is a part. We feel that radiological health control activities can be carried on within the structure of existing public health programs now pursued on a nationwide basis. Competence already exists in such programs like water supply and water pollution control, milk and food sanitation, occupational health, maternal and child health, and other programs. Control measures are carried on by these programs which can be adapted and expanded to encompass certain phases of the techniques of reduction of exposure of the population to radiation. In all cases, there is a beginning made by way of analysis of the problem, the factors that affect its spread, its cyclical variations, and the methods of prevention, regulation and control. Their success resides on a basis dependent upon as exact knowledge as possible of community population status with respect to these.

Furthermore, the program of radiological health should be developed at the State and local level, with the Federal agencies giving assistance in the areas

where the National Government normally makes its best contribution. The latter would be particularly in the areas of research demonstrations and the training and assignment of much needed personnel while the State and local governments increase their capabilities. This will, of course, be discussed more extensively at your forthcoming Federal-States relations hearings.

COMMITTEE FOR NUCLEAR INFORMATION, ST. LOUIS, MO.

The Committee for Nuclear Information was organized in April 1958 by a communitywide group of interested citizens for the purpose of collecting and distributing to the public factual information about the effects of nuclear war, nuclear testing and nonmilitary uses of nuclear energy. The group has 500 members at present and is supported as a nonprofit organization by voluntary contributions from its members and others. The committee's activities include publication of a monthly information bulletin which reports new developments on nuclear energy in nontechnical language, and a speakers' bureau which sent 20 scientists as speakers to about 50 speaking engagements during the past few months. Also sponsored by the committee are public lectures on radiation problems and scientific seminars on these subjects. Dr. Alexander S. Langsdorf, dean emeritus of Washington University School of Engineering, is president of the organization.[1]

A program for the collection and radiochemical analysis of deciduous teeth has been organized by a CNI subcommittee under the leadership of Dr. Louise Reiss, M.D., an internist who has been named director of the project. Other members of the subcommittee are Dr. Barry Commoner, professor of plant physiology at Washington University; Mrs. Leslie E. Heinz; Mrs. Joseph P. Logan; Dr. Eric Reiss, M.D., assistant professor of preventive medicine and medicine at Washington University School of Medicine; and Dr. Alfred S. Schwartz, M.D., assistant professor of clinical pediatrics at Washington University School of Medicine.

Officers and executive board of the committee include:

Professor Alexander S. Langsdorf, President
Dr. Alfred S. Schwartz, vice president-treasurer
Professor Barry Commoner
Professor Edward U. Condon
Dr. Samuel B. Guze
Professor John M. Fowler
Professor Irwin H. Herskowitz
Dr. Park J. White

[From Consumer Reports—A reprint prepared by Consumers Union in the public interest March 1959]

A study—the most thorough of its type yet undertaken—of the effects of fallout on

THE MILK WE DRINK

Based on laboratory tests of samples collected from 50 cities throughout the United States and Canada

THE MILK ALL OF US DRINK—AND FALLOUT

(For Comments on this Report, see vol. 3, app. B)

New public health problems are raised by the effects of radioactive fallout on vital foods. Herewith a 10-page report, evaluating the present and potential hazard and including test data for the milk supply in 50 cities.

Ever since radioactive coral fell as white ash across the decks of the Japanese fishing boat *Fortunate Dragon* in the spring of 1954, fallout from nuclear bombs has been making both news and history. Its involvement with man and his food supply has entangled it in issues ranging from the biological to the diplomatic.

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[1] From Dec. 30, 1958, news release of committee.

Nor have the public press, official statements, or comment from independent scientific sources unraveled the tangles. What fallout means to us, and what to do about it, still are the most controversial issues of all.

The radioactive materials which fall out of the upper atmosphere after nuclear blasts pass through a number of physical, chemical, and biological processes, some of which take years to occur. Each of these steps is a link in a long chain of events which connect the blasts to damage in people, the living and the yet unborn, even thousands of miles away and years later. The scientific facts about some of these links have been well established. Our knowledge of other important links is vague and incomplete. It remains necessary for us to arrive at some assessment of the hazard, even though not all the necessary evidence is in.

Every day each person in the world is exposed to and consumes some measurable debris from fallout in his food, in his drink, in the air he breathes. What this may mean to him in general—but with particular reference to radioactive strontium 90 in the U.S. milk supply—is the substance of this report.

It seems wholly right for a consumer testing organization to undertake to evaluate a new constituent of our food, even one not proclaimed on any label. The main sources of information on the problems of fallout have been the Atomic Energy Commission and, to a much lesser extent, other Government agencies such as the U.S. Public Health Service and the Food and Drug Administration. Other governments and the United Nations also have published extensive data on fallout; but, as a congressional committee reported in 1937, "Information on fallout has evidently not reached the public in adequate or understandable ways."

For at least a decade, the AEC has been studying fallout problems of all kinds—physical, chemical, and biological. Measurements have been made of radioactive fallout materials in air, soil, water, people, and foods—including milk, which is the principal source of strontium 90 in the diet of Americans. The responsibility of the Public Health Service for safeguarding the health of the country has led it, too, since 1956, to study fallout materials in air and food, particularly in milk. But these studies have been mainly exploratory, to study methods and to obtain typical values; they have not explored as yet the full gamut of possible sources of radioactive materials in man's diet and in his environment. With respect to milk, the samples examined have not necessarily represented the milk being drunk by the majority of consumers even in the few areas studied.

By testing samples of milk purchased over store counters in 50 widely distributed places in the United States and in Canada, Consumers Union has sought to extend the work done by the Government agencies. The results, which are presented on pages 108 to 110, serve to check the relatively scant data on milk published by the AEC and the PHS, and to extend their coverage to include many more areas. That a private organization of limited means can carry out such a program suggests that an expanded monitoring network should be economically feasible under Federal, State, or even community or dairy auspices.

Consumers Union goes further. Though the interest of the PHS in the matter has led it to equip and staff some very competent laboratories, it is still true that the overall problem has remained the province of the AEC. But it is hard to see why judgments on matters of public health should have to depend primarily on the reports of the very agency charged with the responsibility of manufacturing nuclear weapons, rather than on those of an agency whose specific job is to safeguard the public health. Without reflecting in any way on the AEC's competence or integrity, Consumers Union would support measures which would lead to thorough and independent investigations (as well as routine surveillance) by the Public Health Service on fundamental biological and control problems, wherever there is concern with fallout.

The fact is that fresh clean milk, which looks and tastes just as it always did, nevertheless contains (wherever you get it these days) an unseen contaminant, a toxic substance known to accumulate in human bone. There is an analogy: bacterial contamination. There, too, the presence of unsensed contaminants can bring grave harm. Most of us, even if we are not bacteriologists, know by now the danger from germs, and yet know how to estimate it rationally, and not to fear it unreasoningly. It is exactly with this kind of reasoned concern that we seek to approach the more novel problem of fallout contamination.

(In the text following and on the next few pages, before we proceed to the results of our analyses of milk samples, we propose to tell something of the story of low levels of radiation as they existed in the pre-atomic-bomb era, and

of how they have been changing since. For it is only against this background that the findings have meaning.)

THE LIVING AMPLIFIER

Why fallout creates a problem

In the time it takes you to read across this line, many thousands of the individual atoms which make up the matter of your own body spontaneously disintegrate. These disintegrations occur in all human beings, in all living things. Radioactive materials always have been with us in extremely minute quantities. They exist in the foods we eat and consequently in the tissues of our bodies. Some radiant energy produced by the disintegrations, rather like the X-rays to which we may be subjected by a dentist or physician, passes right out of the body. If you climb into a "counting cylinder" (see photo), the special liquid inside the tank's wall will react to this radiation and translate it into tiny pulses of light.

Our senses cannot detect such radiation at all—which does not mean that it is without consequence for living matter. Just as the rays by their bombardment produce light pulses within the special liquid of the tank, so they produce changes in the living cells through which they pass. The energy thus imparted within these cells is small in total amount, but it is intensely concentrated: an average of a few hundred atoms will be altered chemically in every living cell through which the rays pass. And thus begins a biological chain of events.

For living matter acts as a kind of amplifying system, somewhat analogous to the electronic circuits of the laboratory counter. The living amplifier takes time; it is slow to respond. Sometimes the cell which was damaged by traversing rays will recover. But the damage to one or a few cells may become in time a self-regenerating and uncontrollable tumor. If the cells affected are cells of the special reproductive tissues, the damage may be done to the subtle molecules of genetic material which contains the blueprint for the next generation; such damage is what we call genetic. When the damage is to a cell of the body not engaged in reproducing the next generation, the effect is said to be somatic. In its most serious form the somatic damage may be malignant (cancer or leukemia); in milder form it may appear as an apparent acceleration of the aging process. For very low levels of radiation, it may be that no somatic effect is observable.

The "counting cylinder" in the photograph across the page includes in its count the rays from a perfectly normal constituent of all living cells: potassium. This chemical element is essential to life, as every gardener knows; one of its isotopes (isotopes are chemically identical but physically different atoms) is radioactive. In a normal 160-pound man, about 5,000 atoms of this isotope (potassium 40) decay each second throughout life. Radiation passes through us not only from internal sources like the potassium in our muscle tissue, but from external sources as well. The very rocks around us contain uranium and thorium in small amounts, along with their radioactive relatives, particularly radium. And there are the cosmic rays, which come in from interstellar space to strike us.

In short, we know that life always has grown in the midst of penetrating radiation. The question must be put: How much has that radiation been? We have to work out some answer to that question before we can proceed to the next one: How much more, if any, can we tolerate?

THE "PERMISSIBLE" RADIATION BURDEN

The adjective doesn't mean what many people think, and no one knows precisely what it does mean; but here's how it's worked out:

There are many ways of measuring radiation. The measure which we shall use is a measure of the energy absorbed from radiation in a given amount of tissue: the unit is called the rad (100 ergs absorbed per gram of tissue).[1] This is not much energy; in terms of heat, 500 rads would serve to raise the temperature of the tissue only $\frac{1}{500}$ of a degree Fahrenheit—imperceptible. But that same 500 rads delivered to a human being by penetrating radiation, in a kind of three-dimensional sunburn, is deadly: a man who receives that much over the whole body within a short time has just about an even chance of surviving a month. The normal external dose is, of course, very much less.

[1] A rad is roughly equivalent to a roentgen, which is an older, common unit for doses of X- and gamma rays.

It it interesting to tabulate some external doses of radiation received by people in a lifetime (about 70 years) under ordinary conditions. The dose varies from place to place. The high mountains have a thin layer of air above them, and the cosmic rays from space, accordingly, are in abundance there. Some places have subsoil rather free from radioactive elements, and the earth's contribution is small there. Here is a small table of typical results (taken from an AEC source):

External radiation doses

Place:	*Rads per 70 years*
Toledo, Ohio	5.3.
Pittsurgh, Pa	6.7.
Little Rock, Ark	7.4.
Colorado Springs, Colo	12.
Average over U.S.A	about 7 (out-of-doors).

Other common sources of external radiation are: from TV tubes and luminous watch dials, less than ⅛ rad per 70 years; from diagnostic X-rays (in the United States at least—see Consumer Reports, September 1958), an amount generally not exceeding 10 rads per 70 years.

In Kerala, at the southern tip of India, about 100,000 people live in fishing villages strung out along a hundred miles of a geological curiosity, an ocean beach whose black sands are radioactive: the people who live there have always lived in a radiation field about 10 times the U.S. average. If adequate medical diagnosis and vital statistics existed for these people, their very long exposure might furnish extensive information on the long-term effects of greater-than-average radiation on human beings. Unfortunately, we have no such statistics for them or for any other highly exposed population, and no way of quickly acquiring such data. Nevertheless, as radiation gradually increases from man's activities in the atomic age, we obviously must attempt to make such estimates as we can to find out where the risk gets serious, where intolerable.

THE TOLERANCE DOSE

The view that there exists some degree of exposure below which there is no appreciable harm often is expressed in terms of a threshold dose. Cross the threshold, and danger begins: stay below, and the processes of recovery and repair reduce to negligible proportions the chance of visible amplification of cell damage. It is claimed that something like a threshold, in fact, has been observed in animal and even human exposures, although not at the low, long-continued doses in which we are now interested.

It is fairly well agreed, however, that genetic damage, which in humans leads to embryonic deaths, stillbirths, and congenital defects, has no threshold. Every increase in the dose which affects the genes of some person is going to appear statistically in his progeny. Most of this damage is believed to be delayed in expressing itself: some remote generation may be the first to suffer from it. Such genetic damage raises quite new problems of medicine, to say nothing of morals. And the fact that the extent of such damage to humans is not known with precision tends to cloud discussions of the problem.

In the words of the authoritative International Commission on Radiological Protection (ICRP): "Since no radiation level higher than the natural background can be regarded as absolutely 'safe,' the problem is to choose a practical level that, in the light of present knowledge, involves a negligible risk." The practice has been to accumulate information bearing on this problem by surveying considerable numbers of individuals who work with radium or X-rays, or in the atomic energy industry. On the basis of such information, permissible levels (or tolerance doses) for exposure to external radiation have been established.

But the history of this concept is not reassuring, because today's permissible level is not yesterday's; the level for such workers has been modified steadily, as shown below:

	Rads per year
1928–36	100
1936–47	35
1948–57	15
1957	5

The last figure corresponds (for a 50-year working life) to some 40 times the natural U.S. background radiation of about 7 rads in the course of a lifetime.

Radiation workers, however, are never under 10 years old, nor often over 70. By their relatively small numbers, they do not represent a fair sample of the diversity of mankind. Just as there are some people who have been found unusually sensitive to sunlight, so there must be some who are more sensitive than others to radioactivity. For such reasons it has been considered desirable to set the tolerance dose for the entire population lower than that for medically controlled radiation workers. Typically, the maximum "permissible" dose for occupationally exposed people is reduced by a somewhat arbitrary factor of 10 to obtain the maximum "permissible" exposure for people at large.

By such a standard, the general population ought not to receive more than about 30 rads per lifetime, or roughly four times the U.S. background radiation. This does not mean that such a dose is guaranteed to be safe, or that it will produce no damage. On the contrary, it implies accepting as socially tolerable some genetic damage, which can be estimated only very roughly because of our poor knowledge of human genetics.

Estimates, nonetheless, have been made. The U.N. Scientific Committee on the Effects of Atomic Radiation, for example, has attributed to one rad, from any source and applied only once to a single generation of the world's entire population, the capability of causing ultimately 100 to 4,000 defective births for each million of the population. The percentages are small (between 0.01 percent and 0.4 percent) and the range is large (a tacit admission to the inexactitude of our knowledge). But when applied to the world's population of about 2½ billion people, half of whom are assumed to be below the mean reproductive age, the total number of defective births will range from 125,000 to 5 million,[1] spread out over scores or even hundreds of generations. These defectives may not be discernible from among the much larger number of cases which are a result of other unknown factors.

Assessments can also be made for the somatic damage induced by radiation. While there is common acceptance of the inevitability of genetic damage, however, there is no such agreement regarding somatic damage. Many scientists believe that a radiation threshold exists for somatic effects, that we have not yet exceeded this value, and therefore that no somatic damage (cancer, leukemia, shortening of the life span) can be attributed to the low levels of radiation to which we have been exposed so far. Other scientists hold to the view that biophysical distinctions cannot be made between somatic and genetic effects and that radiation which causes damage to reproductive cells can also damage any other living cells. No one knows at this time which view is correct.

INTERNAL RADIATION

In addition to the radiation received by the body from without, at least three substances contribute their radiation from within the body, and have done so since there first were men: potassium 40, carbon 14, and radium and its radioactive decay products.

Carbon and potassium are necessary components of life; they are fairly uniformly distributed within the body and from person to person, so it seems not unrealistic to be guided by the tolerance dose information developed in our studies of external radiation. It is plain that potassium 40 (which contributes about 1.5 rad per 70 years) and carbon 14 (about 0.1 rad per 70 years) have not added much to the natural background radiation.

But radium is another story. It plays no known role in the life process; it is present because it is widely distributed in the soil and rocks. It is ingested, or sometimes taken into the lungs, and though it is present in very small amounts, its activity can be more important than that of potassium 40 or carbon 14.

Radium is not uniformly spread through the body. Because its chemical behavior closely resembles that of the element calcium, constituent of all bone, radium concentrates in the bones. Furthermore, in large doses it does not distribute itself uniformly even within the bones. The bone grows most, of course, in youth. But even the mature bone undergoes remodeling; its constituents are steadily being taken up and laid down again. This means that where radium lodges depends on just what part of the bone is active when the radium enters, and on the whole complex matter of bone formation, growth, and maintenance.

The energy emitted in the decay of radium is chiefly carried by particles

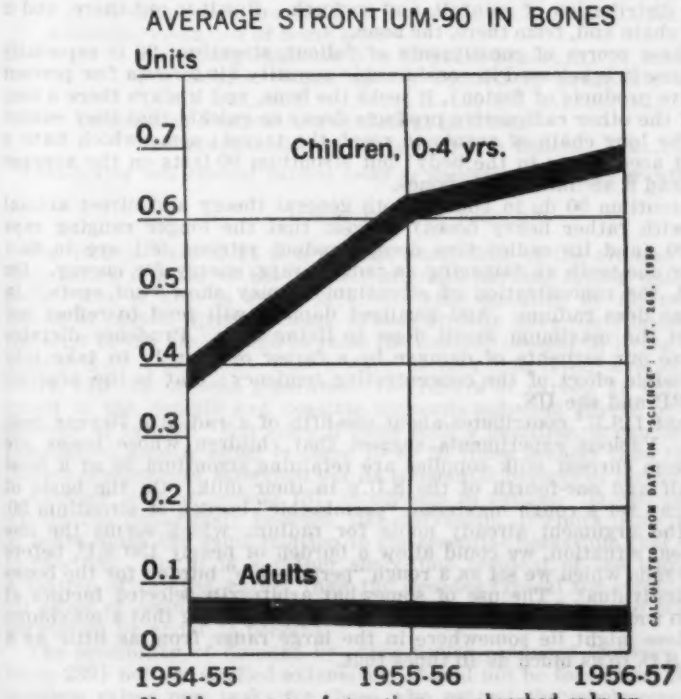
[1] A conservative estimate, based on the assumption that the world population will not increase.

which cannot penetrate the whole body to reach the outside. They spend all their energy in a distance of a thousandth of an inch; they deposit a thousand times as much energy in crossing a cell as do the rays from potassium 40. The variability of the site of radium also implies its possible concentration in local "hot spots" beyond the average value (see photo above).

A hundred or so persons have been examined who have held excessive amounts of radium in their bones for decades. Some of these were young women who painted the small numerals on luminous dials, and tipped the brushes between their lips for the fine work. Some were suffering from rheumatism or other diseases, and were given radium as a therapy by some sincere physicians and by mere quacks, in the 1920's before the grave danger was recognized. In all of these groups, there have been seen malignancies of bone and of blood—evident consequences of the radium irritation.

What is striking is that the incidence of these conditions rare in the total population, was large enough in the small number of radium-poisoned patients to be recognized without chance of error. There is a good chance of developing a bone tumor, or leukemia, if you fix in the bones a mere speck of radium; less than one microgram, an amount one thirty millionth of an ounce, weighing less than the dot over an inked i. It seems very probable that, if there is a safe threshold at all here, it is below a tenth of a microgram, an amount which is a few hundred or a thousand times greater than that found in the normal skeleton. The present ICRP recommendations allow a tenth of a microgram of the stuff for workers in radiation industries, and such an allowance does not preclude the possibility of undetected subclinical changes. If we recall that there may be no threshold for somatic effects, but a straight-line relationship, and that individual responses are variable, it is reasonable to include at least a factor of 10 for safety in setting the "permissible" internal bone burden for the population at large. The U.N. places the normal average radium dose to the bone at a value of about 3 rads over 70 years. Somewhere in the range from ten to a hundred times the background value the "permissible" value lies.

Now there enters a new factor: the element strontium.



Notable amounts of strontium are absorbed only by growing bones. Averages above were calculated on the basis of data compiled from four U.S. cities

Strontium 90: A Special Hazard

The element strontium is relatively uncommon; its most common use is for the rich red of its flame in fireworks and flares. Like radium, it also resembles calcium and it also becomes stored in bone.

The normal skeleton of man has about 2 pounds of calcium, and with it from soil and rock there has always been deposited about a tenth of a teaspoonful of strontium. Normal (pre-atomic-bomb age) strontium presents no hazard, so far as anyone knows; it is not radioactive.

But every explosion of a nuclear bomb spreads into the high air with its fireball a new radioactive isotope of strontium, strontium 90. It comes from fission, and the old-style fission bomb and the new-style fission-fusion-fission bomb both contribute strontium 90. (The yet undeveloped "clean" bomb will contribute proportionately less as it makes less use of fission energy; no one has yet discovered how to eliminate the stuff entirely.)

So far some 200 pounds of strontium 90 have been carried aloft by the churning hot gases of the mushroom explosions. It has spread worldwide. It falls out of the upper atmosphere, in a manner not yet fully understood. It comes down slowly with rain and snow, on river and reservoir, plant leaf, and soil. Some of it is taken up chemically through the roots of plants to become part of grass or seed, and eventually part of the glass of milk or the rice in the bowl. In the United States, about 80 percent of the minute quantity which enters our bodies comes to us in our milk. In Japan more than two-thirds comes from rice. Unpolished or whole wheat, unfiltered rainwater, and similar items of diet, such as vegetables, may also contribute a significant amount of strontium 90. If there never are any more explosions, the fallout from tests already made will reach a maximum about 1970, rising to 2½ or 3 times its present value, and then will decline slowly for a generation or two.

There is not much of it. The load of strontium 90 in your front yard is likely to be about one-tenth of a microgram. It is spread down into the topsoil by rain and plant growth and hoe and worm; how much is taken up by plants depends on a wide variety of conditions—upon the depths of their roots, the chemistry of the soil, the distribution of rainfall, and so forth. But it is out there, and it enters the food chain and, from there, the bone.

Among all these scores of constituents of fallout, strontium 90 is especially hazardous because it is released in considerable quantity (it forms a few percent of all radioactive products of fission), it seeks the bone, and it stays there a long time. Some of the other radioactive products decay so quickly that they cannot pass through the long chain of events to reach the target; some which have a long life do not accumulate in the body; but strontium 90 lasts on the average about 40 years and it accumulates in bones.

What will strontium 90 do in bone? Both general theory and direct animal experiments (with rather heavy doses) suggest that the longer ranging rays of strontium 90 (and its radioactive decay product yttrium 90) are in fact only one-fifth or one-tenth as damaging as radium rays, energy for energy. On the other hand, the concentration of strontium 90 may show "hot spots," in the same way as does radium. And localized damage will tend to reflect not the average but the maximum insult done to living cells. Prudence dictates that we increase our estimate of damage by a factor of 5 or 10 to take into account the possible effect of the concentrating tendency; that is the practice of both the ICRP and the UN.

It appears that 1 S.U.[3] contributes about one-fifth of a rad in a 70-year residence in bone. Various experiments suggest that children whose bones are being formed from current milk supplies are retaining strontium 90 at a level between one-half and one-fourth of the S.U.'s in their milk. On the basis of these facts we can set a rough maximum "permissible" burden of strontium 90. If we follow the argument already made for radium, which seems the one closest to the real situation, we could allow a burden of nearly 150 S.U. before reaching the 30 rads which we set as a rough "permissible" burden for the bones of an average individual. The use of somewhat arbitrarily-selected factors at several stages in such calculations may be viewed as suggesting that a maximum "permissible" dose might lie somewhere in the large range from as little as a fifth of the 150 S.U. to as much as 10 times that.

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[3] Abbreviation for Strontium Unit. Expresses the number of micromicrocuries of strontium 90 per gram of calcium.

It does not seem safe, however, to apply to the poorly known and highly variable bone burden, affecting the whole world's people, any smaller safety factor than that used for external radiation. In this case one would again set the limit at something near 30 rads, several times the natural internal burden. This gives us our 150 S.U. again, which agrees well enough with the ICRP limit of 1955, put at 100 S.U.

But it must be emphasized that any "permissible" level obviously is predicated on the assumption that there is a threshold dose for strontium 90 below which any effects are insignificant. The absence of a threshold would mean that bone damage would be proportional to the dosage in any amount, and the concept of a tolerance dose would then imply accepting as socially tolerable an unknown number of radiation-diseased individuals.

The summary table on this page presents average ranges for radiation dosages, based on authoritative estimates. It should be emphasized that these figures are approximate. Even if the population has an average dose rate within the "permissible" limit, there are many individuals, of course, who are receiving much more.

Radiation dosages summary table

	Rads/lifetime (USA) (approximate)
External:	
Maximum "permissible" dosage, for the population at large	30.
Natural: cosmic rays, soil, rock	4 to 10.
Man-made, at present: diagnostic X-rays	5 to 10.
Fallout	Up to ½.
Other sources, including TV tubes	Up to ½.
Approximate total	10 to 20.
Balance to reach "permissible" level	20 to 10.
Internal:	
Natural: carbon-14, potassium-40, radium	2 to 6.
Man-made strontium-90 (bone dose):	
Maximum "permissible" dosage calculated from the ICRP recommendation of 100 S.U	Up to 20.[1]
Assuming cessation of bomb tests—	
For young children, 5 to 10 years hence	½ to 1.[1]
For adults (bones mostly formed in preatomic bomb era)	Up to 1/10.
Assuming continuation of tests at past rate—	
For young children, 50 to 60 years hence	5.[1]

[1] Not taking into account natural decay or deceleration of bone growth with age.

OTHER FALLOUT PRODUCTS

The isotope carbon 14 is a normal component of the air and of our food; it contributes a little to the normal internal burden, rather uniformly over the body. But it also is made in appreciable quantity by all nuclear weapons, fission of fusion, dirty or clean. These already have contributed a good deal of the stuff to the air, enough to raise the amount present in living things by about one-third of 1 percent. This isotope is very long-lived, averaging about 8,000 years. In the present generation, the effects of carbon 14 will be small, compared to the genetic and possible leukemia-inducing effects of the other components of fallout. But since it lasts such a longtime, it can be shown that its cumulative genetic effect is some 10 or 20 times greater than that of the other fallout components, stretched out as congenital defects of posterity.

Another isotope, cesium 137, is copious in fallout. It can be detected from within the living body because, like potassium, it emits penetrating radiation. It bears much the same relation to the essential potassium that strontium bears to calcium. But it is a rather short-time resident of the body, staying there a matter of months, rather than the decades typical for strontium. The overall effect of cesium is to increase the does received from radiations like that of potassium by about 15 percent.

The possibility of damage by other fission-produced isotopes (such as plutonium 239) not yet studied extensively should not be forgotten; the whole fallout problem raises new tasks for those who control the wholesomeness of modern foods, water, and air.

The milk samples for CU's tests were bought in these 50 localities. The figures represent the number of strontium units found in the samples from each locality last summer. A full list is given in chart form on the facing page, along with comparable figures from other surveys

Consumers Union's Study—Data and Meaning

For reasons advanced on the preceding pages, strontium 90 assumes a degree of importance far out of proportion to the seemingly small absolute amounts involved. It is, in fact, generally agreed that strontium 90 presently constitutes one of the most serious potential hazards attributable to fallout, and that milk is the primary route by which it enters the bodies of most Americans.

The U.S. Public Health Service has been analyzing milk (for strontium 90 and other radioactive isotopes) once each month since 1957 in five geographic areas; this program was enlarged in 1958 to include 10 areas. Sample collections for analyses are made on a single day of each month from each area and represent a single group of farms from the same production area. Because these samples do not include milk from other farms which contribute a substantial part of the milk supply to consumers in most of the areas covered, the samples do not necessarily represent the actual milk supply of a given population center.

The Atomic Energy Commission has reported on milk in several U.S. cities, but it has covered only four of them with any degree of thoroughness. Except for New York City, samples examined by the AEC usually have been powdered milks, which may not be typical of milks found in the consumer market (a fact observed, in at least one instance, by the AEC itself).

There have been other more limited studies of the strontium 90 content of milk. The University of Chicago, for example, has been closely monitoring the Chicago area, and Columbia University's Lamont Geological Observatory has published data on powdered milks from selected areas of the country. The New York State department of health has monitored the strontium 90 content of powered milk from nine locations within the State.

CONSUMERS UNION'S PROGRAM

All of these studies have left largely undetermined and unknown the strontium 90 content of the milk supply for most segments of the population. Consumers Union's study, reported herein, is the first to attack this problem on so large a scale from the point of view of the consumer. Broad coverage of consumer milk supplies on a more comprehensive national geographic basis than has heretofore been made was the prime objective of Consumers Union's project. It was undertaken also to provide an independent, non-government-sponsored check of data released through official agencies, and to point out areas having milk with relatively high strontium 90 content which may merit further study by official agencies.

SAMPLING

CU's milk samples were gathered from 48 cities scattered across the United States and from 2 in Canada close to the U.S. border. Several considerations went into the selection of these sites. Foremost of these was the requirement for a good extensive sampling network, with emphasis, wherever feasible, on the larger centers of population. Various distinctive soil regions of the United States—as classified by the Department of Agriculture—were also included, since soil conditions may be considered as the integrated end-products of climatic, topographical, and geologic effects—all of which are important in affecting the strontium 90 content of soil, which in turn affects the strontium 90 content of pasture, cow, milk, and man.

All areas whose soils had been found by AEC studies to be high in strontium 90 also were included, as well as several sites whose milk had been tested previously by the PHS and the AEC.

At each of the cities selected, CU shoppers purchased 8 quarts of fluid milk each week, for a period of 4 weeks. Generally, two separate across-the-counter purchases were made each week of 1 quart from each of four major local dairies. Four-ounce portions from each of the 32 quarts thus purchased were composited to provide a total of 1 gallon of milk for testing from each sampling site. The pooled composites thus obtained represented a major part of the milk supply of each of the cities sampled for the period from mid-July to mid-August 1958. A total of approximately 1,600 quarts were purchased for these tests.

Analyses for strontium 90 were made for CU by a consulting laboratory which specializes in such work, and whose reliability had been established by check-sample data provided by the AEC as well as by CU.

In drawing up its program for sampling and testing milk, CU consulted the Lamont Geological Observatory, the Atomic Energy Commission, the U.S. Department of Agriculture, and the U.S. Public Health Service, and gratefully acknowledges the suggestions made by these organizations.

RESULTS

The data obtained by CU, together with some related results from other laboratories, are shown in the table on this page. CU's data, it should be emphasized, provide breadth rather than depth of coverage. Any trend or variation of strontium 90 content with time or other factors cannot, of course, be noted by a one-period sampling and analysis. The best data available for observing the progressive change in milk's strontium 90 content probably are the monthly figures of the Public Health Service.

Concentrations of strontium 90 in milk

Cities where samples were taken	Consumers Union July–August S.U.	Public Union Service 1958 S.U.[1]	Atomic Energy Commission	
			S.U.	Sampling period
Alabama, Birmingham	7.4			
Alaska, Juneau	4.9			
Arizona, Phoenix	3.3			
California:				
Los Angeles	1.9			
Sacramento		4.6		
San Francisco	2.6			
Colorado, Denver	5.9			
District of Columbia, Washington	9.8			
Florida, Miami	4.6			
Georgia, Atlanta	7.9	9.4		
Idaho, Boise	6.1			
Illinois, Chicago	12.6	7.6		
Indiana, Indianapolis	7.6			
Iowa, Des Moines	10.6			
Kansas, Wichita	8.1			
Kentucky, Louisville	9.8			
Louisiana, New Orleans	15.6			
Maine, Bangor	7.1			
Massachusetts, Boston	13.9			
Michigan, Sault Ste. Marie	5.4			
Minnesota:				
Duluth	11.5			
Minneapolis	9.2			
Mississippi, Jackson State College	8.6		4–6	May to September, 1956.
Missouri, St. Louis	11.1	13.4		
Montana, Great Falls	8.8			
Nebraska, North Platte	9.4			
Nevada, Las Vegas	2.3			
New Mexico, Santa Fe	5.1			
New York:				
Buffalo	6.6			
Perry			3	January to March, 1958.
New York City	10.5	6.5	4	Do.
North Carolina, Charlotte	9.5			
North Dakota:				
Bismarck	14.1			
Fargo		11.9		
Mandan			20–30	October to December, 1957.
			15	January to March, 1958.
Ohio:				
Cincinnati		9.4		
Cleveland	8.8			
Oklahoma, Tulsa	8.7			
Oregon, Portland	6.6		5–6	April to June, 1956.
Pennsylvania:				
Philadelphia	5.0			
Pittsburgh	13.7			
South Carolina, Columbia	6.9			
South Dakota, Rapid City	11.2			
Tennessee, Memphis	13.6			
Texas:				
Austin	2.5	3.0		
El Paso	3.3			
Houston	4.4			

Concentrations of strontium 90 in milk—Continued

Cities where samples were taken	Consumers Union July–August S.U.	Public Union Service 1958 S.U.[1]	Atomic Energy Commission	
			S.U.	Sampling period
Utah, Salt Lake City	5.2	4.1		
Virginia, Norfolk	10.6			
Washington:				
Seattle	10.1			
Spokane	6.2	[2] 5.9		
West Virginia, Charleston	7.3			
Wisconsin, Columbus			4	January to March, 1958.
Wyoming, Casper	7.8			
Canada:				
Quebec	13.6			
Winnipeg	9.5			

[1] Figures were obtained by averaging the July and August single-day samples, and have been converted from the micromicrocuries of strontium per liter in which the PHS data originally appeared.
[2] August only.

From these and other studies, there is incontrovertible evidence that the strontium 90 content of milk has been increasing since 1954. The average rate of increase was estimated by the AEC in 1957 to be between 0.5 to 1.0 S.U. per year for past years. The overall average of 48 U.S. cities sampled by CU is 8.0 S.U.; the average of the 10 cities covered by the PHS for approximately the same period is 7.6 S.U., a figure that is in good agreement with that found by CU considering the differences in the sites sampled and in the nature of the samples. For purpose of noting change, an overall estimate of the strontium 90 content of milk in 1957 may be obtained by averaging figures published by the PHS for five cities sampled that year (4.35 S.U.) or by taking the average which was published by Lamont (6.1 S.U.). It would appear from these figures that the strontium 90 content of milk in mid-1958 was more than two S.U. greater than it was in 1957, which is at least double the AEC estimate for the annual rate of increase over the last few years. This more rapid rate of increase must be considered tentative, since data for all of 1958 are not yet available. The summer months, when cows graze on the open range, are more likely to yield milk with a higher strontium 90 content than winter months, when cows are fed on stored hay and fodder. Offsetting to some degree such an anticipated drop in the strontium 90 content of milk for the latter half of 1958 will be the effects of fallout from the stepped-up rate of weapons testing during the period August to November 1958. Because of this testing, it may be assumed reasonably that the average strontium 90 content of milk as currently consumed in the United States is about 8 to 10 S.U., and will likely be closer to the higher figure during the coming summer.

However, an average may be falsely reassuring. Local meterological conditions and other factors conceivably could create conditions resulting in high concentrations in relatively small areas that embrace large numbers of people. In CU's tests, a range from about 2 S.U. to almost 16 S.U. was found in the composited samples. It has been estimated that a factor of five applied to the average found would encompass most of the variations that might be expected for all factors. In such "hot" areas the concentration of strontium 90 in milk might be about 50 S.U.—more than half the currently recommended maximum "permissible" concentration.

In its tests of samples collected in July and August 1958, CU found that the strontium 90 in the milk supplies of several cities was about twice the average of the others. These seven cities were:

	S.U.
New Orleans, La	15.6
Bismarck, N. Dak	14.1
Boston, Mass	13.9
Pittsburgh, Pa	13.7
Memphis, Tenn	13.6
Quebec, Canada	13.6
Chicago, Ill	12.6

Several other cities were well above average, though not so much as those above:

	S.U.
Duluth, Minn	1.15
Rapid City, S. Dak	11.2
St. Louis, Mo	11.1
Norfolk, Va	10.6
Des Moines, Iowa	10.6
New York City	10.5
Seattle, Wash	10.1

As was to be expected, most of these cities are in the northern part of the country; several high-count areas were found in the southern part of the United States, too. Ten of the 13 U.S. cities just listed are not included in the present PHS test network. It appears to CU that the PHS would do well to extend its testing to include at least these 10. Furthermore, it appears to be desirable to investigate the milk supplies of all other areas which have not been included up to now in any study, in order to locate any other areas whose milk is abnormally high in strontium 90. It is further suggested that the study of milk supplies might profitably include provision for more representative sampling than has been the practice in the past.

What's To Be Done?

This report cannot be ended with a clear recommendation. None exists. No doubt the best buy is milk without strontium 90, air without fallout, and adequate medical care without diagnostic X-rays. But none of these solutions are to be had, and it would be as foolish to stop drinking milk as it would be to refuse an X-ray examination for a broken limb. The surveys of the strontium 90 content in milk made by CU and by other agencies have demonstrated that there is a potential hazard. A judgment as to whether we are now within or without prudent limits depends on a variety of uncertain factors—ranging in character from the nature of bone growth to the problem of leukemia induction by X-rays—the answers to which have not yet been set by science. Even if those answers were in, we are far from knowing how variable can be the responses of man and weather and soil the world around.

Here is a new problem in public health: a worldwide hazard which neither man nor nature can wash away.

We can surmise that we still are not heavily dosed, but we also can be sure that there have been unattributed individual tragedies caused to persons by fallout. Further, we can project the data here presented in weighing the consequences of a major war fought with nuclear weapons. It is probable that in such a war the surviving belligerents would find themselves confronting many thousands of times the average fallout hazard which tests to date have produced, and neutral lands across the globe could expect hazards a thousand times greater than what they have now.

What's to be done? Much research is now being carried on, but investigation of all the unknown factors is urgently needed. Among other areas, practical measures to reduce the absorption of strontium 90 from soil and to eliminate it from milk are being studied. Of course, such measures may be only palliatives. It is the diplomat who holds the key to the solution of the base problem: cessation of nuclear explosions in the atmosphere (see below).

The growing use of fission-operated power plants requires a similar study because the very fission reactors which will become increasingly important sources of electric power in the years ahead also produce a great deal of radioactivity. Obviously, while the bomb would spread the radioactivity into the four winds, the fission plant may be able to keep it carefully sealed up and safely disposed of. But managing the safe storage of the long-lived radioactive by-products is by no means a simple problem. Here, too, the initiative of the Public

Health Service (supported by Federal legislation for control of radiation), and eventually of the World Health Organization, would be logical.

With this report CU hopes to stimulate wider interest and understanding on the part of the public, of public health agencies, and of commercial producers in the problems of radiation and in the control of its hazards.

Conclusion Based on Uncertainty

International activity in the fallout problem has produced four major developments since last summer including two significant reports made public by two international groups—one governmental, the other private.

The United Nations Scientific Committee on the Effects of Atomic Radiation issued an exhaustive evaluation of the known scientific facts. The importance of this report lay in its demonstration that scientists around the globe are in substantial agreement on existing and potential hazards.

The second of these reports was issued by the so-called Pugwash group sponsored by Ohio Industrialist Cyrus Eaton and named for its original meeting place—Mr. Eaton's summer retreat at Pugwash, Nova Scotia. The 1958 Pugwash meeting was the third in a series begun in response to an appeal for an international forum of scientists made in 1955 by Lord Russell, Albert Einstein, and other prominent scientists. The 1958 sessions were held in Austria under the good offices of many sponsors, including the Austrian Government. The attendance included not only scientists who have been critical of bomb testing but ones long associated with the scientific policymaking of their countries—such men as Sir George Thomson of England, Prof. D. V. Skobeltzyn of the U.S.S.R., and Prof. Eugene Wigner of Princeton University. The 1958 Pugwash statement included a paragraph which rather fairly summarizes the present prevailing opinion among scientists:

"* * * the bomb tests produce a definite hazard and will claim a significant number of victims in present and following generations. Though * * * the genetic damage appears to be relatively small compared with that produced by natural causes, the incidence of leukemia and bone cancer * * * may * * * add significantly to the natural incidence of these diseases. This conclusion depends upon the assumption (not shared by all authorities in the field) that these effects can be produced by even the smallest amount of radiation. This uncertainty calls for extensive study and * * * for a prudent acceptance of the most pessimistic assumption."

Meanwhile, the political pressures generated by public concern at least have been moving diplomatic machinery. A meeting of experts from the big powers in Geneva produced agreement last summer on the feasibility of a control and detection system which could enforce a possible agreement to end tests of nuclear weapons.

And at the time of this writing, diplomats are in Geneva trying, through a welter of bargaining points, to bring about an agreed cessation and a suitable control system. They have wrangled endlessly, and yet there has been about the proceedings an air of hope. The end of tests would, of course, stop the production of bomb fallout in peacetime. Far more importantly, one could view the end of tests, if it came, as a first step toward the prevention of nuclear war.

Statement of Responsibilities and Activities of the Food and Drug Administration With Respect to Radioactivity

The Food and Drug Administration administers the Federal Food, Drug, and Cosmetic Act. This is the Federal law that regulates foods, drugs, therapeutic devices, and cosmetics that move in interstate commerce. Among other things this law places upon the Food and Drug Administration the responsibility for regulating interstate movement of any of the named commodities which contain or have been affected by radioactivity.

The responsibilities and the statutory authority for them (all under the Food, Drug, and Cosmetic Act) are:

1. Establish by approval of new drug applications safe conditions for use of radioisotopes in medicine—section 505.

2. Establish safe tolerances for unavoidable radioactivity in food—section 406(a).

3. Issue regulations stating safe conditions for the intentional or foreseeable use of radioisotopes in food or the protection of food with radiation—section 409(c).

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4. Determine on a continuing basis the radioactivity in foods, drugs, therapeutic devices, and cosmetics and evaluate the results to see whether these commodities are legal in interstate commerce—section 702(a) and the following adulteration and misbranding sections: 402, 501, 502, 601.

This involves—

(*a*) Collecting samples in connection with the administration of the Food, Drug, and Cosmetic Act, for examination for compliance with the Federal law with respect to radioactivity and other factors—sections 702(a), 703, 704(a).

(*b*) Initiating legal actions against foods, drugs, devices, or cosmetics that are adulterated or misbranded because of the use or presence of radioactivity—sections 302, 303, 304.

(*c*) Regulating imported foods, drugs, and cosmetics—section 801.

(*d*) Maintaining the necessary laboratory services and developing and participating in development of the necessary analytical methods to enable adequate enforcement of the Federal and State food and drug laws with respect to radioactivity as well as other factors influencing the legality of the commodities—section 702(a) and section 707.

(*e*) Conducting other studies as necessary to determine that the decontamination and salvage of seized foods, drugs, and cosmetics is in accordance with orders of the Federal courts—section 304(d).

5. Maintain Federal-State relations in the food, drug, and cosmetic field and see that Federal and cooperating State and local enforcement personnel are adequately trained to perform their enforcement functions, including investigations with respect to radioactivity.

These responsibilities are fulfilled in the following respects:

1. Before a manufacturer may market a new drug in interstate commerce, he must submit to the Food and Drug Administration evidence showing that it is safe under the intended conditions of use. The Administration's scientists evaluate the safety data and the Administration allows the application to become effective and the drug to be marketed if safety is established. Under this procedure a number of radioactive drugs have been cleared for safety and are now on the market.

2. Where radioactivity cannot be avoided in food by good manufacturing practice, the Food and Drug Administration has the authority to issue regulations limiting the quantity of radioactivity that may be present to the extent necessary to protect the public health. In establishing these tolerances, the Administration will utilize all available scientific data bearing on the safety of the radioactivity in food, whether they have been developed in its own laboratories, other Government laboratories, or nongovernmental institutions. The so-called maximum permissible concentrations which have been suggested by a national committee and an international commission are not legal standards with respect to allowable radioactivity in food. They have been recommended primarily for other purposes.

The Food, Drug, and Cosmetic Act is the Federal statute under which legal standards may be established for radioactivity in food. The States generally recognize and make use of other Federal tolerances established under the Food, Drug, and Cosmetic Act, and would no doubt utilize tolerances for radioactivity in the same way.

3. If a person wishes to add radioisotopes to food or to treat food with ionizing radiation, he must submit a food additives petition to the Food and Drug Administration which describes the proposed use of radioactivity and gives scientific data to establish its safety. If the Administration's scientists agree that the proposal is safe, and certain other conditions are met, the Commissioner of Food and Drugs may issue a regulation stating the conditions under which the radioactivity may be employed. Here again the research findings and judgment of responsible scientists in the field, wherever they may be located, will be drawn upon in evaluating food additive petitions. Also the States generally recognize and utilize Federal regulations of this type.

4. Through inspection of manufacturing establishments that produce foods, drugs, therapeutic devices, and cosmetics and through sampling and examination of shipments of these commodities, the field offices of the Food and Drug Administration (17 today with an 18th being added) are determining the legality of interstate shipments of these commodities. This jurisdiction extends to all food items including all items of drink; the examinations cover the various conditions which may influence the legality of the commodities, including the presence of radioactive isotopes.

In connection with these activities, the Administration utilizes all available data from other sources. For example, the Atomic Energy Commission, and one of its contractors, have performed laboratory analyses on some food samples collected by the Administration; data obtained by the Public Health Service on the radioactivity of milk have been made available to the Administration; and data being accumulated by the Public Helth Service on radioctive isotopes present in air and water will be utilized in evaluating the safety of food and drug manufacturing processes where contaminated air and water are present.

During recent years the Food and Drug Administration has conducted surveys to determine to what extent the American dietary is becoming contaminated with radioactivity. This radioactivity has arisen chiefly from tropospheric fallout consisting of manmade fission products originating from weapons testing. These surveys have revealed the total radioactivity in food. In general, determination of individual radioisootopes, such as strontium-89 and 90 has been deferred.

To establish a reference baseline, the Food and Drug Administration requested the assistance of consumers, trade organizations, and food manufacturers in the collection of samples of food prepared before the first atomic bomb was exploded in 1945. The response to this request was most gratifying. Over a thousand samples were obtained representing basic food commodities prepared in various sections of the United States and in certain parts of the world outside this country. These were analyzed for radioactivity and give data against which to judge food prepared since 1945.

Samples of the same basic foods produced since 1945 were collected from the same geographic areas and examined for radioactivity. The results on about 1,000 samples representing food produced before the middle of 1957 showed—

1. No measurable contamination of fruits and vegetables most of which were canned.
2. Some contamination of sea foods, such as fish and shellfish.
3. Significant trends to increased contamination in dairy products, such as powdered milk and cheese.
4. A marked contamination of tea.

An additional 1,500 samples of food produced from 1957 to April 1959 showed continuing evidence of radioactive contamination in tea and in fish, chiefly in several shipments of tuna. Furthermore, although it has not been necessary to take legal actions against any foods because of radioactivity, in the period since 1957 (as compared with the pre-1945 samples) there has been a marked increase in radioactivity in fresh vegetables, animal forage (chiefly alfalfa and, to a lesser extent, corn ensilage), and wheat.

These radioactivity testing activities will be increased in the future. If the need for control measures arises, the usual procedures under the Food, Drug, and Cosmetic Act are available for use.

When shipments are found to be significantly adulterated or misbranded, the facts are referred to the Department of Justice for initiation of appropriate legal actions through the Federal courts. These may be injunction proceedings to restrain further violations, criminal actions because of past actions, or seizures to remove illegal merchandise from the channels of interstate commerce.

Additionally, every shipment of foods, drugs, or cosmetics offered for importation into the United States is subject to examination by the Administration. Examinations are made on a spot-check basis to keep abreast of developments and where necessary these are increased to give up to 100 percent coverage of a given commodity. For example, for a considerable period of time after the hydrogen bomb tests in the Pacific, FDA inspectors and chemists with the assistance of Atomic Energy Commission and the Department of Defense, examined every shipment of tuna entering the United States from Pacific waters for radioactivity.

The Administration is responsible for maintaining the necessary Federal laboratory services to permit adequate handling of this problem as well as other problems in the food and drug field and for developing and participating in the development of the necessary analytical methods to enable adequate enforcement of the Federal and State food and drug laws with respect to contamination of food.

Federal analysts and inspectors are given formal training in the various aspects of food and drug regulation. During the past 50 years the Administration has developed close cooperative relationships with enforcement officials in the States and larger cities and it helps them train their inspectors and chemists, furnishes

quantities of resource material on food and drug law enforcement, and develops and distributes important inspection and analytical methods. Upon occasion, cooperating State and local enforcement officials and industry scientists receive intensive instruction in food and drug procedures in the Washington laboratories and in courses presented in the field.

Analytical methods suitable for use in determining various contaminants are developed in the Administration's Washington laboratories and in each of the field laboratories. These methods are studied collaboratively in laboratories of the Federal Government and of the States, State experiment stations, and universities through the "referees" established by the Association of Official Agricultural Chemists. Such cooperative testing, which has been proved to be sound over a period of many years, will continue to be employed in connection with radioactivity testing.

Additionally, the Administration is responsible for supervising and approving the decontamination and salvage of seized foods and drugs in accordance with court orders. These orders, as a general rule, provide that reconditioning must be by procedures acceptable to the Administration and that the reconditioned merchandise must be in harmony with the Federal and State laws before it may be released for sale.

It is apparent from the above discussion that the Administration is responsible for maintaining research to develop methodology for identifying and evaluating radioactivity in commodities subject to its jurisdiction to develop techniques for decontaminating affected foods and drugs, and to develop adequate regulations defining permissible levels of radioactivity in commodities subject to its jurisdiction.

By way of summary, the Food and Drug Administration has—

1. Jurisdiction over all foods, drugs, devices, and cosmetics that move from one State to another, and responsibility for seeing that they are safe with respect to radioactivity and other contaminating materials.
2. The statutory responsibility for establishing, where necessary, tolerances for poisonous materials, including radioactivity in foods.
3. The statutory responsibility for evaluating the intentional addition of radioactivity to food and drugs and issuing permissive regulations (or in case of drugs of allowing new drug applications to become effective) if such added radioactivity is to be tolerated.
4. The statutory responsibility of evaluating and approving the safety of any irradiation process for use on food.

Dr. WEBER. The next section of my longer summary deals with research, principally the fact that additional research in this field is necessary. In doing so an attempt is necessary to correlate information on exposure with actual retention in the human system.

It is agreed that the problem is complex but not insoluble. Radio nuclides other than strontium 90 might now be given fuller attention.

Now, with regard to program expansion for the control of hazards in this particular summary, I have said that this should be a comprehensive one for radiation control of all sources, of which that derived from fallout is one. It is suggested that existing Public Health resources in the local, State, Federal complex be utilized much in the same way as they now are for other public health problems. This would prove particularly valuable in studying the problem in which epidemiological techniques are necessary.

That concludes, Mr. Chairman, my formal statement.

Representative HOLIFIELD. Mr. Weber, would it be fair to say that under present law it is the responsibility of the Surgeon General to protect the public health against radiation hazard in the environment?

Dr. WEBER. Yes, sir; I would say that is true.

Representative HOLIFIELD. Of course, there are specific duties that are imposed on the AEC in their plant operations which they have to

handle under the law, but I am speaking now of the general environment of the population.

Dr. WEBER. I believe it breaks down into two areas, in replying to your question, Mr. Chairman. No. 1, the AEC has specific authority in connection with certain special activities in the nuclear field. There is no question about that whatsoever. No. 2, however, the Public Health Service, under, I believe, section 301 of its act, Public Law 410—and I can provide the exact quotes for the record—enables the Surgeon General to take responsibility for all health hazards. In other words, it is not restricted.

As far as the term "radiation" is concerned, that is not to my knowledge specifically mentioned by name in the Public Health Service law, but it is implied. As a matter of fact, there are a number of other disease conditions also in which we have active programs that are not specifically named in the law.

Representative HOLIFIELD. The law was passed, of course, establishing the Public Health Service and prescribing its duties, I think long before we had the atomic program. If the Surgeon General does not have such authority, what if any measures should be taken to give him such authority? Do you think it is necessary for him to remove this implication of his responsibility and define it in the statute?

Dr. WEBER. That is difficult for me to say, Mr. Chairman, since the reply to that would depend on what the feeling is in higher administrative circles within the executive branch. Not being an attorney, and just a physician, I could have my own personal opinion, but it certainly would not be an expert opinion insofar as political science or jurisprudence is concerned. So if you would like that opinion, I would be very happy to state it without that reflecting as Public Health Service policy.

Representative HOLIFIELD. You may state it.

Dr. WEBER. I feel that specific mention in terms of categories of programs do practically strengthen health operations in the particular field in which one is engaged. I believe the history of public health legislation probably bears that out. To take one example, the successive establishment of the various National Institutes of Health devoted to certain disease categories in order to give proper emphasis to them. Again, not being an expert in jurisprudence, I don't know whether that is absolutely necessary to the exercise of efforts in pursuing, let us say, any national health program when a program is considered to be national in scope.

As I understand, there is legislation pending before the Congress in the form of the Hill-Roberts proposals, which presumably would go into this matter, but on which no hearings have been announced.

I might also add, from what I can learn, and here again I express my own opinion, not necessarily that of the Department, that there is a feeling within the executive branch that it is possible to clarify the respective roles of the various special agencies already within the executive powers. The executive, as you know, via the agency of the Bureau of the Budget, has already undertaken a study in that regard. Of course, we will have to await the results.

Representative HOLIFIELD. Do you have any comment to make on that, Dr. Dunham?

Dr. DUNHAM. I think I am in somewhat the same situation that Dr. Weber is in at this point. I would like to say that I do feel it is very important, regardless of what the legalistic aspects of things are, that the program within the Public Health Service on radiological health be strengthened. It is very important.

Representative HOLIFIELD. Dr. Weber, you have had quite a bit of experience in trying to obtain appropriations for the Public Health Service work. Do you think that if additional responsibility were given in this field that you could get from Congress appropriations with the equal facility, let us say, that the AEC does?

Dr. WEBER. Sir, that is a difficult question.

Representative HOLIFIELD. Maybe I should ask myself that question. This is the one thing that has worried me about all of this discussion of shifting duties from the AEC to the Public Health. I know how hard it is to get appropriations for civilian agencies and comparatively speaking I found it is much easier, if it is for defense.

Dr. WEBER. Yes, sir.

Representative VAN ZANDT. At this point, let me ask Dr. Dunham this question. Assuming that Congress were to amend existing law and shift the responsibility to the Public Health Service, would it be necessary for the Atomic Energy Commission to continue its present effort?

Dr. DUNHAM. Are you speaking in terms of the activities perhaps of the Division of Biology and Medicine of the AEC?

Representative VAN ZANDT. That is right.

Dr. DUNHAM. I don't think it would relieve us of any responsibility, because we would still have our responsibility within the Commission itself.

Representative VAN ZANDT. In other words, then, we would have set up a similar organization within the Public Health and the AEC would retain its organization.

Dr. DUNHAM. I have not had a chance to study these proposed acts, but there is always conceivably a situation in which you would not be having duplicate organizations, no, but in which one organization might have a little more overall responsibility than another.

Representative VAN ZANDT. Probably not of the same magnitude but it would be somewhat similar.

Dr. DUNHAM. Yes.

Representative HOLIFIELD. Mr. Ramey.

Mr. RAMEY. In preparing for these hearings one thing I think that the staff observed was that coordination of the collection of data on fallout and the sort of direction of a program aimed at fallout information was still pretty loose. We have testimony that the Commission is going to put out quarterly reports, and so on. Do you think that is enough, or do you need somebody that is your Fallout Director, or something like that? I think a newspaper reporter who happens to be in the room pointed out that the Commission only has two full-time employees who were concerned with fallout as such.

(See article by John W. Finney, New York Times, Mar. 29, 1959, in vol. 3, app. K.)

Dr. DUNHAM. This is true, Mr. Ramey. Unfortunately, fallout is sort of an unlovely subject. I am sure that if it was not for the fact that there are some very special payoffs from personal interest and things that Mr. Holland and Mr. Hollister probably would not be willing to devote 100 percent of their efforts to this subject. We do want to strengthen the program. There is not any question about it. We do want as much leadership as we can get. I was interested in talking to Dr. Neuman the other evening in which he was pointing out that perhaps we have gained something from the diffuseness of the program. By that I mean that many agencies are involved, many, many people like Dr. Kulp and other individual university scientists, who have not been so much directed as to what they could or could not do. I think we would probably have missed a lot of tricks if this thing had been very tightly knit at the very beginning. At least this was the thought that Dr. Neuman put out, and I think it has much to commend it. That does not mean that there is not plenty of room for strengthening the program. I am furnishing for the record an organization chart of the present makeup of the Division of Biology and Medicine.

U. S. ATOMIC ENERGY COMMISSION
SUMMARY
S — Scientific 32
OP — Other Professions 16
S&C - Secretarial and Clerical 31
Total 79
DIVISION OF BIOLOGY AND MEDICINE
S 3 * OP 2 S-C 4
Advisory Committee for Biology and Medicine
Program Coordination Branch
S - OP 4 S-C 4
Administrative Branch
S - OP 3 S-C 5
Assistant Director for Research
*
Assistant Director for Radiation Protection
S 1 OP - S-C 1
Training & Education Unit
S - OP 2 S-C 1
Biology Branch
S OP S-C
6 1 2
Medical Research Branch
S OP S-C
6 - 3
Environmental Sciences Branch
S O S-C
4 - 2
Health and Safety Laboratory
Radiation Instruments Branch
S OP S-C
3 - 2
Civil Effects & Liaison Branch
S OP S-C
2 3 2
Health Protection Branch
S OP S-C
4 1 3
Radiation Effects of Weapons Branch
S OP S-C
3 - 2
MAY 1, 1960

Representative HOLIFIELD. Admiral Parker, will you please give us a résumé of the Defense Department activities?

STATEMENT OF REAR ADM. E. N. PARKER,[1] CHIEF, ARMED FORCES SPECIAL WEAPONS PROJECT

Admiral PARKER. Yes, sir, Mr. Chairman. I have given you this complete statement plus the tables which gave a breakdown of the money that the Department of Defense has been expending and plans to expend in this program, plus the list of activities, sir.

Representative HOLIFIELD. Can you summarize that, please? We will accept it in its entirety for the record.

(The statements referred to follow:)

DEPARTMENT OF DEFENSE

FALLOUT AND RADIOLOGICAL HEALTH ACTIVITIES

The results of a recent survey within the Department of Defense forms the basis of this report to the Special Subcommittee on Radiation of the Joint Committee on Atomic Energy for the "Public Hearings on Fallout From Nuclear Weapons." The Department of Defense fallout and radiological health activities are in three broad categories:

(1) Fallout and the effects of ionizing radiations as obtained by direct participation in the nuclear weapons tests.

(2) The worldwide fallout program consists of the AFSWP high altitude sampling program; atmospheric radiation studies by the Naval Research Laboratory; and the U.S. Air Force program of fallout and worldwide dissemination of nuclear debris at the Air Force Cambridge Research Center. The principal results of the AFSWP–HASP program have been made available at these hearings in the previous testimony.

(3) The basic laboratory research and development activities funded by the Department of Defense.

The levels of funding by the Department of Defense for each of the categories are contained in the enclosed tables. Table 1 contains the funding for fallout and effects of ionizing radiation as related to participation in nuclear weapons tests. The total for the 3 fiscal years 1956, 1957, and 1958 is $9.1 million or an average of about $3 million per year. The funding for fiscal year 1959 reflects costs for participation in nuclear tests during the first quarter of the fiscal year and studies of previous test results during the remainder of the fiscal year. The fiscal 1960 funds are estimates of the requirements for continued analysis of past test results and some planning funds to maintain a capability to resume testing should this be directed. Typical nuclear weapons test activities have been:

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[1] Present Chief of the Defense Atomic Support Agency, graduated from the U.S. Naval Academy and was commissioned an ensign on June 4, 1925. Attained the rank of rear admiral on Sept. 1, 1952.

During the period between 1925 and 1940 he served in assignments at sea as well as ashore including instruction in general line, ordnance, and gunnery. In World War II, he was awarded three Navy Crosses and the Silver Star for distinguished service and extraordinary achievement while serving with the Pacific Fleet.

In February 1943, he was assigned duty in the Bureau of Ordnance, Navy Department, Washington, D.C., as Head of Ship Characteristics and Fleet Requirement, and Head of the Planning Engineering Section. As Assistant Director of the Research Development Division from February to July 1945, he received a Letter of Commendation with authorization to wear the Commendation Ribbon.

In April 1947, after further service in the Pacific from 1945 to 1947, he joined the staff of Commander Marianas (at Guam) serving first as Logistics Officer and later as Chief of Staff. He was assigned in September 1948 to the Office of the Chief of Naval Operations, Navy Department, and served there until August 1950, when he assumed command of the cruiser U.S.S. *Newport News*. In July 1951, he joined the staff of Commander 6th Fleet, as Chief of Staff. In September 1952, he became Deputy Chief of the Armed Forces Special Weapons Project, Washington, D.C., and served there until December 1954, after which he assumed command of Cruiser Division 6. On May 28, 1956, he reported as Special Assistant to the Deputy Chief of Naval Operations (Plans and Policy), Navy Department, and remained until June 1957, when he was ordered to duty as Chief of the then Armed Forces Special Weapons Project, Washington, D.C. (changed to Defense Atomic Support Agency on May 6, 1959).

(*a*) Measurement and the effects of prompt nuclear radiations from the exploding bombs.

(*b*) Onsite sampling and dimensions of the nuclear cloud,

(*c*) Delineation of the fallout patterns,

(*d*) Radiation shielding afforded by military equipment, various types of soil and structures. Shielding afforded by structures is applicable to the fallout shelters of the Department of Defense and the Civil Defense.

(*e*) Decontamination in the fallout area.

The preparation of preliminary plans and budget estimates of Department of Defense activities on nuclear weapons tests is the responsibility of the Armed Forces special weapons project (AFSWP). Table 2 contains the funding by the Department of Defense for the worldwide fallout programs. The funding for fiscal years 1956–58 was $2.4 million and this reflects the initiation of the large programs in the latter portion of the period, $3.2 million in fiscal 1959 reflects the peak of the effort and the estimated fiscal 1960 funding is about $2.1 million for worldwide fallout programs.

Table 3 contians the funding by the Department of Defense in laboratory research and development programs in the general subject of fallout and effects of ionizing radiation. The funds are provided by one of the services of AFSWP. The funding by the Department of Defense for such activities for fiscal years 1956–58 was $11.6 million or an average of about $3.9 million per year. The effort in the laboratory research and development program in fiscal 1959 has been $8.8 million and the estimates for fiscal 1960 total are about $11.8 million. Table 4 contains the individual projects and illustrates the types of activities pursued in fiscal 1959 for laboratory research and development. Table 5 lists the agencies which have conducted activities in this field for Department of Defense.

In summary, the average total level of funding per year during fiscal 1956–58 was $7.7 million and the total for fiscal 1959 was $12.8 million. It is estimated that the funding for fallout and the effects of ionization radiation for fiscal 1960 will be about $17.3 million. Finally, the assignment of exact costs to projects is a matter of some judgment. The costs, as given here, were directly associated with the specific projects or tasks and the actual total costs may have been somewhat higher.

The Department of Defense has a broad and continuing interest in the problems associated with nuclear weapons and especially in the subject of these hearings on fallout and its effects.

TABLE 1.—*Fallout and effects of ionizing radiation (direct participation in nuclear weapons tests)*

[In millions]

Fiscal years 1956–58	$9.1
Fiscal year 1959	.8
Fiscal year 1960	[1] 3.4

[1] Costs for analysis of past tests and some planning funds required to maintain a capability to resume testing should this be directed.

TABLE 2.—*Worldwide fallout program*

[In millions]

Fiscal years 1956–58	$2.4
Fiscal year 1959	3.2
Fiscal year 1960	2.1

Projects during fiscal year 1959

[Dollar amounts in thousands]

Title	Directing agency	Funded by	Amount
1. High altitude sampling program	AFSWP	AFSWP	$2,721
2. Atmospheric radiation studies	NRL	USN	99
3. Fallout and worldwide dissemination of nuclear bomb debris	AFCRC	USAF	400
Total			3,820

TABLE 3.—*Laboratory research and development on fallout and ionizing radiation*

[In millions]

Fiscal years 1956–58	$11.6
Fiscal year 1959	8.8
Fiscal year 1960	11.8

TABLE 4.—*Laboratory research and development programs in fallout and effects of ionizing radiation, fiscal 1959*

Program area with project title	Directing agency	Funded by	Amount
A. BIOMEDICAL			
1. Effects of Gamma, Gamma-Neutrons, Neutrons on Cells, Organs, Systems, Etc.:			
Effects of Radiation on Mammalian Cells	AFIP	AFSWP	$4,000
Ionization Dose Received by Tissues at Various Depths	NNMC	AFSWP	50,000
Instrumentation Requirements for Determination of Biological Injury.	NRDL	AFSWP	70,000
Study of Post-Irradiation Syndrome in Humans.	AFSWP	AFSWP	22,000
Effects of Radiation on Certain Biological Functions in Man.	AFSWP	AFSWP	26,000
Biological Effects of Radiation	NRDL	USN	483,000
Effects of Nuclear Radiation on Biological Specimens.	AFSAM	USAF	750,000
Evaluation of Health Hazards from a Nuclear Radiation Environment.	WADC	USAF	45,000
Biomedical Effects on Personnel Resulting from Handling, Delivering, and Storing Nuclear Materials.	AFSWC	USAF	45,000
Biological and Medical Aspects of Ionized Radiation.	Surgeon General	USA	600,000
Studies on Effects of Radiation Injury	do	USA	15,000
2. Fallout studies:			
Deposition and Removal of Radioisotopes in in the Body	AFSWP	AFSWP	90,000
Effects of Sr^{90} Administered During Growth Period of the Dog	AFSWP	AFSWP	60,000
Whole Body Counting and Laboratory Studies	WRAIR	AFSWP	58,000
3. Treatment of radiation injury:			
Therapy of Radiation Injury with Spleen Extracts	NNMC	AFSWP	46,000
Clinical Treatment of Radiation Injury	AFSWP	AFSWP	108,000
B. PHYSICAL			
1. Initial gamma and neutron radiation:			
Free Field Nuclear Effects for Nuclear Bursts at Sea.	NRDL	USN	408,000
Hazards Resulting from a Nuclear Reactor Catastrophe.	AFSWC	USAF	395,000
2. Fallout:			
A Study of the Plutonium Contamination in the Local and Fallout Area.	AFSWC	USAF	150,000
Theoretical Fallout Studies for the Field Army.	ASC	USA	150,000
3. Instrumentation:			
Development of Techniques and Instrumentation.	NRDL	USN	408,000
Radiac Instrumentation	NRDL	USN	422,000
Development of a Gamma Rate Meter	AFSWC	USAF	30,000
Development of Tactical Gamma Neutron Dosimeter.	ACC	USA	150,000
4. Induced Activity: Neutron-Induced Activity in Soils.	NRDL	USN	210,000
5. Shielding:			
Shielding Studies	AFSWP	AFSWP	130,000
Fundamental Studies of Gamma and Neutron Shielding Properties of Shelters.	BuDocks N.C.E.R. & E.L.	USN	39,000
Modification of Free Field by Ships	NRDL	USN	824,000
Hazard Evaluations and Countermeasures	NRDL	USN	381,000
Evaluation of Armor Materials With Respect to Shielding Against Ionizing Radiation.	OMRO	USA	248,000
Evaluation of Shielding Materials	BRL	USA	70,000
Radiation Protection for Combat Vehicles	Ordnance Corps	USA	190,000
Protection Against Radiological Hazard	ACC	USA	300,000
Determination of Shielding Characteristics	ERDL	USA	111,000
Study of Shielding Characteristics of Field Fortifications.	ERDL	USA	10,000
Study of Expedient Shielding Materials	ERDL	USA	5,000
Shielding of Engineer Heavy Equipment	ERDL	USA	20,000

TABLE 4.—*Laboratory research and development programs in fallout and effects of ionizing radiation, fiscal 1959*—Continued

Program area with project title	Directing agency	Funded by	Amount
6. Decontamination:			
Roof Washdown Systems	BuDocks, N.C.E.R. & E.L.	USN	38,000
Development of Radiological Decontamination Methods for Cold Weather Regions.	BuDocks, N.C.E.R. & E.L.	USN	38,000
Decontamination of Water Supplies	ERDL	USA	20,000
Decontamination of Structures and Equipment.	ERDL	USA	50,000
Development of Remote Controlled Decontamination Equipment.	ERDL	USA	10,000
7. Miscellaneous:			
Radiation Preservation of Food	QMC	USA	1,100,000
Training Relative to Techniques of Handling Nuclear Materials.	Ordnance Corps	USA	54,000
Effects of Radiological Contaminated Areas on Personnel and Equipment.	ACC	USA	100,000
Development of Equipment, Procedures, and Techniques Relative to Radiological Hazards of Nuclear Warfare.	ACC	USA	60,000
Effect of Radiological Contamination on Combat Operations.	ACC	USA	15,000
Technical Evaluation of Atomic Warfare and Radiological Warfare Information.	ACC	USA	45,000
Prototype Nuclear Power Plant Environmental Monitoring Program.	ERDL	USA	11,000
Effects of Ionizing Radiation on Equipment	ERDL	USA	145,000

TABLE 5.—*Identification of agency abbreviations*

ACC	Army Chemical Corps.
AFCRC	Air Force Cambridge Research Center.
AFIP	Armed Forces Institute of Pathology.
AFSAM	Air Force School of Aviation Medicine.
AFSWC	Air Force Special Weapons Center.
AFSWP	Armed Forces Special Weapons Project.
ASC	Army Signal Corps.
ASRDL [1]	Army Signal Research and Development Laboratory.
BRL	Ballistic Research Laboratories.
BuAer	Bureau of Aeronautics, Department of Navy.
BuDocks	Bureau of Docks, Department of Navy.
BuShips	Bureau of Ships, Department of Navy.
CRL	Chemical and Radiological Laboratories, U.S. Army.
CWL	Chemical Warfare Laboratory, U.S. Army.
ERDL	Engineer Research and Development Laboratory, U.S. Army.
FC/AFSWP	Field Command, Armed Forces Special Weapons Project.
NBS	National Bureau of Standards.
NCER & EL	Naval Civil Engineering Research and Evaluation Laboratory.
NML	Material Laboratory, U.S. Navy.
NMRI	Naval Medical Research Institute.
NNMC	National Naval Medical Center.
NOL	Navy Ordnance Laboratory.
NRDL	Naval Radiological Defense Laboratory.
NRL	Naval Research Laboratory
NYOO	New York Operations Office, Atomic Energy Commission.
OCE	Office of the Chief of Engineers, U.S. Army.
OMRO	Ordnance Materials Research Office, U.S. Army.
ONR	Office of Naval Research.
QMC	Office of the Quartermaster General, U.S. Army.
Rand	Rand Corp.
Sandia	Sandia Corp.
SIO	Scripps Institution of Oceanography.
Surg. Gen	Surgeon General, U.S. Army.
WADC	Wright Air Development Center, U.S. Air Force.
WRAIR	Walter Reed Army Institute of Research.

[1] Formerly Signal Corps Engineering Laboratories (SCEL).

THE DEPARTMENT OF DEFENSE FALLOUT PROGRAM AND NUCLEAR TEST OPERATIONS

1. Since the first light of the atomic bomb over Alamogordo, N. Mex., the United States has set off well over 100 nuclear explosions. Some of these have been actual weapons, but most were nuclear devices detonated to advance scientific and engineering knowledge as applied to such weapons. They have been detonated in widely varying environments, designed to facilitate the collection of maximum effects measurements and operational experience. What is presently known about fallout has been determined from studies of these detonations and has been expanded to theory and laboratory tests as rapidly as possible. It was during the test series, Greenhouse, Ivy, Castle, and Redwing, conducted during the 1951–56 period, that the greatest efforts were made to document local fallout from large yield weapons. Operation Plumbbob in 1957 was restricted to measuring fallout from small weapons fired on towers. In the latest test series (Operation Hardtack in 1958) the fallout effort can be divided into three areas: (1) the unique effects of underwater bursts, specifically the base surge; (2) fallout from low yield weapons; and (3) aircraft and rocket sampling of the radioactive cloud to further the knowledge of the contribution of cloud fission products to worldwide fallout.

2. The early atomic bomb bursts did not deposit fallout locally, since air bursts result in dispersion of all radioactive debris in the atmosphere in extremely small-sized particles. This was the case for the two weapons detonated over Japan, and thus fallout was not a factor in the loss of life that resulted. Early U.S. testing was done mostly on high towers which kept the local fallout to a minimum. Most recently surface tests of large megaton devices were carried out at the Eniwetok Proving Ground. A significant portion of the radioactive debris from these larger yields was deposited in the Pacific, minimizing the worldwide contamination.

3. The large Department of Defense effort in the field of nuclear weapons effects has been divided between the three services and the Armed Forces Special Weapons Project (AFSWP) has assisted and coordinated their efforts in the field of weapons testing as outlined above. One of the fundamental problems always present is the accurate measurement of nuclear radiation, and all services, as well as agencies outside the Department of Defense, have been working steadily to improve the present capability in the field of instrumentation. We have a very active committee doing an effective job in keeping the participating agencies informed as to what each is doing and coordinating their overall effort. Each of the services is working toward the continuing improvement of personnel dosimetry and survey instrumentation. The Army is especially concerned in the effects of weapons on various terrain, on waterways, and structures for obvious planning purposes. They are developing structural specifications, quick reentry techniques, and atomic demolition capabilities. Their work on the use of gamma radiation in the preservation of foodstuffs has received considerable publicity and could be of far-reaching value.

4. The Navy has been making worldwide fallout studies, some of which are supported by other agencies. They are making detailed studies of the hazard of the radioactive base surge from underwater depth charges. They are also developing shipboard and shore station fallout countermeasures and ship recovery techniques that are intended to improve their defensive stature. Much work has been done on the prediction of ship damage and ship shielding. (The Army has a parallel interest insofar as tanks are concerned.) The effects of ionizing radiations on materials has not been overlooked and, as a result, there is now available a considerable quantity of information in this area to support most present day requirements.

5. The various environments a weapon might be in when it explodes have a great deal to do with the radiological effects of the fallout. The depth underground or underwater and the altitude of air bursts are critical when within certain limits. The yield of the burst also has a complicating effect. It can be said that tests have been directed towards furthering our knowledge as to how effects vary with the weapon environment and yield. From our data effects predictions and models are made.

6. The military services have developed what are called fallout models to improve fallout prediction techniques. These are mathematical procedures used to compute expected fallout surface dose rate contours for nuclear weapons of any size fired in any environment. The models are dependent on accurate winds and wind predictions at all altitudes. The weather bureau has been doing much

work in this field and has made considerable progress towards improving the weather inputs. As a result, we can now predict areas of probable fallout with increased assurance.

Admiral PARKER. Very quickly, we divide our program into three parts. Fallout as we get from direct participation in nuclear weapons tests. Then the worldwide fallout program. Most of it is the high altitude sampling program which has been described earlier in the hearings. Third, basic laboratory research and development activities which are funded by the Department of Defense.

We have table 1. I don't have some charts to put up. Table 1 shows that the money spent on direct participation on nuclear weapons tests is a function of the testing cycle in the period 1956–58 we had $9.1 million. Fiscal 1959, when we had a very small test program, the end of the last Hardtack series, we only had $800,000. For fiscal year 1960, we have $3.4 million planned for analysis of past tests, and some planning funds required to maintain a capability to resume testing should this be directed.

Representative HOLIFIELD. Is this appropriation you speak of here for the field of analyzing the tests and all the effects of the tests?

Admiral PARKER. Of fallout. Only those having to do with fallout.

Representative HOLIFIELD. Only those having to do with fallout?

Admiral PARKER. Yes, or the effects of ionizing radiation, sir. Not anything else.

Representative HOLIFIELD. You have quite a substantial program there.

Admiral PARKER. Yes, sir, we do. Then we have in the matter of obtaining information—direct information—worldwide fallout. This is mostly the high-altitude sampling program. We spent 1956–58 $2.4 million; 1959, that was the height of the high-altitude sampling program, $3.2 million. In fiscal year 1960, $2.1 million is planned. We took the year 1959 and broke it down to show that the $3.2 million in 1959, the great majority is in the high altitude sampling program, $2.7 million.

In the third category, which is our laboratory research and development of fallout and ionizing radiation, development of information, fiscal years 1956–68, $11.6 million. Fiscal year 1959, $8.8 million. In 1960, we plan $11.8 million.

Representative HOLIFIELD. Do you see what I mean, Mr. Weber, by the amount of appropriations the Defense Department gets for projects as compared to your civilian department?

Admiral PARKER. The rest of these tables, such as table 4, give greater detail on the laboratory programs, what we are doing, directing agency, who funds it, and the amount of money.

In table 5 we gave you a list. There are many abbreviations here. This is a list of the identification of the agency by these abbreviations.

So, in summary from 1956 through 1959, the Department of Defense has obligated, or has ready for obligation, nearly $36 million in this program, and for 1960 we are planning an additional $17.3 million for a little over $53 million.

Representative HOLIFIELD. Admiral, I want to say that I approve of every dollar that is being spent in this field by the Department of Defense, and anywhere we can get additional information on this

vital subject, in my opinion, we are justified in putting up money for it. I want at this time to again express my appreciation for the work that the Naval Radiological Laboratory at Hunters Point has done in this field, since the Bikini test, and also the Navy's kindness in allowing us to use Dr. Paul Tompkins as a consultant in previous hearings and this hearing.

When printed in the record, your program will be very impressive—when the people see that there is this large program going on, and the concern which is being expressed by the Defense Department on these effects of fallout.

Representative VAN ZANDT. Mr. Chairman, I think we should add there that this program, while not only supporting weapon development, also supports the peaceful uses of the atom.

Admiral PARKER. Yes, sir.

Representative VAN ZANDT. I notice much of your program has to do with the peaceful use of the atom.

Admiral PARKER. Yes, sir. I think everyone has agreed, sir, that this high-altitude sampling program we did has made a major contribution since 1957 in information on fallout.

Representative HOLIFIELD. Dr. Shelton's contribution of that material was very helpful.

Are there any questions of Admiral Parker or the other witnesses? If not, we will excuse you, and we thank you for your assistance to the committee in presenting testimony.

The Chair wishes at this time to thank all who have participated and thank the audience for their patience and their quietness during these hearings.

I would also make the following announcement. The record will be held open for such pertinent material as has been requested and such material that has been offered by people of competence who have submitted to us written papers on some of these subjects.

The meeting is adjourned.

(Thereupon, at 12:50 p.m., Friday, May 8, 1959, the hearings were concluded.)

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